THE NORWEGIAN

NORTH

POLAR EXPEDITION

1893—1896

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SCIENTIFIC RESULTS

EDITED BY

FRIDTJOF NANSEN

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THE NORWEGIAN

NORTH POLAR EXPEDITION 1893—1896

SCIENTIFIC RESULTS

VOLUME V.

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XIV.

ON THE BOTTOM DEPOSITS OF THE NORTH POLAR SEA

BY

O. B. BØGGILD.

WITH

APPENDIX I:

ANALYSES OF THE BOTTOM DEPOSITS

By O. Heidenreich and Charles J. J. Fox.

APPENDIX II:

THALAMOPHORA OF THE BOTTOM DEPOSITS AND THE MUD FROM THE ICE SURFACE

By Hans Kiær.

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I.

INTRODUCTION.

Having been entrusted with the mechanical and mineralogical examination of the bottom-samples of the *Fram Expedition*, I now publish the results of this examination, in the hope that a tolerably clear idea will thereby be obtained of the lithology of the bottom of the North Polar Sea, a sea which, on account both of its geographical situation, and of most of the factors that have to do with sedimentation, occupies a most exceptional position as compared with all other seas.

Among the innumerable important scientific results obtained by the *Fram Expedition* the proving of the existence of a large, deep, North Polar Basin is by no means the least. Formerly, we generally believed that the North Pole was surrounded by great tracts of land, and that the sea to the north of the Old World was of comparatively small extent, and particularly, of inconsiderable depth compared with the large oceans of the world.

It was formerly, hardly possible to form a clear conception of the appearance of the bottom-deposit of this ocean bed, but it was scarcely expected that it would be of so consistently fine a character as these bottom samples would seem to indicate. As long as nothing very definite was known about the remarkable drift which nearly always prevails in the Polar Sea, it was conceivable that a portion of the region at least, might be covered with icebergs; and even though it be conceded that their power of transporting gravel and stone is of a very disputable character, yet it is certain that with the lapse of long periods the bottom would obtain some coarse material from this source.

As the *Fram Expedition* has made clear that there is nearly always a drift over the Polar Sea from the Bering Strait to the Sea between Greenland and Spitzbergen, and that there is no land anywhere in the neighbourhood of the *Frams's* drift, from which icebergs could originate, it is now to be regarded as absolutely impossible that this sea bottom could be sprinkled with coarse material in that way.

All the ice found must therefore consist of sea ice — which cannot possibly contain any earthy matter — or to a much smaller extent, of coast or river-ice from Siberia and N. America. At these places the bottom as far as is known, chiefly consists of fine loose material such as sand and clay, and the conveyance of stony material from these districts must therefore also be extremely small.

Furthermore, other sources from which the sea bottom very largely derives coarse material such as the presence of solid rocks or moraines, are in these regions entirely missing, and the combined result of all these factors is therefore the quite surprising one that *there has not been found a single stone* in any one of the samples brought home by the expedition; the size of the largest particle observed, being little more than 2 mm. Most of the samples, as I shall subsequently relate at greater length, are even without any component larger than 1/2 mm. Whenever we have any knowledge of the bottom-deposits in other arctic regions, we find that they, compared with deposits from other parts of the Ocean, generally contain a greater abundance of stones, including every possible transition from boulder clay sediments — more especially in the region of the great sea-currents carrying ice-bergs — down to the finer sorts of clay that are found farther out at the bounderies of the temperate seas, where ice-bergs play a less important part. Now as this last condition was everywhere paramount in the field of the *Fram Expedition's* researches, it has occasioned the above-mentioned remarkable fineness of the particles in the samples, so that, mechanically considered, these samples correspond almost exactly with those from warmer seas.

On the other hand, there is another characteristic which the deposits under discussion have in common with other true arctic deposits, namely, the small amount of organic particles they contain. In other arctic regions this may be ascribed to two altogether different reasons; on the one hand

to the great quantity of deposit continually being formed, and on the other hand to the small number of organisms that can exist at the surface of a sea completely covered with ice for the greater part of the year, and which only for a short time in the summer presents a small, open area in which the microscopic organisms, as far as we can judge, are even then not found in nearly such large numbers as in the warmer seas. In the *Fram Expedition* deposits, only the latter of these two circumstances is of any importance, and yet the result is almost the same. The deposits before us contain exceedingly few remains of organic origin; even in the deepest places, farthest from land, their total amount scarcely ever exceeds 5 % of the total deposit, and in most places it is very considerably less.

Yet a third, very striking peculiarity of these deposits is the great uniformity of their mineralogical composition. So far as can be judged from the bottom-samples, they originate exclusively from quartziferous rocks, either from granites, gneisses, and other similar rocks, or from the sediments originating from them, such as sand and clay. There is here a complete absence of any volcanic region, such as for instance, in the North Atlantic, contributes such great mineralogical variety to the bottom-samples; and older basic rocks, such as gabbros, diorites, etc., seem, at any rate, to occupy a very subordinate place. The consequence of this again, is, that in the whole of the region here treated, it is extremely difficult, not to say impossible, to make out the distribution in detail, a circumstance which, more than any other, has produced the difficulty experienced in examining these deposits.

It will be apparent from the foregoing remarks that *several important circumstances conspire to give a particularly uniform character to the bottom-samples of the Fram Expedition.* Since there are neither stony ingredients, as is mostly the case elsewhere in the polar regions, nor great quantities of organisms such as are found in the temperate and warm seas, nor yet any great variation in the mineralogical composition, it might easily be imagined that there can only be comparatively slight differences between the various samples. A closer examination will show, however, that a number of properties can nevertheless be brought to light, which sufficiently characterise the various localities in their relation to one another. It is here my intention in the first place to give as complete a description as I possibly can

of these deposits, which, as already stated, occupy a peculiar position among the various marine bottom-deposits.

Among the works that I have employed in the preparation of the present treatise, I will here confine myself to a mention of the following: Report of the *Challenger Expedition* on Deep-Sea Deposits, by F. RENARD and Sir JOHN MURRAY, Report of the *Norwegian North Atlantic Expedition*, Part IX, Chemistry, by LUDVIG SCHMELCK, and Report of the *Danish Ingolf Expedition* on the Deposits of the Sea-Bottom, by O. B. BØGGILD and of the *Danish Expedition to East-Greenland* 1900 by the same author (Meddelelser om Grønland. Vol. 28).

All local information concerning the hydrography of the regions here concerned, is to be found in the very detailed treatise of NANSEN: The Oceanography of the North Polar Basin; Vol. III, No. 9 of this work.

II.

MECHANICAL COMPOSITION OF THE SAMPLES.

The material submitted to me for examination consists of samples from 16 different localities, most of which are situated along the north coast of Siberia, while the remainder of the samples have been taken from the bottom of the deep North Polar Basin, more or less distant from land. From some of the first-named localities, there are two or more samples, taken at different hours on the same day, and they frequently present very great differences, and give a very characteristic illustration of the rapidity with which the nature of the bottom may change in the neighbourhood of land, even in adjacent spots, according as the depth becomes greater or less. The distribution of the samples may be seen from the accompanying chart, Table I, in which the stations are numbered in chronological order, and to which constant reference is made in these pages. The samples will be designated by their numbers, and the addition of letters will indicate the various samples from the same locality.

The samples were preserved either in alcohol, or in a dry condition. We do not know at present whether this difference has any influence upon the washing-results. It is possible that by being dried and kept for some time, the clay may become rather more coherent than it originally was, so that a wrong idea of the mechanical nature of the samples may be obtained. As far as I am aware, nothing has yet been proved with regard to this circumstance; but when we consider how easily dried samples such as these, become soft in water, it would seem as if the degree of coherency attained by the clay in drying, can be only very small. Upon the whole, the mechanical

No. of Sample	Date	Lat.	E. Long.	Depth in Metres	Designation
1	1893. July 24, 10 p. m.	71° 17′	48° 22′	130	Shallow-water deposit
2	— Aug. 17, 12 p. m.	75 10	80 31	46	— —
3	— — 18, 9 a. m.	74 38	80 15	26	— —
4	— — ·, 12 noon & 2 p. m.	74 29	80 32	40	— —
5	— — ·, 10 p. m.	73 51	80 44	39	— —
6	West of the Kjellman Is.			?	?
7	Near Taimur Sound			?	?
8	1893. Sept. 13, 12 noon	74 55	116 40	20	Shallow-water deposit
9	— — ·, 8,30 p. m.	74 37	114 44	40	— —
10 a	— Oct. 4	78 42	135 40	1460	Grey deep-sea clay
10 b	— — ·	—	—	—	— — —
10 c	— — ·	—	—	—	— — —
11	— — 21	78 19	135 45	135	Shallow-water deposit
12	— — 26	78 31	136 10	100	— —
13 a	— May 1	80 46	131 18	3800	Grey deep-sea clay
13 b	— — ·	—	—	—	
14	— Aug. 6	81 8	127 32	3850	Grey deep-sea clay
15	1895. Jan. 23	83 24	102 14	3450	— — —
16	— July 23	84 32	72 20	3700	— — —

consistency of the clay is one of the most complicated questions met with in a mechanical examination of the deposits; but as it is of the highest importance, and very greatly influences the results obtained, it will be necessary subsequently to enter into the question more fully.

The accompanying table gives the geographical situation of the several stations, and the designation, general consistency, and colour of the samples.

As was already pointed out in the introduction, and as will also appear from this table, the bottom-samples in most respects are remarkable for their very slight variation. They can only be referred to two of the ordinary classes of marine bottom-deposits, namely, (1) the *Shallow-Water Deposits*, and (2) the *Grey Deep-Sea Clay;* and these two classes do not differ from one another in any one decided character. The boundary is here placed, as usual, at 200 m.; and here, as elsewhere, it is apparent that on the whole this division is fully justified. Where the depth is less than 200 m., the deposits consist

No. of Sample	Colour when dry	Consistency	Degree of Coherence	Remarks
1	Brownish grey	Very clayed sand	Slight	
2	Brownish grey	Fine, slightly sandy clay	Great	
3	Brownish grey	Clayey sand	None	
4	Brownish grey	— —	—	
5	Brownish grey	Slightly sandy clay	Great	
6	Brownish grey	Coarse sandy clay	Considerable	
7	Brownish grey	Very sandy clay	Rather slight	
8	Grey	Sandy clay	Great	
9	Grey	Very clayey sand	Very slight	
10 a	Brownish grey	Sandy clay	Great	Taken at one time; 7a formed the uppermost, 7b the middle, and 7c the nethermost layer.
10 b	Brownish grey	— —	—	
10 c	Greyish brown	— —	—	
11	Brownish grey	Slightly clayey sand	None	
12	— —	Sandy clay	Slight	
13 a	Greyish brown	Fine, slightly sandy clay	Great	Taken at one time; 10b formed the uppermost layer of the sample. Very little of the deposit obtained.
13 b		Clay		
14	Brownish	Fine, slightly sandy clay	Very great	
15	—	— — — —	— —	
16	Light brown	Very fine clay	— —	Very little of the deposit obtained.

as a rule of gravel or sand; and only where special conditions exist, such as the presence of more enclosed sea-basins, or where the coast consists mainly of argillaceous rocks, or in places where the rivers carry great quantities of clay, are they for the most part clayey, and therefore in no way different from the grey deep-sea clay.

The above-mentioned clay does not differ essentially, in the region under discussion, from the grey deep-sea clay found in other parts of the world. It is always of a rather fine, clayey consistency, and becomes finer and finer with increasing distance from the coast, and with greater depths, although it never becomes so pure in this region that it is not possible to distinctly feel the sandy particles in it, when rubbed between the fingers. Its colour, as a rule, is a tolerably pure grey near the coast, while farther out in deep water it acquires an increasingly brown tone, and in the deepest places is almost pure brown. Taken separately, however, some exceptions are apparent, of which

No. 10 must be specially noticed, its peculiarity being that it consists of several layers, of which the uppermost is of a greyish colour, while the lowest is a fairly decided brown. As the ocean-bed seems very generally to consist of several layers of different colours, it will be necessary to investigate this somewhat more fully, although the facts obtainable are not sufficiently numerous to permit of a clear understanding on this point.

The colour of each species of clay depends, as a rule, exclusively upon the colour of the clayey matter itself. By washing, it will generally be apparent that the finest substances composing the deposit have almost exactly the same shade of colour as the entire sediment, while the sandy components may have any other colour answering to their mineralogical composition, although, the sandgrains are often provided with a very thin outer coating, which makes their colour somewhat resemble that of the clay. This circumstance is of course particularly marked when the sand is made up mainly of colourless minerals, especially quartz. If, for instance, we take a brown clay, we shall see that the sand that is washed out of it, possesses a faint light-brown colour, while this colour is altogether wanting in the sand that may be washed out of the grey clay. It is easy to convince ourselves by chemical means that this colour is due to hydrated peroxide of iron, which is deposited in the form of a very thin coating round each separate grain of sand. It is the same with the clay itself. Upon treating it with hydrochloric acid, the brown colour completely disappears, and is replaced by the usual grey. Thus here, too, this colour is produced by the precipitation of hydrated peroxide of iron on the separate clay-particles. SCHMELCK has shown that the brown clay in the bottom-samples is, on the whole, considerably richer in peroxide than is the grey clay, for he has found that the proportion between peroxide and protoxide varies as a rule between 1 and 2 in the grey clay, while in the brown it is generally between 3 and 4.

We may suppose this change in the proportion between the oxides of iron to have occurred in two different ways. Either the transformation has taken place in the clay itself by the aid of the oxygen in the sea-water, or the water has deposited hydrated peroxide upon the clay by the aid of the iron compounds dissolved in the water itself. In the first case the chemical analysis should show that the total quantity of iron remained unchanged after

the transformation; in the second case, the amount of protoxide of iron should be unaltered, while the peroxide should have increased. On looking at the numerous iron analyses made by SCHMELCK, the first proves to be the case; the average amount of iron is about the same in the brown and the grey clays, when the percentage is calculated on what is left after the carbonate of lime has been removed.

There is, however, much to indicate that sea-water can deposit oxide of iron upon the bottom. As I have mentioned in the report on the *Ingolf Expedition*, it is a phenomenon of very general occurrence, that the upper surface of stones lying upon the sea-bottom becomes covered with a brownish coating of oxides of iron and manganese, while this is not the case, or only very slightly so, with the under surface. When the stone is of such a species of rock as, for instance, quartzite, which does not contain the smallest quantity of iron, this coating must necessarily be deposited by the sea-water itself. It is difficult, however, to imagine that a deposition such as this can take place upon the stones, without also at the same time taking place upon the sediment between them; and there thus appears to be good reason to assume that this fact also plays a part in the production of the brown colour of the clay, although it is perhaps not of quite so much importance as the chemical transformation of the constituents of the clay.

In any case, it seems necessary to assume that the transformation of the grey clay to brown can only take place in the very uppermost, exceedingly thin layer, that is in immediate contact with the sea-water; for otherwise all the mud upon the bottom could not but acquire the same brown colour, as it is not possible to imagine the existence of differences in the sea-water itself, that can cause the mud to be oxidised in one place, and not in another. The only possible explanation appears to be that the grey clay is deposited so much more rapidly than the brown, that the uppermost layer has not time to be changed before it is covered with new sediment. We cannot, of course, draw any positive conclusion from this, regarding the rapidity with which the sediment is deposited, as long as we are althogether ignorant as to the length of time required for the transformation; but on the other hand, this circumstance is very important in estimating the relative rapidity with which the various sea-bottom deposits are formed. Thus, where the conditions in the main are uniform, and where there is no great variation in the composition of the

sediments that are carried out into the sea, we may assume that the rapidity with which the deposition takes place is about proportional to the amount of brown in the colour of the sample. It is not impossible, however, that several different factors, such as temperature, currents, etc., may modify the conditions to some extent. The amount of lime it contains does not seem to influence the colour of the clay in any way, except in giving it a lighter shade.

With regard to the circumstance that in one part of the sea-bottom, deposits may be found in which the upper and lower layers are of different colours, it seems most natural to assume that it is due to changing rates of sedimentation. It is also possible to imagine other reasons; for it appears that on shaking a sample into a little water, the heavier parts are precipitated first as a grey layer, and then above this the lighter, considerably browner parts. We should imagine that this separation takes place when the sample is taken, were not the apparatus constructed in such a manner as to make this impossible; the bottom-deposits may possibly have been previously set in motion by some large animal or other. The results of this would be that the uppermost brown layer will be of a very fine, clayey consistency, while the lower will be sandy clay. This is the case with several of the deposits from the Indian Archipelago[1]; first there is a layer of brown clay of a pasty consistency, wich attains a thickness of up to 15 cm., and below this there is grey, green, or blue clay. This circumstance has not yet been very closely investigated, however, and may possibly be due to other causes.

In the North Atlantic nearest the coasts of Norway, there is according to SCHMELCK, grey clay, and beyond this brown clay. The latter, nearest to the grey clay's territory, forms as a rule only a thin layer above it, getting thinner with a nearer approach to land. Here the brown clay is expressly designated as sandy clay, and cannot be separated by movement in the clay. The probability thus is, that formerly, at the time when the grey clay was deposited in these localities, there has been considerably greater sedimentation. It has not been decided whether this can be traced back to the Glacial Period, or only to the last, more humid period. The latter is the more probable, as it is scarcely possible to imagine that the deposition takes place so slowly that only a very thin layer of clay has been formed since the Ice Age in a

[1] "Die niederländische 'Siboga' Expedition, von Prof. MAX WEBER". *Peterm. Mitt.* 1900. Heft VIII.

region where a comparatively large quantity of material is carried out from the land. It has been proved that since the Glacial Period there have been several alternate periods of drier and moister climate. The greater the precipitation, the greater is the erosion and the consequent deposit upon the sea-bottom, of terrigenous matter; and thus in a moister period, the grey clay will extend farther out into the sea. If we had a vertical section of the ocean-bed, we should see that wedges of the grey and the brown clay alternate with one another just as many times as there have been climatic periods.

Sample No. 10 of the *Fram Expedition* consists, unlike those from the other localities, of clay greyish above, brownish underneath. Here the possibility of a separation brought about by movement seems to be excluded, especially as the two kinds of clay have an equally large admixture of sand. It may be imagined that there was temporarily a rather considerable deposition of sediment at this particular spot as compared with what there had previously been; but as long as nothing is known of the climatic changes in these regions, there is nothing on which to base a conclusion as to the reason for this. On comparing the two kinds of clay, it appears that the sedimentation must take place two or three times as rapidly now as formerly. Strange to say, the chemical analysis of these two samples shows no corresponding difference in the proportion between the quantities of protoxide and peroxide of iron. The grey clay contains 2·40 and 5·30 per cent respectively, while the brown clay has 2·95 and 5·25 per cent; but this must be due to chance causes, possibly some difference in the mineral contents.

No chart of the distribution of the various kinds of bottom-samples accompanies the treatise, nor could one be drawn up with any great accuracy. The boundary between the shallow water deposits and the Grey Deep-Sea Clay, is placed, as already mentioned, at the 200 metres contour, and is thus not directly connected with the nature of the bottom-sample. Throughout the whole region, the only deep-sea deposit found is the Grey Deep-Sea Clay, a name that is not altogether appropriate, as it also includes several rather distinctly brown deposits. The boundary between this species of clay and Globigerina Ooze is placed at 30 per cent carbonate of lime in the report of the *Challenger Expedition*. If, following SCHMELCK, we make a special class under the name Transition Clay, some of the most northerly samples might

possibly come under that designation. As a rule, however, the Transition Clay contains a larger quantity of carbonate of lime than is the case with any of these samples. In the report of the *Ingolf Expedition*, I have put the limit at 5 per cent, a limit which is rather arbitrary, but which lends itself better to classification than the colour alone, which is generally very difficult to determine. If this classification were adopted, none of the *Fram Expedition* samples would come under the Transition Clay. Only one sample — No. 12 — has about 4½ per cent of carbonate of lime; *all the others have about 1—2 per cent* or even less, and thus cannot possibly be said to form any transition to Globigerina Ooze.

In order to obtain a more complete idea of the mechanical nature of the samples, and thus possibly to find out the laws that govern their distribution upon the sea-bottom, I have subjected some of them to *washing*. In the case of the samples of this expedition, washing is of comparatively far greater importance than it is generally. The proportion between the various sizes of the particles here, is almost the only thing that causes any considerable difference between the several samples, whereas in other cases the difference in the quantity of organisms, especially the calcareous organisms, and in the mineralogical components, are of equally great significance in unravelling the complicated conditions that assert themselves in the distribution of the different kinds of bottom deposits.

The washings are accomplished by the aid of a SCHOENE washing apparatus. In detail the same method is followed as in the case of the *Ingolf Expedition*, with the one exception that the lowest limit for the size of the grains is fixed at 0·01 mm. instead of at 0·02, principally because the first-mentioned limit is most employed in geological researches, and is thus more practical when it is a question of comparing bottom-samples with other deposits. In other respects, if the mechanical nature of the samples be represented graphically, the one of these limits may just as well be employed as the other, and as that of 0·02 mm. is much quicker to work with, and certainly also more accurate, it is very doubtful whether it is not to be preferred.

There is one very important circumstance in connection with the washing, to the clearing up of which my attention during the examination has been very particularly directed, and that is, whether it is possible to make any complete and reliable distinction between the various sized particles in which

the *Clay* is found in the samples. In order to solve this question, it will be necessary to subject it to a close investigation.

All clays or kaolins are originally possessed of so extraordinary a fineness that in the process of washing a large quantity will come out among the constituents that are less than 0·01 mm. It is for instance a well-known fact that the clay can be held suspended in fresh water for a very long time, so that in kaolin-pits and brick-fields it is necessary to have very shallow, flat reservoirs in order to get it separated from the water. This circumstance, however, is modified in many ways, and the transition to salt water in the first place, plays a very important part. It is also a fact that has often been observed, that clay sinks to the bottom far more rapidly in salt water than in fresh, although the former is not so fluid as the latter, and moreover has a higher density. THOULET[1] has pointed out that large particles sink at a perceptibly slower rate in sea-water than in fresh, and that the proportion between the velocities is about 0·955. The density of sea-water is about *1·0265*[2], and THOULET, in his experiments, has employed a solution of NaCl of this specific gravity. The proportion between the velocities of deposition would be 0·974, *(the reciprocal of the above named 1·0265)*, if the density alone determined the velocity of deposition. As the difference is, however, greater, it shows that the smaller degree of fluidity in sea-water must be of some consequence, and it would seem most natural to suppose that this would be of still greater significance in the case of the finest particles. Here, however, other circumstances assert themselves, and in order to come to a clear understanding of them, I have made the following experiments.[3]

A sample of very fine clay was boiled thoroughly in distilled water, and after being cooled down, an equal quantity of the liquid was poured into 3 glass cylinders of the same size and shape. One of these contained fresh water, the second, water with 1 per mille NaCl, and the third water with 35 per mille NaCl, or about the same permillage as sea-water. I have not investigated the question of the possible influence of the other salts in sea-water, but they probably act in the same way as common salt, although in

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[1] Analyse mécanique des sols sous-marins. *Annales des Mines*, April, 1900.
[2] Not in the Polar Sea, where it is 1·028. [F. N.]
[3] For further particulars see ATTERBERG: Studier i Jordanalysen. *Landtbruks-Akademiens Handlingar och Tidsskrift*, Stockholm, 1903, p. 217 etc.

a different degree. I then, in the various experiments, poured different quantities of the washed clay into the glasses, but exactly the same quantity in each of the three glasses, each time. It then appears that if the quantity of clay is so great as to make the water quite opaque in a glass with a diameter of 1·5 cm., there is a marked difference in the rates of deposition in the 3 glasses. During the first few minutes, no difference is to be observed; but at the expiration of half an hour, it will be seen that the water containing the greatest amount of salt has become almost clear, the slightly saline water is almost clear in the upper part and perceptibly clearer below, while in the fresh water only the uppermost layer shows a slight indication of a decrease in the quantity of clay, the greater part being apparently entirely unaltered. After the expiration of twenty-four hours the water containing most salt is quite clear, and the slightly saline water has only a very little clay floating in it, while the fresh water still contains so much, that for the greater part of its height it is quite thick, and it only becomes quite clear after several weeks. If, however, we observe the clay in 3 glasses, both during deposition and after, it will present a very different appearance in the different cases. In the fresh water, it is always exceedingly finely and evenly distributed, while in the salt water it coagulates, and after being deposited has an uneven, gritty appearance. Thus the effect of the salt is to cause the separate fine particles of clay to unite, and so in a manner alters the size of the grains. This is confirmed still further by the phenomena exhibited when the clay is in the form of a much diluted emulsion. It then appears that the differences between the action of the several kinds of water are much smaller. If we put into the glasses no more clay than is necessarv for a fairly clear observation of the deposition, no noticeable difference is perceptible in the 3 glasses for the first half-hour; indeed it almost appears as if the fresh water were a little clearer at the very top than the salt. After the lapse of a few hours, however, we shall nevertheless see that the salt water has become almost clear throughout, while the fresh has scarcely made any progress in that direction. This seems then to indicate that at first the particles of clay are at so great a distance from one another that during the first part of the time they are unable to unite to any great extent, and the fresh water's greater rapidity of deposition can thus actually make itself apparent; but a union nevertheless

subsequently takes place, after which it is not long before the clay is deposited in the salt water.

It might seem natural to assume that the mutual attraction between the clay-particles was due to osmotic action between the salt water surrounding them, and the fresh water enclosed in them from the boiling. For the purpose of investigating this, I boiled clay in salt water of the same concentration as sea-water, and poured the liquid into the various glasses. In connection with this it must be remarked that it is quite impossible to work with perfectly fresh water, as a certain amount of salt is conveyed by the clay; no very marked results can therefore be expected. It appears, however, as far as observation is possible, that the clay sinks with the same rapidity in the various kinds of water, and moreover with the same rapidity with which the clay boiled in fresh water, sinks in salt water. It also appears that in the last experiment the clay always coagulates very much, and that the above-mentioned osmotic action thus cannot be of any great consequence. At present it is not possible for me to give any physical explanation of the circumstance.

If we apply the result of these observations to conditions in nature, we must assume that the clay sinks to the bottom at very different rates, according as it originally comes out into the sea-water in larger or smaller quantities. The clay in the sea generally originates either from the rivers or from coast-erosion; but in the greater part of the polar regions, the glacial ice is an additional important factor. This, however, as already mentioned, is not the case in the region traversed by the Fram. The clay that is carried out by the rivers may generally be assumed to be present in the water in a far greater degree of concentration than that which is produced by coast-erosion. The result of this must be that in the neighbourhood of the great river-estuaries, deposits that are to no small extent argillaceous, must as a rule be formed; and this, as will subsequently be shown, is the case here in a marked degree. It must be assumed that the greater part of the clay-particles the rivers bring with them, will unite when they come out into the sea, down into the less disturbed water-strata, and will sink to the bottom very soon, while only a small quantity will be carried farther out to sea. At other places, on the contrary, a great part of the clay will be carried far away, while only a comparatively small part will be deposited nearer the coast. The

nature of the deposits in such places will thus, to a very great extent, depend upon the nature of the coast rocks. As those, however, of the north coast of Siberia, consist in great measure of clay deposits, morainic or stratified, the sea-bottom north of Siberia will, on the whole, be covered with a particularly argillaceous deposit.

The aim of the washing is in the first place, to try to find out the laws which have governed the distribution of the various deposits upon the sea-bottom. This is in contrast to washings performed for agricultural purposes where it is a question of the practical use of the clay, and where the aim of the washing is to get it as fine as possible. Here the ideal to be aimed at would be to get the clay washed out in exactly the degree of consistency in which it was deposited in nature, so that whatever had sunk in a coagulated condition could be retained in the same state, and thus come among the coarser particles. In practice this is scarcely possible. It is not unreasonable to suppose that the clay particles that fall upon the sea-bed in a very finely distributed condition, acquire, upon coming into contact with the other clay particles, exactly the same consistency as the clay that has sunk to the bottom in a coagulated condition. It should moreover be remembered that as time passes, the clay upon the sea-bottom will scarcely remain in the condition in which it was originally found. In all older deposits, the clay continues to become more compact, chiefly owing to the percolating water, which probably endeavours to cement it more and more together. As the extent of this operation is exceedingly varied in the various parts of the sea-bottom, the result is that the clay taken up in bottom-samples will be of a consistency that has little connection with the conditions under which it was originally deposited. The chief result obtained by the washing is thus to find out the proportion between the total amount of clayey substance on the one hand, and of true mineral particles on the other, and further the proportion between the various sizes of the latter. It is clear, therefore that in order to attain this, special importance must be attached to the measurement of the percentage of clay and sand of each separate size of grain, with the greatest possible accuracy.

In the actual washing, the salinity of the water will now play a very important part. As all the samples when taken are saturated with salt, this will be capable of disturbing the rate of washing to quite an extraordinary

degree as regards the clayey matter, if no attempt be made to get rid of it. This is best done by filtration. The sample is first thoroughly boiled in order to loosen the clay as much as possible and the liquid filtered, until it is impossible to demonstrate the presence of salt in it. When the boiling has been repeated, the clay will be ready for washing. It frequently proves, however, that even after this treatment the very small particles of the clay coagulate and fall rapidly to the bottom. In this respect there may often be a striking difference between samples that are altogether similar in appearance, and have been treated in exactly the same manner. In the one the clay may be quite finely distributed, and come among the constituent particles that are less than 0·01 mm.; in the other only a comparatively small amount will remain suspended at this rate of washing, while a large quantity will come among the coarser products. The rule in the main holds good that in those samples containing the greatest amount of sandy particles, the greater part of the clayey matter will also be thrown down, during washing, among the coarser particles. This again is a natural consequence of the circumstance that in places where sand is deposited, the wave-action, or the currents, must have such force that they can remove the finest particles, so that only such of the clay can be deposited as is more coherent than the remainder, virtually only that which originates from older, compact clay deposits on land, and which have become so firmly cemented together in the course of time, that they can no longer be disintegrated by natural force. A portion may indeed be comminuted by boiling, and there is consequently always, even in apparently, perfectly pure sand-samples, some small percentage of particles of less than 0·01 mm., especially as some of the sand particles may have been disintegrated and comminuted during the time they have lain upon the sea-bottom.

It is stranger when in two samples that are otherwise quite uniform, the clay is altogether different. We may here, for example, look at the curves for the samples Nos. 14 and 15. Both contain more clay than anything else, the clay in both being of the same brown colour, which gives the samples externally the appearance of being exactly similar; but the maximum size of the particles in the clay in No. 14 proves to be only a little less than 0·01 mm., so that a very large proportion of them are found among the coarser constituents, while the clay in No. 15 is extraordinarily fine, and almost entirely

consists of immeasurably small grains. In this case, almost the only possible explanation is, that even on the sea-bottom the clay must be subjected to some influence or other, in consequence of which the clay in No. 14 has been firmly cemented together, while that in No. 15 has been preserved almost in the condition in which it was deposited. It is not possible at present to give any reason for this phenomenon, which can probably only be explained by a large number of observations from many different localities.

Table of the quantity of the Particles of various Sizes in the Bottom-Samples.

No. of Sample	Depth in Metres	Percentage of Constituents				
		Under 0·01 mm.	0·01—0·05 mm.	0·05—0·5 mm.	0·5—1 mm.	Above 1 mm.
1	130	19·42	9·89	69·26	1·21	0·22
2	46	79·18	9·73	11·09	0	0
3	26	5·64	2·49	84·13	7·44	0·30
5	39	85·76	9·75	4·49	0	0
6	?	72·23	8·08	14·70	3·96	1·03
7	?	41.53	19·73	35·00	2·37	1·37
8	20	54·94	41·21	3·85	0	0
9	40	19·10	5·63	73·31	1·93	0·03
10 c	1460	70·95	26·18	2·87	0	0
12	100	56·88	30·01	13·11	9	0
13 a	3800	56·16	37·84	6·00	0	0
14	3850	58·23	35·85	5·67	0·25	0
15	3450	80·50	9·92	9·58	0	0

On looking at the above table, we are especially struck by the small quantities of the coarsest particles. It has previously been stated that these bottom-samples were distinguished from the great majority of others that are of purely terrigenous origin, by this peculiarity, and this is all the more striking when they are compared with others from the polar regions, which are generally strongly characterised by containing stony particles. On the average, the samples contain 0·23 per cent of particles of more than 1 mm., and 1·32 per cent of particles between 0·5 and 1 mm., total 1·55 per cent above 0·5 mm. If only the actual deep-water deposits, viz. Nos. 10 c and 13—15, be considered, the average of particles between 0·5 and 1 mm. is 0·06 per cent, while not one of these samples contains particles of more than

1 mm. By way of comparison, the results from the *Ingolf Expedition* may be given here, showing the particles of more than 0·5 mm. to have amounted to 4·17 per cent in the case of the Grey Deep-Sea Clay, a value which is about *69 times as large* as in the case of the *Fram Expedition* samples; while the number for the Transition Clay is 3·44, and for Globigerina Ooze 0·90. Even if a large proportion of these coarser particles in the *Ingolf Expedition* samples are derived from localities where the deposits contain a large quantity of stony particles from the rocks on the floor of the ocean itself, it has also been proved that a very large proportion of the stony particles must be carried thither by icebergs. In any case, the small percentages in the *Fram Expedition* samples show that both these factors are virtually altogether absent.

It has already been mentioned in the introduction, that in the Polar Sea north of Siberia, there are no icebergs to convey clay, sand, and gravel to the sea-bottom. Siberia sends out no glaciers, and those that come from Spitsbergen and other arctic lands, are not brought by the currents into this region. The inference that may be drawn from the above figures is of greater importance, namely, that *in the whole region traversed by the Fram north of Siberia, there are no projecting rocks upon the sea-bottom;* for these would always reveal their presence by a considerable quantity of stones in the samples, and generally also by an alteration in the mineralogical composition of the samples from one locality to another, if the submarine rocks varied in their nature. As will subsequently be related, it is not possible to prove anything of the sort in this region.

Again, a very important conclusion may be drawn from the complete absence of solid rocks on the floor of the ocean, namely, that *this region has been raised very little or not at all above the sea in any recent geological period;* for wherever this has been the case, will always be apparent from the great unevenness of the sea-bottom. The deposition of sediments always takes place exceedingly slowly, and in the region under discussion even more slowly than in most other places; and the very rough configuration of the bottom, appearing near the coasts of the mainland, will only be completely obliterated in the course of a very long time, by the gradual filling up of the hollows with sediment. It may thus be assumed that this

region has scarcely been raised above the sea in the Quaternary Period, nor probably in the Tertiary either.

As the particles of more than 0·5 mm. appear in such exceedingly small quantities, it follows that the sandy particles on the whole must be considerably in the minority as compared with the clayey particles. In the case of the 4 samples of Grey Deep-Sea Clay, the average quantity of particles between 0·05 and 0·5 mm. is 6·03 per cent, while in the *Ingolf Expedition* it is 29·55 per cent — quite a striking difference. An increase in the quantity of the finest particles will of course be found in the Fram samples in a corresponding degree.

A number of quite important variations between the several samples, also appear in the details. In order to obtain a better idea of these, and to give as clear a picture as possible of the mechanical composition of the samples, I have tried to represent this graphically, and have given the size of the grains as abscissa and the percentage as ordinate. In practice, however, this presents certain difficulties, and it becomes necessary to discuss more fully the manner in which a representation such as this, may be made.

The marking of the figures on the ordinate presents no difficulty whatever. The percentages may be put down in an ordinary arithmetical series, which will prove to be sufficient in all cases. This is by no means the case with the abscissa figures. If we attempt in a similar manner to put down the figures denoting the size of the particles in an arithmetical series, it will be impossible in the great majority of cases to find a scale which will include all the sizes of the particles in the sample, and at the same time give a characteristic representation of the most important constituents of it. This may be best illustrated by one particular example. Sample No. 1 contains, as we have seen, 0·22 per cent of particles of more than 1 mm.; the largest is 1·55 mm. If all the sizes of grain are to be included in one scale, this may be divided, for instance, into 30 parts of 0·05 mm. each. The particles of more than 1 mm. will extend over 10 divisions, and will thus average a height of 0·022, which it will be impossible to show on a drawing unless the units of the abscissa are very large, and this, as we shall presently see cannot be done with the finer particles. The particles of sizes between 0·5 and 1 mm. also extend over 10 divisions. As they are found to the amount of 1·21 %, the curve will here be drawn at a height averaging 0·121 units, which at any

rate better admits of being represented. The particles between 0·05 and 0·5 mm. extend over 9 divisions. They amount, in all, to 69·26 per cent, and thus the height becomes 7·7; so that this size occasions no difficulty. The same may be said of the particles between 0·01 and 0·05 mm. which occupy 0·8 of a division; the percentage is 9·89, and the height consequently 12·36. But the difficulty returns with the finest particles of less than 0·01 mm. As they occupy only 0·2 of a division, and are present to the amount of 19·42 per cent, they would have to be marked at a height of 97·1, which is practically impossible, when a height of 0·022 has to be shown for the coarsest particles at the same time. Even then we get no idea of the manner in which these finest particles are distributed within the area they occupy. It is always possible to see approximately in the microscope what is the size of the majority of the particles, and to observe that in this respect there is a considerable difference between the several samples. At some place or other the curve must have a maximum, whence it descends regularly to both sides; and shades of difference such as these cannot possibly be drawn in the space of 0·2 of a division, when at the same time there are to be 30 divisions. It will thus be seen that with this mode of representation it will be impossible to include either the finest or the coarsest particles, and it is clear that the case will be much worse when the particles of less than 0·01 mm. rise to 100 %, or thereabouts or when, on the other hand, stony particles appear in the sample. A graphic representation with the sizes of the particles in arithmetical progression can thus be employed only when the samples dealt with, are very homogenous practically only with pure sand or gravel deposits, and it can only be employed for purposes of comparison with other samples, when the latter are very nearly of a similar composition.

The case is altogether different when the figures for the sizes of the particles are given in a geometrical series. As will be seen from the tables, the quotient 2 is here chosen as the unit. With 1 mm. as the starting-point, we put 1/2, 1/4, etc. on the left, and 2, 4, etc. on the right. The abscissa can thus be continued indefinitely on both sides, but will in no case acquire a length that is not applicable. The advantages of the method are best seen by looking at the curves themselves. The greater compression of the coarser particles in relation to the finer, enables even the smaller percentages that are as a rule found, to be very clearly shown, and the curve can easily be extended to in-

clude even very coarse, stony components, such as, for instance, diluvial gravel and the "Till" contain. With regard to the finest particles of less than 0·01 mm., it will here also be immediately evident that there is a very essential advantage in their curve being extended over a greater space, as a considerable percentage of them is usually found in the samples. It also becomes possible to give the differences in the size of the grains in the clay, that can be observed under the microscope, by making the highest point of the curve correspond to the size of grain found in the greatest quantity. But this part of the curve cannot, of coarse, be very accurate in detail, while the area enclosed by the curve must naturally give accurately, the value found for the percentages of the particles under 0·01 mm. The only difficulty which cannot in any way be surmounted in this mode of representation, is the impossibility of determining how far to the left the curve is to extend, as there is no means of finding out the size of the smallest clay-particles. That the sizes of the particles do not decrease to an unlimited extent may be inferred from the fact that all the clay does at last sink to the bottom in water, even though in certain cases it may take weeks to do so. In none of the samples is the clay of such fineness that 99 % of it will not have sunk to the bottom in the course of 24 hours in a glass that is 20 cm. in height. I have always put the lower limit at a size of about $^1/_{8000}$ mm. If it were in reality to be a few units farther to the left, that part of the curve lying to the left of 0·01 mm. would be somewhat flatter as the total area must remain the same; but in other respects the figure would not be altered in any essential degree. By adopting the same mode of procedure everywhere, it will be possible to make every difference between the various samples stand out sharply and distinctly.

The reason for choosing geometrical progression for representing the sizes of the grains, is not only that by its aid it is possible to represent quite otherwise than with arithmetical progression, the mechanical composition of any deposit whatever, and thus by the same system to delineate all kinds of rock, from the finest clays to the coarsest gravel deposits; but it is also because this mode of representation is quite in keeping with the natural forces that operated in the formation of the rock. If we examine a tolerably uniformly grained rock, such as stratified sand, we shall always find that when most of the grains are, for instance, 1 mm. in size, the percentages of grains measur-

ing 1/2 and 2 mm., 1/4 and 4 mm., etc., will be as nearly as possible equal. A single operating force — in this case the velocity with which the water moves at a particular place — will thus always make itself apparent in a perfectly regular, symmetrical curve — the so-called curve of probability —, which will be more arched or flatter according to whether the force in question has operated more or less regularly and exclusively. If we see that the curves take another form, we may conclude that some factor or other has asserted itself, which must be calculated separately in each case. If, for instance, the right side of the curve is greater than the left, coarser particles must have become mingled in the sample by some special natural agency; accidental circumstances in the principal depositing force cannot by themselves have produced such irregularity.

As regards the bottom-samples, the curves that demonstrate their mechanical nature, will have many varied and peculiar forms. This shows directly that a number of different natural forces have asserted themselves at the time of deposition. The bottom-samples on the whole, belong to the less regular deposits; the numerous alternations in the sea, in wind, temperature, and currents, the many different sources whence the constituents originate, and further the previously mentioned, peculiar condition that clay presents in salt water, all cooperate in making the deposits irregular. The curve for each separate sample shows directly how great this irregularity is, and thus supplies a means of finding out to some extent, to what degree the various forces have asserted themselves in the deposition of the sample.

The curves were constructed in the following manner. First, on the axis of the ordinate, figures are placed indicating the percentage of grains of each separate size. The highest value that it has ever been necessary to employ with these samples is somewhere about 30. In very homogenous rocks, it will rise somewhat higher, though very seldom above 70 or 80. In the next place, the values of the sizes of grains are placed on the axis of the abscissa, 1 mm. being taken as the starting-point, and the quotient 2 as the unit. The area of the curve situated over each single unit indicates the quantity of the corresponding size of grains. The scale is thus unlimited on both sides. In these samples it has not been necessary to go to greater values than 2 mm., and the lowest value given is $\frac{1}{8192} = \frac{1}{2^{13}}$. Among the values employed in washing, 0·05 and 0·01 mm. do not fit directly into this

scale; 0·05 lies between $\frac{1}{16}$ and $\frac{1}{32}$, and by calculation, its distance from $\frac{1}{16}$ is found to be 0·32 of a unit; 0·01 lies between $\frac{1}{64}$ and $\frac{1}{128}$, and is found to be at a distance of 0·64 of a unit from $\frac{1}{64}$. Among the parts into which the whole sample is divided by washing, the particles measuring 1—2 mm. and 0·5—1 mm. each occupy 1 unit, those measuring 0·05—0·5 mm. 3·32 units, and those measuring 0·01—0·05 mm. 2·32 units, while the finest particles, of less than 0·01 mm., extend over a number of units, which cannot be exactly determined. When $\frac{1}{8192}$ is placed, as already stated, as the extreme limit of the scale to the left, this gives 6·36 units for this part of the sample. If we now divide the percentages of the various sizes of particles found by washing, by these figures, which give the number of units embraced by each size, we obtain the average height attained by the curve in the space in question. By being content with the horizontal lines only, we should have obtained a very poor representation of the actual thing, where sudden leaps from one size to another are never found. I have therefore always rounded off the curves, and two different methods may be employed for getting at the correct form. The surest is of course to examine the samples themselves under the microscope or with a lens. If we place, for instance, a sample of the size 0·01—0·05 mm. under the microscope, and examine the grains of these particular sizes, a fairly accurate idea may be formed as to what are the intermediate sizes that are most abundantly represented in the sample, and thus as to where the highest point of the curve will lie, and also to some extent how much higher it will be at this spot than at other parts. A consideration of the adjacent sizes will also to a great extent assist in the determination of the form of the curve. For instance, it is evident that when we find a very small percentage of one size, the curve for the size or sizes that lie next to it, falls down very rapidly towards it. Both ways always lead to the same result.

It will be seen that there are always two curves drawn. The upper one gives the quantity of all the constituents of the sample, the lower the quantity of the true mineral particles. Two areas are thus obtained, one for the clayey matter, the other for the mineral particles, here designated, for the sake of brevity, respectively *clay* and *sand*. This division is made by counting under the microscope a number of particles, and calculating the percentage of clay and sand in each separate size. This admits of being accomplished with tolerable accuracy so far as the coarser particles are concerned; but in the very

finest constituents it is impossible to distinguish the clay and the sand from one another. For this reason, the part to the extreme left of the lower curve is rather inaccurate; it is not drawn quite so far out as the upper one, as it may be taken for granted that the mineral grains do not attain the same degree of fineness as does the clay; but how small they can actually be, it is impossible to say.

In the following table are given the values by the aid of which the curves are constructed. For the three smallest sizes — less than 0·01 mm., 0·01—0·05 mm., and 0·05—0·5 mm. — the percentages are given, and the average height of the curve for these sizes calculated from the percentages, by dividing by 6·36, 2·32, and 3·32 respectively. With regard to the larger particles, which each occupy 1 unit of the scale, the height of the curve is equal to the percentage found in the preceeding table. The percentage of clayey matter in the four smallest sizes is also given: particles of more than 1 mm. never contain clay.

Constituent Particles.

No. of Sample	Less than 0.01 mm.			0·01—0·05 mm.			0·05—0.5 mm.			0·5—1 mm.
	Percentage	Height of Curve	Per-cent. Clay	Percentage	Height of Curve	Per-cent. Clay	Percentage	Height of Curve	Per-cent. Clay	Per-centage of Clay.
1	19·42	3·05	80	9·89	4·31	40	69·26	20·83	15	25
2	79·18	12·45	80	9·73	4·19	25	11·09	3·34	15	
3	5·64	0·89	50	2·49	1.07	70	84·13	25·19	5	5
5	85·76	13·48	80	8·75	4·20	30	4·49	1·35	15	
6	72·23	11·36	70	8·08	3·48	20	14·70	4·43	10	5
7	41·53	6·53	70	19·73	8·50	50	35·00	10·54	35	20
8	54·94	8·64	60	41·21	17·76	25	3·85	1·16	10	
9	19·10	3·00	50	5·63	2·43	35	73·31	22·08	8	10
10 c	70·95	11·16	70	26·18	11·28	45	2·87	0·80	8	
12	56·88	8·94	70	30·01	12·93	15	13·11	3.95	10	
13 a	56·16	8·83	80	37·84	16·31	50	6·00	1·81	30	
14	58·23	9·02	80	35·85	15·46	50	5·67	1·71	10	0
15	80·50	12·66	85	9·92	4·28	5	9·58	2·89	5	

As the samples throughout are rather varied in their mechanical composition, it is hardly possible to give any general opinion regarding the relation between the forces that have aided in their deposition; they will therefore be taken separately in the following pages.

No. 1, which was taken on the 24th July, 1893, about 60 miles west of Goose Land on Novaya Semlya, is a good example of a partially, uniformly grained sample. The bulk of the particles, which lie between 0·05 and 0·5 mm., form a complete whole, which is due to a single factor. In this instance it can only be the ocean currents, which flow in a north-westerly direction in this region, coming from the Vaigach Straits (cf. NANSEN, IX, p. 267). The current must have just sufficient velocity to carry grains of this size from Novaya Semlya and Vaigach to the station in question, while the larger particles fall to the bottom nearer these islands, and the finer particles are for the most part carried farther away. The great expansion of the curve to the left, however, shows that in this locality too, there must be some possibility of the finer particles being deposited. This can only be the case when the water near the bottom does not travel with any very great velocity, and this again may possibly be caused by the fact that the locality is situated in a hollow compared with its nearest surroundings. Other causes, however, may also be imagined. The existence of two maxima in the curve for the clayey matter is probably due to the fact that the clay in the sample is not of the same consistency as when it was deposited. As the very great majority of the sandy particles are of sizes between 0·05 and 0·5 mm., it is probable that the greater part of the clayey particles had also corresponding sizes, when originally deposited, they have thus been in a coherent condition, and they may to some extent have been disintegrated by boiling. The fact that this is not the case with the remainder shows that the clay must have had time to become partially transformed since its deposition, which in this locality does not take place with any very great rapidity, as the comparatively deep brown colour of the clay also indicates. The extension of the curve to the right, which is quite marked although very small, shows the influence of the ice. Whether ice floes of shore ice, or possibly icebergs from Novaya Semlya, there will easily be carried a small quantity of particles of all possible sizes, of which only those of more than 0·5 mm. will be perceptible in the sample, while the finer particles will be occluded among the other constituents.

Nos. 2—6 were all taken in the sea north of the mouths of the Obi and the Yenisei. The three samples, Nos. 2, 5 and 6, resemble one another greatly, and have a very characteristic appearance as compared with the other samples taken near the coast. The river-water, as already mentioned,

brings with it much greater quantities of clay than can be produced by coast-erosion, and in consequence of this clay being present in so comparatively concentrated a degree, a considerably greater percentage of it than would otherwise be the case will sink to the bottom as soon as it comes out into salt water. The result of this is that these samples contain a greater quantity of argillaceous matter than any of the others with the exception of No. 15, which was taken at a particularly great distance from land. As the process of deposition in these localities must be assumed to be relatively very rapid, the clay has not had time to undergo any appreciable transformation, and has therefore almost all been washed out in a very fine condition. The colour of the clay also leads to the conclusion that the deposition has taken place rapidly, all three samples being described as brownish grey, about half-way between grey and brown-grey.

The curve, however, for all three samples shows a very considerable extension to the right, which must have its own peculiar origin. It cannot be supposed that the rivers themselves have brought these coarser particles with them, for in that case the curves would have had a far more regular appearance, whereas Nos. 2 and 6, at any rate, have a separate maximum at the particles between 0·05 and 0·5 mm. The reason of this extension is to be found either in the river-ice or in the particles arising from coast-erosion. The latter seems to be best suited to the form of the curves.

NANSEN (These Reports Vol. III, No. 9, p. 294) mentions that the Fram during nearly the whole of her voyage along the coast of Siberia, experienced currents running almost exactly to the SW. in a direction opposite to her course. These currents were related to the winds prevailing at that time, which were likewise contrary. If such a current is at all usual in this region the differences in these three samples just mentioned, can of course be very easily and naturally explained. Material torn loose on the coasts will be carried down by this stream towards the mouths of the Obi and Jenisei and the two localities represented by samples 2 and 6 would in that case be more exposed to the effects of the current than would No. 5, and would in consequence contain much more sandy material. As No. 6 came from a good deal nearer to the Siberian coast, it is naturally somewhat coarser with quite a considerable quantity of particles over 0·5 mm., than

No. 2; particles of such a size could only be to a very small extent carried to a locality as far out at sea as that from which the latter was taken.

Meanwhile, it is scarcely probable that the above mentioned current is very constant in direction, and in that case other factors will also in time be able to make their influence felt. No. 2 probably obtains a considerable quantity of its sandy material from the near-lying *Sverdrup's Island* which according to NANSEN is certainly formed of rather sandy kinds of rocks; Sample No. 5 lay rather farther away from this Island and the conveyance of sand from the one to the other must consequently have been much less. It would also appear to be likely that large amounts of material expelled from the large rivers in this neighbourhood, could be scattered over the sea bottom here; and this being so, No. 5 will owing to its closer proximity, receive the largest amount of such material; which accounts for this particular sample being of a more clayey character than the other two. As, however, nothing is up to the present, known as to what sort of material the drift ice which undoubtedly plays a very great part in the transportation of the river material, actually does carry with it, there appears to be no possible chance of saying exactly what influence the addition of materials derived from this source can have had on the general texture of the samples.

Sample No. 3 has a quite different appearance; it is composed of nearly clean sand. As it was taken in rather close proximity to *Sverdrup's Island* it is reasonable to suppose that at least a large portion of the sanded ingredients owe their origin to this source. Meanwhile, it is difficult to imagine that an Island of such insignificant dimensions should be able to supply such a large quantity of sand, as to many times outweigh the amount of clay presumably brought down from the great rivers, were it not that the sample was taken from such a small depth that wave motion, currents, etc., may have had a very appreciable influence in determining the proportion of admixture. Where the water near the sea bottom is either always, or at least at frequent intervals in motion, clay particles could not possibly be deposited, and even if they sink whenever the water happens to be still, they will soon be carried away again. The comparatively insignificant amounts of fine clayey material that appear on examination, appears to make it probable that this sample was deposited entirely as clean sand; and the small amount of material under 0·05 mm. must consequently be attributed

to later changes that have possibly occurred to the sea bottom itself, owing to which a proportion of the sand granules have fallen apart once more. The subsequent boiling-out in the laboratory must also have been of influence in this connection.

Sample No. 7 was taken west of Taimur Sound, in a little bay in the Western Taimur Peninsula. The geographical conditions here are especially complicated, as just off the land there is a large number of islands that have not as yet been carefully surveyed, and these may give rise to a very complicated condition of currents and deposits. The curve, too, presents a rather irregular appearance with several maxima both for the clay and the sand; and it is quite impossible to make out the details. The colour of the clay is a rather decided grey, which makes it reasonable to assume that the deposition has taken place very rapidly. A large portion of the clay, however, has not been capable of disintegration by boiling, but this may be because it orginates from older deposits on shore, which can often become so hardened in the course of time, that the clay can no longer be disintegrated by boiling. The comparatively large number of particles of more than 1 mm. may possibly be due to conveyance by ice, but may also arise from the close proximity of the sample to land.

Stations 8 and 9 are situated to the east of the Eastern Taimur Peninsula, outside the Khatanga Bay and north of the mouth of the Anabara. In this region there are *north-flowing* currents of river water from the coast, so that we may assume that a large proportion of the material originates from the rivers. The two samples differ greatly from one another. The curve for No. 8 is very regular in its course, as regards both the sand and the clay; there are here very few particles of more than 0·05 mm. The maximum of the curve for the sand is about $\frac{1}{50}$ mm. with an even descent to both sides; and thus, whether the material originates from the rivers or from the coast, the drift of the current must be such that most of the coarser particles have been deposited already. The colour is a purer grey than that of any of the other samples. It is scarcely probable, however, that this sample should have been deposited more rapidly than those already mentioned off the mouth of the Yenisei; features in its chemical nature may possibly also have asserted themeselves here.

No. 9 is very different from the foregoing sample. The right side of its curve bears a great resemblance to No. 3, and, like it, indicates a regularly assorted sand deposit. The sample as compared with No. 8, must thus have been in a situation that was considerably exposed to the currents. Probably it was taken from a place somewhere in the centre of one of the currents carrying water from stony-bedded rivers; No. 8 on the other hand, was taken farther out at sea and most probably, must be regarded as coming from a locality at the edge of the current, where the water would be likely to be almost always quite still. The curve for No. 9, however, has also a fairly large left part, which is too large to have been formed by division and disintegration of the sandy particles. It almost seems as if the whole sample consists of two parts that are formed quite independently of one another; and as it is not possible to imagine them formed simultaneously, it must be assumed that the sample has been stratified in such a manner that either two or several layers of clay and sand have alternated with one another. It is true that no such stratification has been directly observed, but it may easily have been obliterated when the sample was taken. That portion of the sample that consists of clay, must have been formed in one or several periods, during which the circumstances were identical with those prevailing at the formation of No. 8 which happens when the prevailing current changes its direction away from the locality of 9. The colour of the clay in No. 9 is also a fairly pure grey, although not quite so grey as No. 8. This colour may have been produced by a mingling of the altogether grey clay in the finer portion of the sample with the browner clay in the coarser particles.

The samples Nos. 10 and 13—15 all belong to the true deep-sea deposits, and are formed far from land. All chance local circumstances such as influence the samples nearer the coast are consequently excluded, and moreover these samples, all without exception, show quite a regular form of curve, with only one maximum for both the sandy and the clayey particles. In detail, however, there are certain differences between them. They have the flat, broad form common to all deep-sea deposits, which are never indeed made up of very much assorted material; but it must be noticed that the curve draws more and more to the left the greater the distance from land, and that in the greater depths of the great oceans the coarser particles

altogether disappear. This stage is not reached by any of the deposits of the *Fram Expedition;* No. 15 alone shows a slight indication of it. Most of the deep-sea deposits have shades of brown deeper than any of those formed in the vicinity of the coast, a fact which is immediately connected with their slower formation.

As already stated, sample No. 10 consists of various layers, of which the uppermost, No. 10 a is of a greyer hue, and the lowest, No. 10 c, more brownish, an order which differs from that generally prevailing wherever the ocean-floor consists of deposits of various colours. In other respects, the different samples exhibit very much the same consistency; the greyish layer being at most a trifle more sandy. Only the lowest layer, No. 10 c has been washed. As the curve shows, the maximum for the sandy particles is found at about 0·01 mm., and only a trifling proportion is greater than 0·05 mm. The current, which here flows north from the New Siberian Islands and the Siberian coast east of the mouth of the Lena, has thus not sufficient force to enable it to convey the still coarser particles so far[1]; and the small quantity of these also shows that the transport by ice can only play an insignificant part in these regions. It is impossible to say anything decided regarding the reason of the greyer colour of the uppermost layer as compared with the nethermost. It may, as usual, be assumed that this colour denotes that the layer has been deposited more rapidly than the lowest layer; and this change may possibly arise from the increased velocity of the currents in these regions.

The fact that raised beaches with shells, etc., are found on the New Siberian Islands[2] indicates that the surrounding region once stood at a lower level than it does now. It is very likely that the brown layer was formed at this time; the higher water level must have lessened the velocity of the currents going north from the Siberian rivers, and the transport of material must consequently have taken place on a smaller scale and at a slower rate. This would all seem to indicate that deposition in this region must be an extremely slow process; and in confirmation of this, it may be pointed out

[1] Especially as NANSEN proved that the stream which here runs towards the North, only has a range of ca. 200 m.; underneath it there is a current in the opposite direction.

[2] See JAMES GEIKIE: "The great Ice age," 1894, p. 700.

that a not very powerful current such as this north-going stream, can hardly be expected to carry great quantities of material very far from land, before they will fall into the cross-current lying below it. Neither can the material originating from the ice be very considerable as there can hardly be much opportunity of melting in these districts.

Samples Nos. 11 and 12 are from just east of the preceding one, and thus rather nearer to the New Siberian Islands. The samples are taken from a far smaller depth, 135 and 100 m., while No. 10 is from about 1460 m. These samples are quite different, No. 11 being more sandy, and No. 12 more clayey. So little of the deposit was obtained in the first, No. 11, that it was not examined more especially. The reason for its more sandy consistency in comparison with No. 12 may be the same as was given in the cases of samples 8 and 9, namely that No. 11 is situated in a stronger current, while No. 12 is in comparatively quiet water; but possibly also other unknown reasons can have prevailed during the formation of these samples.

The two samples, Nos. 13 a and 14 were taken rather close to one another, in very deep water; it is not therefore to be expected that there would be any appreciable difference between them. The curves are also almost exactly alike, and are both exceedingly regular, with few particles of more than 0·05 mm., and with the maximum for the sandy particles at about $\frac{1}{64}$ mm., and that for the clay particles about 0·01. The clay has thus only partially been disintegrated by boiling, but still in far greater quantities than might be expected from the circumstance that these samples have been deposited so slowly; the rule already mentioned holds good here, that in very deep water the cementing together of the clay takes place only in a very slight degree. The colour of both samples is given as greyish brown, and in reality is not very far from pure brown. Sample No. 13 b is designated as the upper layer; unfortunately there was so little of it that not even the colour could be determined; otherwise it would have been extremely interesting to see whether the difference between the upper and lower layer was in the same direction as in No. 10.

Sample No. 15 was taken at a great distance from land, almost due north of Cape Chelyuskin. It has the most pronounced deep-water character of all the samples examined, with a greatly preponderating quantity of clayey matter, and among the mineral particles the finest are especially predominant.

This last attribute is not immediately apparent from the curve, which shows quite a large number of more than 0·05 mm.; but in reality the greater part of this area consists of Foraminifera, which with the increased coarseness of the particles, make up a continually increasing percentage in the sample. As the size of the Foraminifera in the sample have nothing whatever to do with the laws that determine the inorganic particles, they ought in reality to be either altogether omitted, or drawn as an independent part of the curve, *e. g.* under the axis of the abscissa. As, however, No. 15 is the only sample in which the Foraminifera play any appreciable part, I did not wish to introduce a new principle for this one curve. The colour of this sample is quite a pure brown; it has probably been deposited very slowly. Most of the clay, and the finer sandy particles may be assumed to have been carried to the locality by currents; while the coarser sandy particles and a corresponding portion of the clay have probably originated from the ice. Almost all the clay has been brought out in the washing among the very finest particles; and thus, at this great distance from land, special conditions must prevail, which prevent the coagulation of the clay. Several samples would be necessary, however, for a closer investigation of this matter.

III.

MINERALOGICAL COMPOSITION OF THE SAMPLES.

After treating of the mechanical nature of the samples, and the results that could be obtained by washing, it still remains to look at the samples from the other side, and examine their composition from a mineralogical point of view. One circumstance belonging to this side of the question has already been discussed in the preceeding section, namely the amount of clayey matter in the various sizes of grains; for this plays a very important part in the composition of the samples, which cannot be lost sight of in treating of this composition. In the following pages, the relation between the remaining minerals will be as far as possible determined.

For practical reasons, the treatment of each size of grains has been taken separately, so that the particles of more than 0·5 mm. are examined directly with a lens, while the particles between 0·05 and 0·5 mm. are axamined as microscopic preparations. The still finer particles are not considered here, as the separate minerals in them cannot be accurately distinguished from one another, and the proportion between them may be assumed to correspond fairly well with the proportions of the coarser particles. The particles of more than 0·5 mm. on the other hand, may sometimes give valuable information especially regarding the mineral combinations, or rocks present, which cannot be obtained from the finer particles. In this particular, however, the bottom-samples of this expedition are rather unsatisfactory, as few of them contain these coarser particles, which, moreover, in no case attain a greater size than 2·5 mm. Thus one of the most important means of determining the species of rock that have furnished the material of the samples is wanting here.

1. *Particles of more than 0·5 mm.*

No. 1. 1·63 per cent. Largest grain 1·5 mm., quartz.

Almost exclusively quartz in rounded particles; a few grains of felspar, magnetite, biotite, and a single grain of muscovite.

No. 3 a. 7·74 per cent. Largest grain 1·4 mm., quartz.

A great majority of quartz-grains, which were found in very large numbers, most of them only a little larger than 0.5 mm. Some felspar and magnetite were also found, and a few grains of garnet, olivine, and hornblende.

No. 6. 4·99 per cent. Largest grain 2·5 mm., fine-grained, grey, quartzitic sandstone.

Quartz predominated in the remainder, but there were also a great many concretions of iron, consisting for the most part of sandstone, cemented together with limonite; and there were also some clay concretions. Further, there were found a few grains of felspar, magnetite, augite, chlorite, and biotite.

No. 7. 3·74 per cent. Largest grain 2·2 mm., quartz.

Quartz predominated, but here, too, there were a number of iron concretions. A number of small fragments of quartziferous, fine-grained rocks were also found, which could not be very accurately distinguished from one another, possibly quartzite, quartz-porphyry, granulite, or something similar; also a few grains of felspar and magnetite.

No. 9. 1·96 per cent. Largest grain 1·2 mm., graphite.

The remainder was almost exclusively grains of quartz, with a few particles of felspar, magnetite. and similar fine-grained rocks as in tbe preceeding sample, a single piece of sandstone.

No. 14. 0·25 per cent. Largest grain 0·7 mm., concretion.

The only things found in this sample were 19 brown or black iron conretions, 9 grains of quartz, and 1 piece of graphite.

As will be seen from the above, these coarser particles reveal, on the whole, very little as to the nature of the rocks that have yielded material to the bottom-samples. The proportion between the various minerals may indeed

declare something concerning the composition of the original rocks; but this can be found out much better from the finer particles, as their number is always far greater.

As far as we know, the whole of the coast of Siberia along which the Fram sailed, consists of very quartziferous rocks. On the Western Taimur Peninsula, granite is found, and crystalline schists; on the Chelyuskin Peninsula, quartzite has been observed. The coarser-grained rocks were not to be found as such in the bottom-samples, which contain only very small particles; the fine-grained are met with in a few of them, but on account of their minuteness are always in a rather unrecognisable condition. It is impossible to say whether the quartzite found in samples Nos. 6, 7, and 9 comes from the quartzite *in situ* on the Chelyuskin Peninsula. There are still such great stretches of the coast that are quite unkown, and moreover the material is quite as likely to have originated from the loose materials on the coast, which, over long distances, have been observed to form plains outside the solid rocks, and, as such, will be more likely to be carried away by the shore-ice and the waves than the solid rocks. As these looser deposits consist largely of morainic formations, and may have their origin in rocks that are *in situ* at a considerable distance, it would be quite impossible to say anything about the first origin of the by no means many kinds of rock, found in the samples.

With regard to the concretions of iron that are found in certain of the samples in quite considerable numbers, there may be some doubt about their primary or secondary origin. It has long been a well-known fact that on the floor of the ocean concretionary formations of iron and manganese are often formed. Concretions of iron are of course also found in the rocks on land whence the material of the bottom-samples originates; but it is not probable that they would come thence in quantities so large in proportion to other constituents — especially quartz — found in certain of the samples. In such cases then, we must assume that the greater number are formed on the floor of the ocean itself. This is quite certain, notably in sample No. 14, where the concretions of iron considerably outnumber the grains of quartz. They must then be looked upon as formed outside some very ferruginous mineral grains, that have been very easily decomposable. Their degree of firmness is very varied. They sometimes form well-defined, hard bodies; but most of them are of a considerably looser

character, so that they can be partly disintegrated by boiling, and thus cannot be distinctly seen. The formation of iron concretions is not very closely connected with the other chemical processes that take place in the samples, and this confirms the assumption that they must owe their formation to some ferruginous mineral particle or other. They are found, for instance, in great quantities both in some of the samples that are of the darkest brown colour, *e. g.* No. 14, and in some of the greyest, *e. g.* Nos. 6 and 7. The appearance of a brown colour in the clay must also be due to a formation of ferric oxide, but this has nothing to do with the process by which the concretions in the clay are formed. The former consists, as already stated, in a deposit on the surface of the clay, precipitated by the sea-water itself; and is therefore most abundant in the most slowly formed samples, which on that account are the brownest. On the other hand, the concretions in the clay are certainly connected with the formation of the concretions described in the "*Challenger*" Report, from the very deepest oceans. Here the deposition of sedimentary matter takes place so slowly, that the iron and the manganese of which the concretions are composed, have time to be formed in great, coherent masses.

Nor can there be said to be much connection between the formation of iron concretions, and that other, not very well known, chemical process, which causes the clay to acquire a firmer, more undecomposable consistency. The formation of concretions will naturally bind the clay together as far as it extends; but that it is very local may be seen from the fact that in a few of the samples that contain most concretions of iron, such as Nos. 6 and 14, the clay otherwise is very incoherent, this being the case particularly with No. 6.

2. *Particles measuring 0·05—0·5 mm.*

In order to form an idea of the nature of the rocks that have yielded material to the samples, it has been necessary to subject the sandy particles in the samples, to a close examination. On a direct inspection of the sand, it appeared that by far the greater part of it in all the samples, consisted of quartz. To make microscopic preparations of the sand as a whole could thus lead to no result, for in such preparations the other minerals would be pre-

sent in only very small quantities, so that the differences that possibly existed between the several samples, could not be clearly brought out. It was therefore necessary to separate the quartz from the other minerals, and for this purpose acetylene bromide mixed with ether until the specific gravity was 2·85, was used. By this means the quartz and the felspar on the one side were separated from the great majority of minerals on the other. In the case of the particles that consist of a single mineral, the separation can be made very complete; but there are of course always a certain number of particles that consist of a very fine-grained mixture of several different minerals. There is no definite specific gravity for these, and they may therefore enter both sections. The same may be said of the iron concretions, which may have a very different specific gravity, according to their greater or slighter impregnation with ferric oxide. On the other hand, all the conglomerated clay that is found between the grains of sand, will come among the lighter constituents. The percentage of the two parts of sand was directly determined by weighing. As there was only a very small quantity of sand in some of the samples, it was impossible in their case to make any separation; and as an examination in any other way can give no result where it is a question of pointing out such small differences as those under discussion, the examination of these samples has been altogether omitted.

a. *The Heavier Particles.* The mode of treatment here was to count 100 grains of each sample on a slide, and the percentage of the several minerals is then directly given. The percentage in numbers must correspond fairly accurately with the percentage in weight. It is true that certain of the minerals, especially magnetite, have a considerably higher specific gravity than the others; but in a like degree the size of the particles will be less. The sizes of grains that were indicated as obtained by washing, are only applicable accurately to rounded quartz-grains; considerably smaller grains of the heavier minerals will continually be washed out among them.

The table below gives first the percentage of minerals with a higher specific gravity than 2·85, and next, the proportion among these of the various minerals. A cross under any of them indicates that it is not found among the 100 minerals counted, but on searching in the remainder of the preparation. It is thus highly probable that such a mineral is present in a percentage of less than 1.

No. of Sample	Percentage of Heavier Minerals	Opaque	Aggregates, Concretions etc.	Garnet				Hornblende		Augite		Hypersthene	Epidote	Olivine	Tourmaline	Rutile	Zircon	Biotite
				red	violet	colourless	brown	green	brown	ordinary	green							
1	0·45	10	7	4	6	6		21	2	18	6		4	11	1		3	1
3	2·90	12	26	4	3			4	3	48	1			6	1	1	1	
6	1·33	10	38	3	6	+		1		28	+			7	2	1	4	
7	1·26	8	37	2	22		1	3		22		1	1	1			2	
9	0·59	6	28	11	17			+	1	23		1	1	9	2	1		
15	2·48	20	20	1				12	1	33	4		1	6	1		1	

As almost all the samples have, as we have said, a rather marked local character, there is here, as in most other respects, scarcely any common feature that binds them together except that they all contain rather small quantities of the heavier minerals. If, for instance, we compare these samples with corresponding samples from the west coast of Greenland, we shall see that the latter nearly always contain considerably greater quantities; even the one that contains the smallest quantity (No. 38 of the Ingolf Expedition) has 4·5 per cent of heavier minerals, and the others have 10 per cent or more. And yet the West Greenland samples also originate solely from very quartziferous rocks, especially gneiss and other crystalline schists, besides a certain amount of granite. The cause of the difference must thus be sought elsewhere. As SCROEDER VAN DER KOLK[1] has pointed out, the percentage of heavier minerals is largely dependent upon the amount of alteration to which the material has been subjected. Of all the minerals in the sand, quartz is the least liable to alteration, and the longer, therefore, the latter operates, the larger will be the quantities of quartz found. From this again, it follows that the greater the number of transportations the material has undergone, before it has reached the bottom of the sea, the richer will it be in quartz, as most of the geological processes upon the earth's surface, expose the material very considerably to the action of the atmosphere.

The difference between the Siberian and the West Greenland samples is explained quite naturally in accordance with the different geological conditions

[1] Bijdrage tot de karteering onzer zandgronden (I), 1895. Verh. der Koninklijke Akad. van Wetens. te Amsterdam (tweede sectie), deel IV, No. 4.

in the two places. In Greenland the loose deposits play a very small part. The coast almost everywhere consists of solid rocks, and nearly all the rivers flow upon rocky ground. In consequence of this, all the constituents come out into the ocean as nearly in their original condition as possible. Some of the heavier minerals will probably be altered, but their percentage in the sand of the sea-bottom will still be not much less than in the original rocks. A great part of Siberia, on the other hand, consists of sedimentary material, of which probably a large proportion has its origin at a great distance, and has undergone numerous transportations before reaching the ocean.

A comparison of these percentages with those known from other places is also extremely interesting. SCHROEDER VAN DER KOLK gives a very large number of measurements of sand-samples from Central Europe, and shows that in diluvial sand of Scandinavian origin there are percentages of heavier minerals that as a rule are higher than 0·5, while sand-deposits of more southern origin as a rule have smaller quantities. Thus the deposits at the north of Siberia answer with tolerable exactness to the first kind of diluvial sand, of which samples from Denmark, England, North Germany, and North Holland have been examined. As the deposits on shore from which the material of the ocean-floor comes, cannot have a smaller percentage of heavier minerals than this material itself, we thus have a means of comprehending the great extent to which the Silurian loose rocks have undergone transportation and subsequent deposition.

The differences between the percentages for the several samples are due in a great measure to purely local causes; the various localities obtain their material from widely different parts of the coast. The large percentage in No. 3 is characteristic, that sample being taken from a locality not very far from that of No. 6, which would lead us to expect that their sandy particles would be of a similar nature. The difference can arise from the fact that while the greater part of the constituents of No. 6 emanates from the coast of Siberia or from the neighbouring Kjellmanns Island, No. 3 has obtained its materials, at least partly, from Sverdrups Island. Different current conditions can also have played their part in the separation of the materials. The comparatively large percentage (2·45) in No. 15 is more immediately connected with the circumstance that this sample gets its material from the most eastern part of Siberia, where the country is rather rich in solid rocks; and it is there-

fore reasonable to suppose that a large proportion of the minerals are of primary origin.

The proportion between the various heavier minerals exhibits rather great variations in the various samples. The opaque particles are made up for the most part of magnetite, but some of them may also consist of the previously mentioned iron concretions. This is true also of the greater number of the class named "Aggregates, Concretions, etc." by which is meant grains that are only very slightly transparent, but which obviously do not consist of a single crystal. If the latter be the case, the particle is classed with the mineral in question. The iron concretions, as we have already said, are for the most part formed upon the sea-bottom itself, round some one or other very ferruginous mineral, most frequently magnetite; and thus the sum of the figures in the first two columns gives the approximate quantity of magnetite deposited in the sample. The various remaining samples do not differ much in this respect.

But the proportion of garnets is remarkable. Particularly large quantities are found in samples 7 and 9, which indicates that Asia's most northern peninsula consists very largely of garnetiferous rocks, (especially crystalline schists), some of which, in certain places, are found *in situ*. According to NANSEN the strand on Renø Isle one of the Kjellman group, was red with garnets, and the micaceous clay at the same place also contained them in abundance. As it appears impossible that samples 7 and 9 can have obtained their materials from such a source we must assume that there are also garnet-bearing rocks at other places farther eastwards. The strikingly small quantity of garnet in No. 15 can only imply that this mineral must be exceedingly poorly represented in eastern Siberia whence the sample in question obtained the greater part of its material.

The percentage of hornblende in the samples is very characteristic. No. 1 has quite a considerable quantity, but all the other samples along the Siberian coast have an infinitesimally small amount. As hornblende is generally one of the commonest minerals in quartz sand, and as it is also very characteristic and easily recognised in the preparations, it is difficult to imagine any other reason for this circumstance than that the great majority of rocks in northern Siberia must be almost destitute of hornblende, but on the other hand particularly rich in augite, as may be gathered from the generally considerable

quantity of this mineral found. Sample No. 15 again, contains rather more hornblende than the samples along the Siberian coast, and it may therefore be assumed that the rocks containing hornblende occur in rather greater quantities in east Siberia.

b. *The Lighter Particles.* The same method of treatment has been employed for these as for the heavier minerals, namely, that of counting 100 grains in a preparation, whereby the percentage of the various minerals is given directly. As the separation of quartz from felspar in sand preparations is exceedingly difficult, in some cases even impossible, I first attempted to separate these minerals by treating the sand with hydrofluoric acid. This acts upon both minerals, but in such a manner that the surface of the felspar becomes coated with a layer of insoluble fluoride, while the surface of the quartz particles remains quite clean. This difference only becomes visible when, after the sand has been washed, it is placed in a solution of fuchsine or some other strong dye. The surface of the grains of felspar then becomes coloured, and their number can be determined by counting. The advantage of this method is that all the different felspar minerals are separated from the quartz; but it is not particularly adaptable in practice, as in spite of all possible variations in the process of the experiment, it appeared that some of the grains became very imperfectly coloured, so that all possible transitions were found, and the number per cent could not be counted with very great exactitude.

I have consequently employed the difference in refraction to determine the quantity of felspar and quartz. While orthoclase, microcline, albite, and the plagioclases rich in soda, have a weaker refraction than Canada balsam, that of quartz, anorthite, and the plagioclases rich in lime, is stronger. By this means only, some of the felspars can be separated; but the last named plagioclases will certainly occur in very small quantities in materials originating from very quartziferous rocks. In most of the grains they will also be recognised by the oscillatory twinning, as there is a greater probability of their lying vertically than horizontally on the slides. I have not found, however, in any one of the samples, a striated grain that had not a weaker refraction than Canada balsam; and we may therefore conclude from this that these plagioclases must be exceedingly rare.

The actual difference in refraction is observed with extreme ease and distinctness by the method recently described by SCHROEDER VAN DER KOLK[1], namely, the employment of oblique transmitted light. If a shade be pushed under the slide until it reaches the middle of the grain of sand, the latter appears to have a bright edge on the side turned towards the shade, and a dark edge on the opposite side if the refraction of the grain is greater than that of the surrounding medium; if it is less, the two sides are reversed. In practice this kind of illumination is effected most simply by inserting a ruler in the space between the mirror and the slide, and at the same time endeavouring to hold the ruler as near to the latter as possible, or otherwise the opposite condition of light in the grains may supervene. In this manner a considerable area of the preparation may be rapidly inspected, and the separate grains of felspar are then easily recognisable at the moment they come in contact with the edge of the shadowed part. If the grains are then examined between crossed nicols, a large proportion of them will show distinct oscillatory twinning, while the remainder will not show the slightest difference from the quartz grains, and will here be designated orthoclase. Under this heading, however, are classed all albite and oligoclase particles that have the twin lamellæ lying horizontally, and also grains of these minerals, that only contain a single individual. They will certainly not occur, however, in any very great quantity.

Table of the Percentage of Minerals in the Lighter Parts of the Sand (specific gravity less than 2·85).

No. of Sample.	Concretions, etc.	Quartz.	Feldspar.		
			Orthoclase.	Microcline.	Plagioclase.
1	18	72	5	4	1
3	6	82	6	5	1
6	15	77	7	1	
7	39	51	5	3	2
9	9	71	14	6	
15	9	75	7	6	3

[1] "Bijdrage tot de mineraaldeterminatie", Handelingen van het 7e Natuur- en Geneeskundig Congres.

In the first column are placed all the grains that did not consist of one or a few recognisable individuals. Under this head come a number of different minerals and rocks that cannot be very clearly distinguished from one another. The greater part of the whole class is made up of iron concretions, which, unlike those found among the heavier particles, cannot be very strongly impregnated with iron. There is also every possible transition to particles that consist exclusively of coagulated, hardened clay. The grains in this column are to a smaller extent made up of fine-grained rocks such as sandstone, porphery, etc. In the samples that contain particularly large quantities in this column, by far the greater number are grey clayey particles, *e. g.* No. 7, where the quantity is so great that the sand acquires a peculiarly impure appearance. The proportion between the clayey matter and the other mineral particles has been previously discussed as regards each separate sample, and will not be further commented on.

Respecting the proportion between felspar and quartz, the various samples exhibit certain differences which may arise partly from the kind of rock from which the sample had its origin, and partly from disintegration, which will always greatly affect the felspar, and thus lower the percentage in proportion to that of the quartz. The proportion of felspar is smallest in No. 46, where the quantity is about $\frac{1}{10}$ that of the quartz, and rises with every possible transition to No. 9 where the proportion is 2 : 7. For the rest it is impossible to say anything more definite as to the causes of these variations, as long as nothing more definite is known about the geological conditions in the places whence the samples come.

The same may be said of the proportion between the various kinds of felspar. On an average, about half of them are orthoclase. No. 6, however, shows a remarkable exception to this, 7 out of 8 particles being orthoclase. This sample must thus have originated from rocks that are essentially different from the remainder. The quantity of plagioclase is always far below that of the other kinds of feldspar, which corresponds with the general proportion in granites and crystalline schists.

IV.

ORGANIC CONSTITUENTS OF THE SAMPLES.

We still have to mention the organic particles in the samples. The exceedingly unimportant part which these play has already been pointed out in the introduction. In the samples taken nearest the coasts, there is hardly any trace of organisms, and even in the deep-sea deposits there are strikingly few, as compared with corresponding deposits from other oceans. The reason of this is of course, that the greater part of the sea to the north of Siberia is covered with ice all the year round, and therefore the conditions for the development of plants or animals at the surface, are as bad as they possibly can be[1]. This will appear more clearly in the following pages, in which the quantities of the various organisms in the samples, will be briefly reviewed. There is no question of the determination of the various species, as that is beyond the scope of this paper. All that can be done here is to establish the importance of the organisms as rock-forming constituents, in order to have a basis of comparison between these deposits and other corresponding deposits from early and late periods.

Remains of *Algæ* are found in certain of the samples, always in extremely small and indeterminable fragments. They can only be found in the rapidly-formed deposits nearest land, as in others they would be destroyed in the course of time. As a rule they are found in very small quantities, a single piece, or a very few pieces, being found among the coarser constituents of most of the shore deposits; the greatest quantity is found in No. 1, in which, however, they can only be estimated at 10 per cent of the coarser

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[1] For the reasons of this, see NANSEN's account in the 3rd vol. of the present work.

constituents, or 0·14 per cent of the whole sample. As will be pointed out below, the contents of a part of the samples taken from the surface of the ice, include appreciable quantities of Algæ; but even on the assumption that a portion of this material will be able during the summer to thaw out and settle on the bottom, it is nevertheless probable as before mentioned, that all organic matter will be soon decomposed in the more slowly formed samples, and algæ are therefore not found in any one of them.

Siliceous Organisms have not been observed in any of the bottom-samples of the *Fram Expedition.* This is very remarkable, as sponge spicules and diatoms in particular, are generally found in very great quantities, and are scarcely ever absent, even from the samples formed nearest land. If found at all, it must be in exceedingly small quantities, as a considerable number of all sizes of particles have been examined under the microscope, and these organisms are always very conspicuous. Even in the small area of open water found in these regions, diatoms must be very feebly represented; and the entire ocean-floor where the samples were taken must be nearly wholly destitute of siliceous sponges.

Fragments of *Mollusc Shells* are found in several of the samples among the coarser particles, but never more than 1 or 2 very insignificant pieces, except in No. 6, where they occur in somewhat greater numbers, though only in indeterminable fragments.

Foraminifera, which are generally particularly characteristic of bottom-deposits, play a very unimportant part in these samples, although they occur in considerably greater quantities than any of the other organisms. There are only 2 specimens of the larger forms — more than 0·5 mm. — in the samples, viz. a rotaliform foraminifer in No. 6, and a Haplophragmium in No. 6. As usual, by far the most numerous of the smaller forms are Globigerinæ, with a very few rotaliform foraminifera; it is only in the samples from near the shore — and this is also the case elsewhere — that the last named may be in the majority, or even the sole occupants. The smaller forms of Foraminifera were found in the following samples:

No. 2, a few rotaliform Foraminifera

No. 7, a single Globigerina

No. 9, some Foraminifera, chiefly Globigerinæ

No. 13a, a few Foraminifera, exclusively Globigerinæ

No. 14, about 10 per cent of the sand (0·57 per cent of the entire sample) was made up of Foraminifera, almost exclusively Globigerinæ. The difference of their quantity in this and the preceding sample is not easy to account for, as they were taken in the immediate vicinity of one another, and are very similar in all other respects.

No. 15, about 40 per cent of the sand (3·83 per cent of the entire sample) was made up of Foraminifera, almost exclusively Globigerinæ. This is the largest amount that has been found in any of the samples. There was too little of No. 16, which lay considerably farther to the NW, for examination; but the chemical analysis shows that this sample can contain only very few Formanifera. The reason of this is probably to be found in its situation in rather close proximity to Franz Josef Land, of which the consequence is a large deposit of terrigenous matter. If there is any part of the sea north of Siberia where one might expect to find samples even richer in Foraminifera, it must be the still unknown regions north-east of No. 15. It can hardly be expected, however, that a percentage will be found anywhere so relatively large, as to permit of the sample being designated Globigerina Ooze.

Coccoliths were found only in No. 15, and then only after a very long search, and in very small numbers. They are so unbroken, however, that it is not probable that they originate from early deposits in Siberia. As they occur in such small numbers, it is not certain that they come from coccospheres that have lived in the Arctic Ocean itself; but they may possibly have been brought all the way from the Atlantic Ocean by the previously mentioned, warm under-current. In size and shape they exactly correspond with the species found in the North Atlantic, described by OSTENFELD[1] under the name *Coccosphœra atlantica.*

It will be sufficiently evident from the above, that the samples here discussed contain, as has been already pointed out, exceedingly few organic constituents, and therefore, in this way, occupy a somewhat unique position among sea-bottom deposits. When the organisms are not found in larger quantities in bottom-samples, the deeper of which at least must be assumed

[1] 'Ueber Coccosphaera und einige neue Tintinniden im Plankton des nördlichen Atlantischen Oceans.' Zool. Anzeiger, Vol. XXII, No. 601, 1899; and 'Ueber Coccosphaera.' Ibid, Vol. XXIII, No. 612, 1900.

to have been formed exceedingly slowly, the animal and plant life in the sea in question must have reached a minimum as regards the siliceous and calcareous shell organisms. It has also already been shown that this accords well with the physical conditions in these regions, especially with their being covered so extensively with ice.

SAMPLES TAKEN FROM THE SURFACE OF THE ICE.

It is convenient to give an account here of 5 samples taken from the surface of the Ice between April and June 1894, in a region NW of the New Siberian Islands about 80—83 N by 120—135 E. In most respects they show close resemblance with one another as also with the finer bottom samples.

An investigation of these surface samples promised, it might be expected, less of interest than the bottom samples themselves; the constitution of the latter is in every case intimately related to the physical conditions prevailing round about the district concerned. And indeed since the composition and constitution of the bottom is very largely a direct result of the co-operation of influences of this kind, it is from a suitable investigation of samples, possible to draw many valuable conclusions as to these influences. On the other hand, a sample taken from the Ice might conceivably have originated entirely from one locality or have come, indeed, direct from land; in any case its origin and destination is very largely a matter of mere accident. The samples here under consideration probably came from the coast, broken loose by wave action from the shore; or material may have fallen from the land out on to the ice or have been brought down by rivers, already occluded in the frozen surface. The constitution of a sample depends upon the nature of the rocks in the neighbourhood from which it came; it may evidently be very variable. The fact that all the samples here described show a very markedly similar composition can only be explained on the basis that all have the same origin. It is true they were taken at places far apart, but it must at the same time

be borne in mind that the ice in this region was travelling in the same direction as the Fram and at about the same rate, which would make it possible that all the samples were taken from a relatively limited area of ice, and it may well have been, from one and the same locality. In this district one must always remember there is a strong probability that earthy matter once in the ice, may be carried a great distance from land.

The importance of such samples as evidence bearing on the agencies at work in forming the sea bottom may in other cases be very great indeed; but in connection with the bottom-samples of this expedition they are of only very insignificant, or no importance. For practically the whole year the ice is one continuous, massive sheet all the while growing in thickness through the freezing going on at its lower surface; there seems small likelihood that the ocean bottom can receive appreciable increment from this source. In some places however, notably in the Atlantic ocean where the ice is continually melting, this factor may become of importance, and an investigation of samples bearing on this fact might be of great interest.

The 5 samples are rather fine, as will be seen from the analyses here given. In all cases the colour is practically pure grey, as would be expected, since a brown colour is, as already mentioned, only developed after the lapse of some considerable time on the sea bottom. They are rather friable; most of the particles seem to wash out with the fine constituents, but it is difficult to draw definite conclusions from this kind of circumstance because as already stated, it may have happened that all have come from the same place.

The following table gives the results of a mechanical analysis of the samples.

No. of sample	Date 1894	Constituents			
		Less than 0·01 mm.	0·01–0·05 mm.	0·05–0·5 mm.	More than 0·5 mm.
I	April	57·5	33·9	8·5	0·1
II	May 24th	62·3	34·6	3·1	0
III	June 2nd	57·3	41·3	1·4	0
IV	June 4th	59·9	34·8	5·3	0
V	June 17th	52·3	46·9	0·8	0

A comparison of these numbers with those of the bottom samples shows that these are on the whole finer than the four samples of grey deep water deposits, which on an average contained 6·03 % between 0·05 and 0·5 mm. and 0·6 % between 0·5 and 1·0 mm. The finest of these samples (Nrs. III and V) are markedly finer than the finest of those from the bottom (No. 10 c.); it contains on the whole extremely little sandy material among the clay particles, which fact places it in this respect, nearest to the plastic clays of the tertiary period. It is however scarcely legitimate to infer that it has actually originated from a formation of that age, since it would probably not be difficult to find among alluvial clays some of similar character. In any case it seems unlikely that there can have been any washing out of the clay, during the time it was being transported from its original starting point out on to the ice.

The mineralogical composition was not investigated in detail; the small proportion of particles over 0·5 mm. in Nr. I was found to consist almost always of Algae in fragments, and in some cases of Foraminifera which were very probably occluded by the ice during a period when open water conditions obtained. There were a few particles of conglomerated clay and very few quartz grains. The sandy constituents were almost always wholly quartz; the quantity is however so small that it is not possible to say whether a separation of the constituents could be accomplished by treatment with fluids of different densities.

APPENDIX I

to Memoir No. XIV.

ANALYSES OF THE BOTTOM DEPOSITS FROM THE NORTH POLAR BASIN.

By Dr. O. N. Heidenreich and Dr. Charles J. J. Fox.

ANALYSES OF BOTTOM DEPOSITS FROM NORTH POLAR BASIN.

By Dr. O. N. Heidenreich.

Number of Sample	10 a.	10 c.	11.	12.	13 a.	14.	15.	16.	17.
Date	Oct. 4, 1893	Oct. 4, 1893	Oct. 21, 1893	Oct. 26, 1893	May 1, 1894	Aug. 6, 1894	Jan. 23, 1895	July 23, 1895	July 8, 1896[1]
N. Lat.	78° 42′	78° 42′	78° 19′	78° 31′	80° 46′	81° 8′	83° 24′	84° 32′	83° 2′
E. Long	135° 40′	135° 40′	135° 45′	136° 10′	131° 18′	127° 32′	102° 14′	72° 20′	12° 52′
Depth	1460 m.	1460 m.	135 m.	100 m.	3800 m.	3850 m.	3450 m.	3700 m.	3400 m.
Loss on Ignition	6·41 %	6·40 %	4·75 %	5·43 %	5·81 %	5·81 %	5·03 %	8·24 %	5·58 %
Soluble in 20 % hot HCl: SiO_2	0·07 -	0·11 -	0·11 -	0·11 -	0·18 -	0·60 -	0·10 -	0·10 -	0·11 -
TiO_2	0·00 -	0·00 -	0·00 -	0·00 -	not det.	0·00 -	0·00 -	not det.	0·00 -
Al_2O_3	4·01 -	4·68 -	4·48 -	4·25 -	5·30 -	4·74 -	4·50 -	5·08 -	4·65 -
Fe_2O_3	5·30 -	5·25 -	3·52 -	3·36 -	7·12 -	4·71 -	3·27 -	8·87 -	5·69 -
FeO	2·40 -	2·95 -	2·98 -	2·76 -		1·97 -	4·06 -		1·92 -
MnO	0·30 -	0·40 -	0·11 -	0·12 -	0·39 -	0·64 -	0·58 -	0·49 -	0·47 -
MnO_2	0·43 -	0·74 -		0·10 -		0·11 -	0·05 -		
CaO	0·79 -	0·74 -	0·35 -	0·64 -	0·73 -	0·85 -	2·61 -	1·12 -	1·61 -
MgO	1·62 -	2·17 -	1·69 -	1·77 -	1·78 -	1·21 -	1·40 -	2·61 -	1·87 -
K_2O	0·65 -	0·75 -	0·59 -	0·41 -	not determined	0·38 -	0·26 -	0·41 -	0·65 -
Na_2O	0·38 -	0·26 -	0·35 -	0·39 -		0·31 -	0·89 -	0·22 -	0·28 -
CO_2	0·41 -	0·37 -	0·26 -	0·51 -		0·77 -	2·20 -	not det.	1·31 -
P_2O_5	0·21 -	0·29 -	0·22 -	0·07 -		0·05 -	0·26 -	0·17 -	0·22 -
	22·98 -	*25·17* -	*19·41* -	*19·92* -		*22·15* -	*25·11* -	*27·31* -	*24·36* -
Insoluble in 20 % HCl: SiO_2	57·97 -	56·11 -	63·08 -	62·27 -	60·50 -	59·35 -	57·11 -	52·41 -	57·47 -
Al_2O_3	12·54 -	12·07 -	11·16 -	10·96 -	12·14 -	11·99 -	11·14 -	14·05 -	12·35 -
Fl_2O_3	1·87 -	1·50 -	1·45 -	1·53 -	1·53 -	1·29 -	1·78 -	2·07 -	1·27 -
FeO	0·10 -	0·17 -	0·20 -	0·13 -		0·09 -	0·19 -		0·11 -
CaO	0·50 -	0·55 -	0·56 -	0·18 -	1·05 -	0·43 -	0·95 -	0·76 -	0·41 -
MgO	0·64 -	0·88 -	0·21 -	0·10 -	0·36 -	0·58 -	0·29 -	0·61 -	0·27 -
K_2O	2·53 -	2·48 -	2·42 -	2·73 -	not determined	2·35 -	2·48 -	2·42 -	2·09 -
Na_2	1·59 -	1·47 -	1·93 -	2·42 -		1·92 -	1·07 -	1·34 -	1·45 -
	77.74 -	*75·23* -	*81·01* -	*80·32* -		*78·00* -	*75·61* -	*73·66* -	*75·72* -
	100·72 %	100·40 %	100·42 %	100·24 %		100·15 %	100·71 %	100·97 %	100·08 %

[1] The Date and Locality was not indicated on the label of this sample, but there is but little doubt that the above date is the correct one. *F. N.*

ANALYSES OF BOTTOM DEPOSIT.

By Dr. Charles J. J. Fox.

Prof. Nansen brought me what was left over of Samples 10, 10a, 10c, with the request that I would carry out a few analyses just to obtain some idea of the order of accuracy of the methods as employed above by Dr. Heidenreich. Sample No. 10 was in three portions, one being distinctly brownish another brownish grey and the third one grey. They are referred to here as 10^1 10^2 10^3 respectively. The samples as brought to me were from 6—8 gms. each, and therefore really too small to allow of as complete an investigation of this obviously important point, as would have been desirable.

After due consideration it was decided to estimate in each sample, the Ferrous and Ferric Oxides and in addition, in the cases of samples Nos. 10a and 10c the Lime and Carbonic Acid. These seemed to be the most important constituents and after a short preliminary investigation, it also appeared likely that they could be estimated even on such small samples, with very fair accuracy.

Some of the samples were in alcohol and some were dry; all were powdered finely and dried in a dessicator for a few days. It then appeared that they were slightly hygroscopic and the weighings had to be done from a closed weighing bottle. Of each sample two quantities of about 2 gms. each were weighed out; the one sample was used for a determination of the Iron (ferrous and ferric) and the Lime, and the other for the Carbonic Acid. The former sample was boiled for some time in an air-free flask with 30 cc. of 20 % HCl. After allowing to settle, the liquid was rapidly decanted, the residue washed thoroughly and the washings added to the bulk of the liquid. The whole was much diluted and then titrated with Potassium Permanganate to get the amount of Ferrous iron; pure zinc was added and the whole

of the iron reduced to the Ferrous state and again determined by the same method. These titrations furnished the data for calculating both the Ferrous and the Ferric Oxides. To the solution obtained after these operations Ammonium Chloride, Ammonia and a slight excess of Ammonium sulphide were added. The precipitated Fe., Mn. etc., was then filterred off, the filtrate warmed and an excess of solid Ammonium Oxalate added to precipitate the Calcium. The Calcium Oxalate was filtered off, dissolved in HCl and reprecipitated with Ammonia, to remove traces of Magnesium possibly carried down with it. Finally it was transferred to a Berlin porcelain crucible, carefully heated and weighed as Carbonate; it was then treated with a drop of Ammonium Carbonate solution, gently warmed and reweighed, the operation being repeated until a constant weight was obtained. In this way it was made quite certain that the Calcium was quantitatively in the form of Carbonate.

It was feared that the determination of the Iron might not be quite exact by this method, owing to partial oxidation of the HCl, even though rather dilute, by the Permanganate[1]. To make quite sure of this, a similar quantily of the original sample was treated in the same way as before but with 30 % *Sulphuric Acid.* instead of Hydrochloric Acid. and the estimation of the Iron repeated as a control. It will be seen that the agreement of the two methods is satisfactory; though a lack of agreement might possibly not be regarded as unsatisfactory, inasmuch as there is no reason to suppose that the two methods would furnish the same results in the case of every clay.

The Carbonic Acid was determined gasometrically; the apparatus used, which has been specially designed for determining the gases dissolved in sea water[2], allowed the clay to be introduced, boiled with acid and the volume of the Carbonic acid evolved, to be estimated with an accuracy of a few hundredths of a cc. — an order of accuracy higher than here required.

[1] This is the method as used by TORNØE and subserquently by almost all who have carried out thiskind of analysis. It appears to be rather reliable as the analyses here recorded also appear to show, but according to my experience it is better to do the titration with Permanganate after adding a solution of Manganese Sulphate and Phosphoric acid to the Iron solution. The end point is then much more permanent as well as sharper. (See CLASSEN, Ausgewählte Methoden der Analytischen Chemie. Braunschweig 1901. Vol. 1. p. 455).

[2] See CHARLES J. J. FOX, On the Determination of the Atmospheric Gases dissolved in sea-water. Publications de Circonstance No. 21 du Conseil permanent international pour l'exploration de la mer, Copenhague 1905.

	FeO		Fe_2O_3		CaO	CO_2
	HCl sol.	H_2SO_4 sol.	HCl sol.	H_2SO_4 sol.		
10[1]	2·63	2·55	5·68	5·53	—	—
10[2]	3·20	3·06	5·66	5·49	—	—
10[3]	3·49	3·58	5·11	5·15	—	—
10a	3·21 HEIDENREICH 2·40	3·06 —	5·60 HEIDENREICH 5·30	5·11 —	·82 HEIDENREICH ·78	·40 ·41
10c	2·88 HEIDENREICH 2·95	2·79 —	5·35 HEIDENREICH 5·25	5·50 —	·79 ·74	·40 ·37

The results of these few analyses appear to agree very well under the circumstances with those obtained by HEIDENREICH, except in the case of one Iron estimation; and it may be assumed that the methods adopted give quite reliable results. It is commonly believed that in a clay deposit graduating from brown to grey through grey-brown and brown-grey, the ratio of Ferrous to Ferric oxide found, should alter simultaneously with the change of colour — the Ferrous oxide giving to the clay its grey-green and the Ferric oxide its brown colouration. And this may on the whole be said to hold in the case of the samples here under consideration, with possibly just one exception. This exception however, happens to coincide with the above mentioned discrepancy in the two Iron estimations of HEIDENREICH and myself. As will be seen from HEIDENREICH's analysis, a greyish sample appears to have a higher percentage of Ferric iron than a brown layer of the same clay; my analysis on the other hand seems to indicate the exact contrary. In view of the fact that the samples have been all used up, it is now impossible to decide which is right for certain, but inasmuch as the former result appears unlikely and the latter what might be expected, it seems at all events safe to say that so for as is known a brown layer probably does contain more Ferric iron than a grey layer of the same clay.

Kristiania, May 27th 1905.

APPENDIX II

to Memoir No. XIV.

THALAMOPHORA FROM THE BOTTOM SAMPLES AND THE MUD FROM THE SURFACE OF THE ICE IN THE NORTH POLAR SEA.

By Hans Kiær.

In the following account of the Thalamophora from Prof. Nansens bottom samples, r — indicates rare, c — common, v. c. — very common.

No. 1.

Juli 24, 1893. 10 p. m. 130 m.
71° 17′ N. Lat.; 48° 22′ E. Long.

Truncatulina ungeriana (r).

No. 2.

Aug. 17, 1893. 12 midnight. 46 m.
75° 10′ N. Lat.; 80° 31′ E. Long.

Polystomella sp. (r).

No. 3.

Aug. 18, 1893. 9 a. m., 26 m.
74° 38′ N. Lat.; 80° 15′ E. Long.

No Thalamophora.

No. 4.

Aug. 18, 1893. 12 noon and 2 p. m., 40 m.
74° 29′ N. Lat.; 80° 32′ E. Long.

No Thalamophora.

No. 5.

Aug. 18, 1893. 10 p. m. 39 m.
73° 51′ N. Lat.; 80° 44′ E. Long.

Polystomella sp. (r).
Haplophsagmium nanum (r).

No. 6.

West of the Kjelman Islands.

Polystomella artica (r).

No. 7.

West of Taymyrsound.

Polystomella artica (r).
Spiroplecta biformis (r).
Pulvinulina Karstenii (r).

No. 8.

Sept. 13, 1893. 12 noon. 20 m.
74° 55′ N. Lat.; 116° 40′ E. Long.

Polystomella arctica (r).
„ *subnodosa* (r).
„ *striatopunctata.*
var. *incerta* (r).

No. 9.

Sept. 13, 1893. 8·30 p. m.
Off. Anabara. 40 m.

No Thalamophora.

No. 10.
Oct. 4, 1893. 1460 m.
28° 42′ N. Lat.; 135° 40′ E. lang.

The upper layer (No. 10a).

No Thalamophora.

Between upper and under layer (No. 10b).

No Thalamophora.

The under layer (No. 10c).

Reophax difflugii formis (r).
Nodulina sp., young specimens (r).

No. 11.
Oct. 21, 1893. 135 m.
78° 19′ N. Lat.; 135° 45′ E. Long.

No Thalamophora.

No. 14.
Aug. 6, 1894. 3850 m.
81° 8′ N. Lat.; 127° 32′ E. Long.

The upper layer.

Gloligerina bulloides (v. c.).
„ *pachyderma* (v. c.).
Reophax difflugii formes (r).
Discorbina arancana (r).
Pullenia quinqueloba (r).

No. 15.
Aug. 7, 1894. 3850 m.
81° 7·6′ N. Lat.; 127° 27′ E. Long.

The upper layer.

Globigerina bulloides (v. c.).
„ *pachyderma* (v. c.).
Quinqueloculina seminulum (r).
Polystomella arctica (r).

No. 16.
Jan. 23, 1895. 3450 m.
83° 24′ N. Lat.; 102° 14′ E. Long.

Globigerina bulloides (v. c.).
„ *pachyderma* (v. c.).
Pullenia sphaeroides (c).
„ *quinqueloba* (c).
Lagena orbignyana (r).
Botivina punctata (r).
Spiroplecta biformis (r).
Discorlina berthelothiana (r).
„ *arancana* (r).
Quinqueloculina seminulum (c).

Sample from the surface of the ice.

Nodulina scorpiura (r).
Cassidulina crassa (r).
Polymorphina lactea (r).
Nonionina scapha var. *labradorica* (r).

Globigerina bulloides.

Exists in great quantities and varying size in the great depths of the Polar sea. The larger specimens are always thick-shelled, while the smaller have generally thinner shells. The size of the shells is variable, from 0·1—0·3 mm. Only a few small 2—4 chambered shells were found (size: 0·03—0·06 mm.).

Globigerina pachyderma.

Very common, as in the case of the preceding species. Size: 0·1—0·38 mm. There also occur some small, starved, thick-shelled specimens.

Between bulloides and pachyderma numerous transition forms were observed.

The *Globigerina* shells seem to have been dead for a long time. They are either empty or filled with a brown granular mass, which absorbs dye-stuffs badly[1]. This mass is certainly mud. The thickness of the shell-wall is 0·012—0·035 mm.

The other Thalamophora, from the brown clay are true bottom forms. The most of them are small, starved deep water forms, only three species, *Bolivina punctata, Spiroplecta biformis* and *Polystomella arctica* are as a rule distributed in comparatively shallow water.

The size of	*Pullenia sphaeroides*	is 0·07 mm.
„ „ „	„ *quinqueloba*	„ 0·07—0·16 mm.
„ „ „	*Lagena orbignyana*	„ 0·2 mm.
„ „ „	*Bolivina punctata*	„ 0·1 „
„ „ „	*Spiroplecta biformis*	„ 0·14 „
„ „ „	*Discorbina berthelothiana*	„ 0·042—0·072 mm.
„ „ „	„ *arancana*	„ 0·14 mm.
„ „ „	*Quinqueloculina seminulum*	„ 0·3 „

With respect to the occurrence of Thalamophora it may be said that the mud from the depths of the North Polar Basin is very much like the Biloculina clay in the Norwegian Sea, although it lacks the Biloculina shells. But the other Thalamophora occur in like number and appearence just as in the Biloculina clay. This is aspecially true of *Globigerina* as also of the true bottom forms such as *Quinqueloculina seminulum, Pullenia* and *Discorbina* spp. etc.

The bottom forms are proportionally rare and they probably originate from mud, which the ice constantly carries from the coasts of the Polar Sea. The occurrence of salt water Thalamophora in the mud from the surface of the polar ice, shows that the mud belongs to recent or post-tertiary marine deposits.

The mud from 1600 metres lacks Thalamophora. Only in the under layer were some small arenaceous shells found, of which one specimen, *Reophax difflugii formis*, is distributed in deep water deposits from Franz

[1] The majority of the bottom samples were preserved in spirit.

Josephs land to the Antarctic Ocean. The Thalamophora from the shallow water along the Siberian coast are pure arctic and are only mixed with a few boreal forms.

In the brown clay from 3850 metres one fragment of a *Radiolaria* was seen and in the grey clay from the surface of the ice and from a depth of 39 metres at the Siberian coast, some Diatoms (*Coscinodiscus* sp.). In the mud from the ice were also found some badly preserved and indeterminable vegetable substances, probably grains or fragments of moss. Prof. WILLE and cand. HOLMBOE have assisted me with the examination of the vegetable fragments and Diatoms, for which ready assistance I would here express my thanks.

Empty shells of shallow-water molluscs were found scattered over the deep parts of the Arctic Ocean by the Norwegian North Atlantic Expedition 1876—1878.

Dr. A. S. JENSEN has attempted to explain this phenomenon as connected with a subsidence of the sea-bottom[1]. The remains of shallow-water animals found there, would thus be fossil remains.

FRIELE and GRIEG on the other hand think, that the shell-fragments must have been carried out into deep water by the ice[2].

Certainly it would be interesting, in order to obtain a satisfactory explanation of the phenomenon, to examine the distribution of Thalamophora in the nothern oceans.

Rhabdammina abyssorum is very common in the Arctic Ocean between Norway, Spitsbergen and Novaja Zemlja, and also in many of the Norwegian Fjords of depths of 100—600 metres (in the Sogne Fjord to 1200 metres). Only in the eastern portions of the *Biloculina* mud were some few fragments found to a depth of 2000 metres.

On the other hand *Rhabdammina abyssorum* does not exist in shallaw-water, but the occurrence of the species as sub-fossil in some parts of our Fjords especially the Kristiania Fjord[3] proves its comparatively great age.

Operculina ammonoides is very common along the coast of Norway to Vadsø, and is never found at greater depths than 624 metres, except in one

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[1] Om levninger af grundtvandsdyr . . . . Vidensk. Meddel., 1900, p. 229.
[2] The Norwegian North Atlantic Expedition 1876—1878, vol. XXVIII, p. VIII.
[3] H. KIÆR, Synopsis Norw. Mar. Thal. p. 55.

locality, the Sogne Fjord, where it was found at a depth of 1036 metres. The species occurs in the post-tertiary deposits of Norway.

Uvigerina pygmaea and *Biloculina simplex* have similar distribution.

Operculina ammonoides is not found north of Norway.

Not a specimen nor a fragment of these three *Thalamophora* were found in the Norwegian North Atlantic Ocean.

If this Ocean was shallow in Quarternary times, we should now expect *Rhabdammina abyssorum* and the three other species to be common there.

On the other hand *Thalamophora* occurs in mud taken from the ice by NANSEN's North Polar Expedition and also in mud from the ice in the Denmark Strait, collected by NANSEN in 1882.

In the mud from the surface of the ice from Denmark Strait, kindly sent me for examination by Prof. NANSEN, very few *Thalamophora* were to be seen, in fact only three specimens of *Trochammina inflata*. The other sample of mud from the ice contained proportionally[1] many more Thalamophora-organisms and altogether it seems probable that an examination of mud from different parts of the drifting ice would show the presence of a great many *Thalamophora*.

[1] Of the mud from the ice from Denmark Strait Prof. NANSEN sent me $^1/_4$ of a litre, which was treated by Dr. MADSEN's method (V. MADSEN, Istidens Foraminiferer i Danmark og Holsten. Copenhagen 1895). The other sample was not very large.

ERRATA.

Page 8. Table, 2nd column, line 17, *read* 1894. May 1.

Page 9. Table, last column, *for* 7a, 7b, and 7c *read* 10a, 10b, and 10c, *for* 10b *read* 13b.

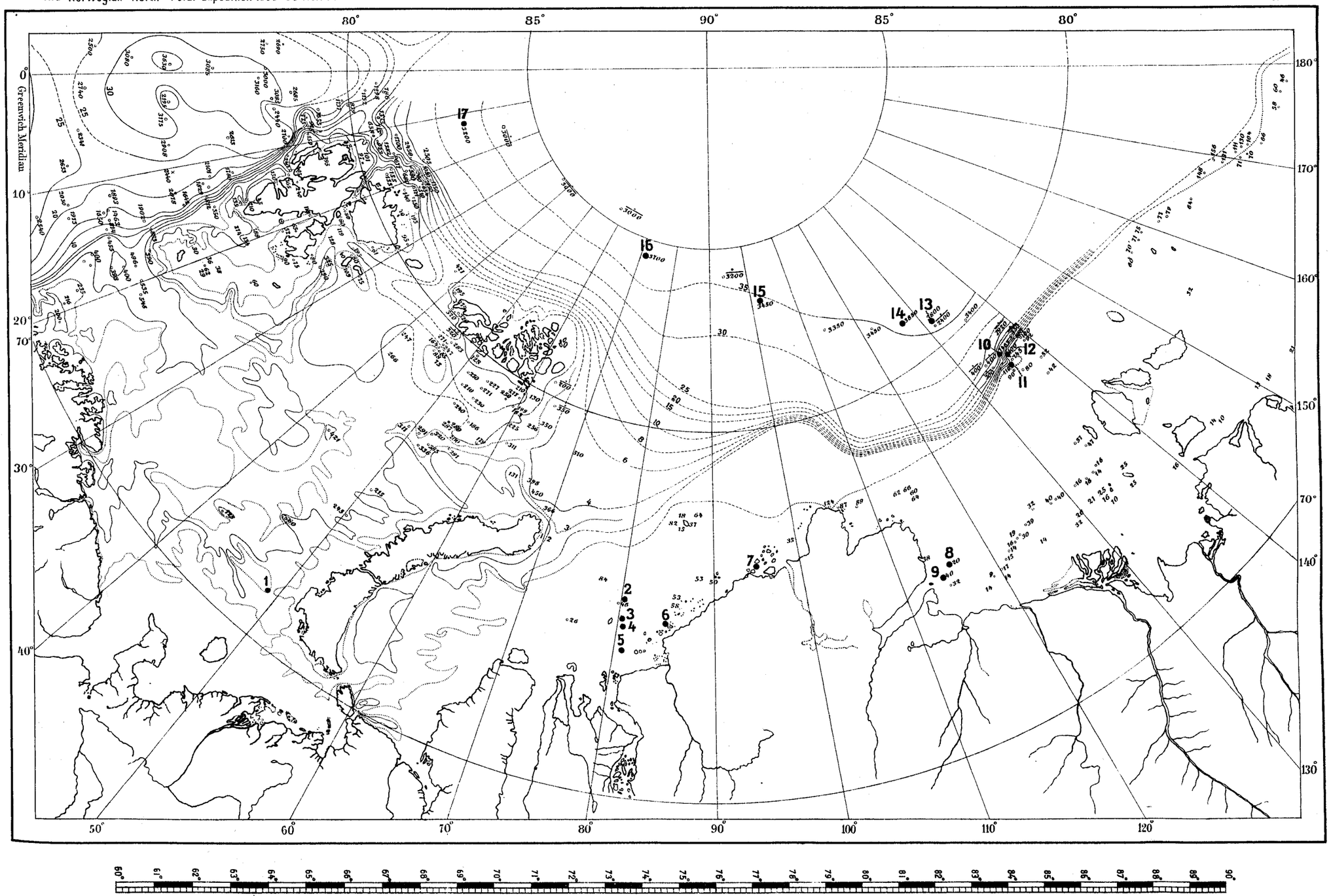
Page 14, line 8 *for* No. 12 *read* No. 15.

EXPLANATION OF THE PLATES.

Pl. I. The map of the North Polar Sea gives the position of Stations where bottom-samples were taken during the Expedition.

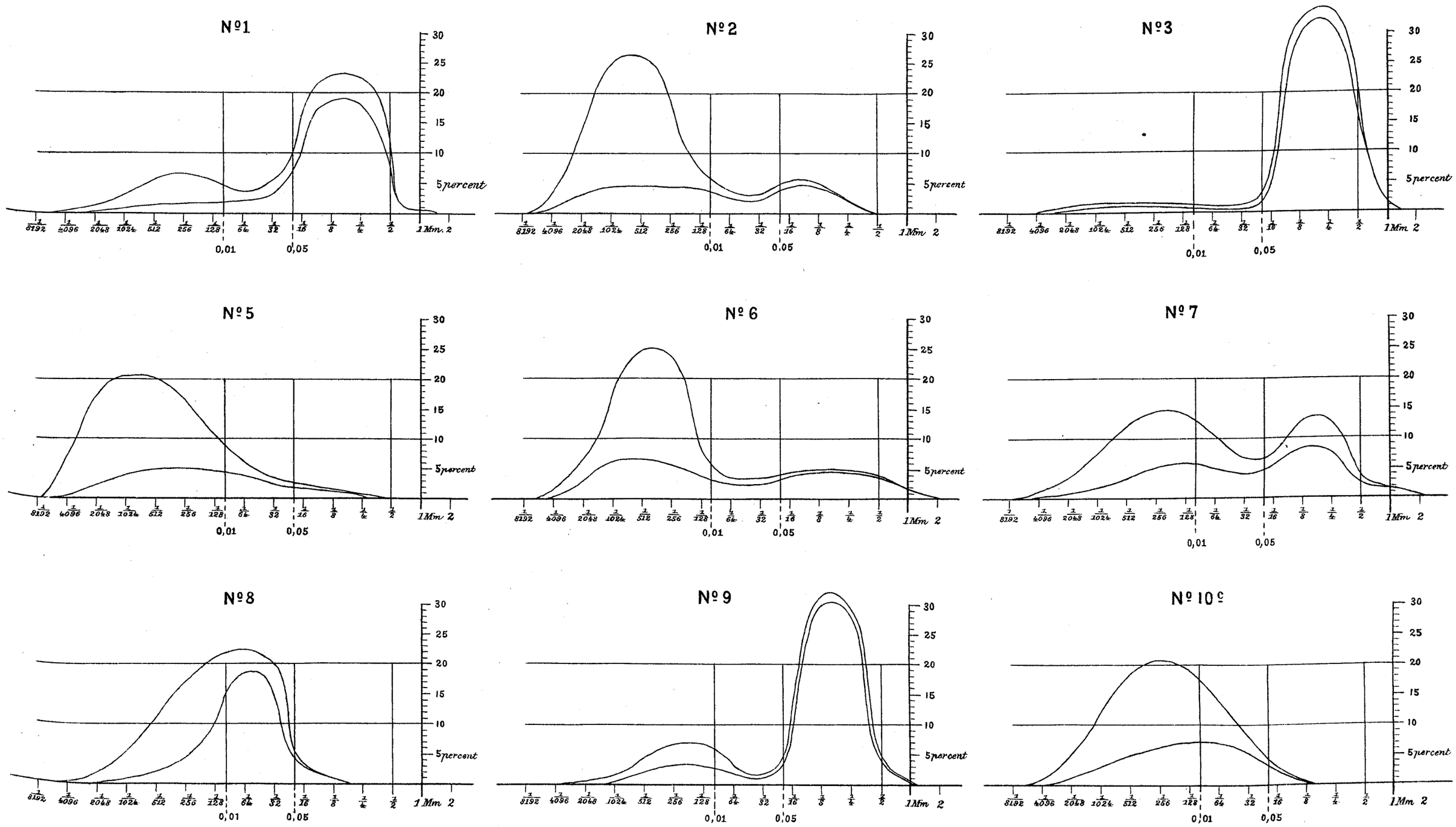
Pls. II & III. The Curves illustrate the quantities and different sizes of the grains in the samples of bottom-deposits. The area delimited by the upper Curve represents the whole of the sample. The lower curve represents the actual mineral grains (mostly Quartz). The area between the two curves therefore represents the Clay.

For further explanation see also pp. 22—27.



Map of the North Polar Sea, *showing Stations (•) where Bottom-Samples have been taken.*
The figures (2) *at the stations indicate the numbers of the Bottom-Samples (see Table p. 8)*

Kart-og Litogr. kontoret, Christiania.


N°1
N°2
N°3
N° 5
N° 6
N° 7
N°8
N° 9
N° 10c
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1/8192 1/4096 1/2048 1/1024 1/512 1/256 1/128 1/64 1/32 1/16 1/8 1/4 1/2 1 Mm 2
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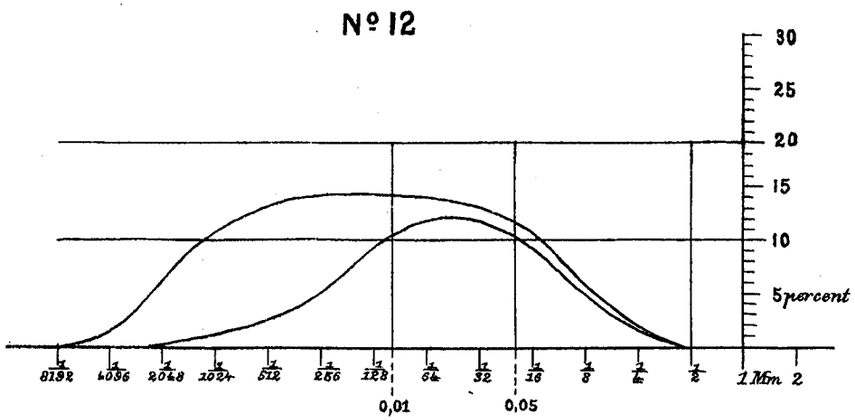
Nº 12
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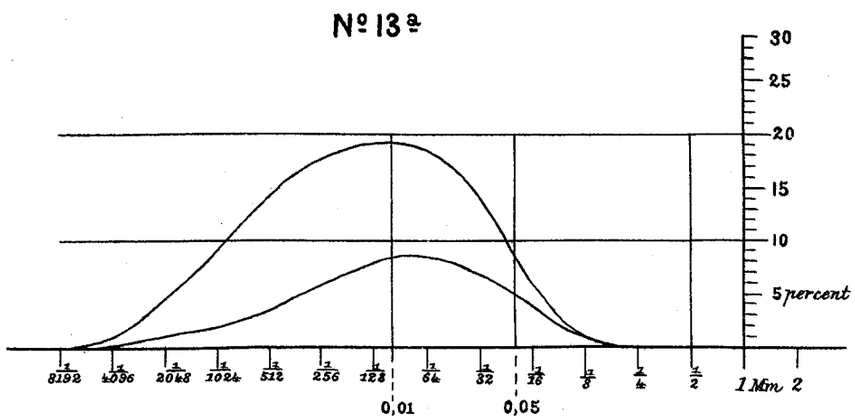
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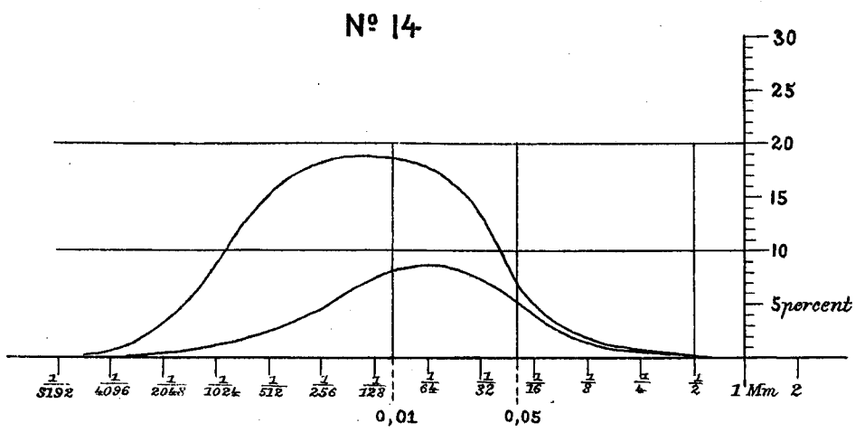
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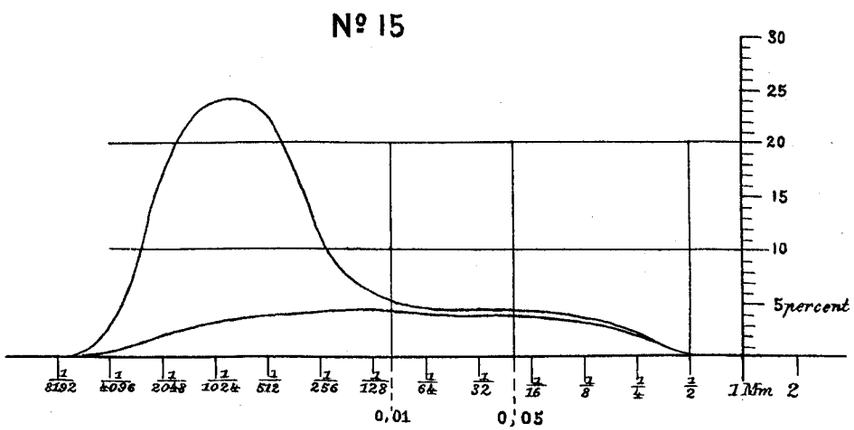
Nº 15
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XV.

ON DEAD-WATER:

being a description of the so-called phenomenon often hindering the headway and navigation of ships in Norwegian Fjords and elsewhere, and an experimental investigation of its causes etc.

BY

V. WALFRID EKMAN

WITH A PREFACE BY

PROFESSOR VILHELM BJERKNES.

PREFACE.

The present investigation of "Dead-water" was occasioned by a letter in November 1898 from Prof. NANSEN asking my opinion on the subject. In my reply to Prof. NANSEN I remarked that in the case of a layer of fresh water resting on the top of salt water, a ship will not only produce the ordinary visible waves at the boundary between the water and the air, but will also generate invisible waves in the salt-water fresh-water boundary below; I suggested that the great resistance experienced by the ship was due to the work done in generating these invisible waves. In addition to some inaccuracies — at that time I was not intimately acquainted with the theory of water waves — my letter further suggested a plausible explanation of the suddeness with which the resistance ceased: when the ship's speed has become so great that the waves in the boundary below cannot follow her, the work done in sustaining them will be saved.

The matter was subsequently discussed once or twice when Prof. NANSEN and I occasionally met, and it was finally decided that I should have the subject rigorously examined by experiment. In December 1899 I consequently suggested to a pupil of mine at the Stockholm's Högskola, Dr. V. WALFRID EKMAN, now assistant in the Central Laboratory for the International Study of the Sea, Kristiania, that he should do some simple preliminary experiments. From the very beginning they were successful and furnished results confirmatory of the above mentioned assumptions.

Mr. EKMAN has since the beginning of the following year been carrying out the systematic investigation of the phenomenon, and the present paper embodies the results obtained independently by him since then, without any intervention from me.

STOCKHOLM, 1903.

V. BJERKNES.

INTRODUCTION.

Norwegian seamen often speak of a strange phenomenon which they call "*dödvand*" or *dead-water*, and which without any visible cause makes the vessel lose her speed and refuse to answer her helm. Their only definite knowledge of its origin is that it exists solely at places where the sea is covered by fresh or brackish water. The same phenomenon is known by seamen in other countries although to a much smaller extent; but most people have certainly never heard of it. The tales of dead-water have often been regarded as mere fictions of the imagination.

When the *Fram* made her way along the Siberian coast in the autumn of 1903, she three times met with dead-water off Taimur Island. This occurence made Prof. NANSEN very desirous of having the causes of the phenomenon cleared up, and it led, as mentioned in the Preface, to Prof. V. BJERKNES' explanation of the phenomenon and to the present investigation. If the objective reality of *dead-water* may now be acknowledged as an undeniable fact and if its causes are, in the main, known, they may certainly be reckoned among the scientific results of the first *Fram*-expedition.

In addition to the collective description of the dead-water, which introduces Chapter I, the original accounts are frequently inserted in detail, and for two reasons:

1) The narratives are not in every detail quite in accordance with one-another, and it was not possible always to decide in what degree the features of the phenomenon itself, are variable, or what descriptions are the most correct. They ought therefore to be accessible to the reader's criticism.

2) It was thought that the original accounts would best give a clear and complete picture of the phenomenon in its different aspects. Parts of Accounts Nos. 1, 5, 6, 8, 9, 10, 13 may be specially referred to in this respect.

In some of the accounts desirable rearrangements and alterations in form, have been made, but in every case, I have endeavoured to render the substance as exactly as possible. Statements, apparently having no particular import have been omitted. Of the narrators' own explanations of the phenomenon, their means of getting rid of it, etc., only a summary is given in the collective description. A *mile*, when the contrary is not expressly stated, always denotes an English nautical mile (= 1852 m.); all other measurements are reduced to the metric system.

Some parts of Chapter II on matters bearing on the subject, are merely a summary from other books and are inserted only for the convenience of the reader. A brief account of Sir JOHN SCOTT RUSSELL's experiments on phenomena in canal navigation, which are remarkably analogous to "dead-water", is found in this chapter. It is interesting to notice here, that from these phenomena and Lord KELVIN's and Sir G. G. STOKES' mathematical treatment of them and of waves in the boundary between two liquids (see p. 139 and p. 41), it would be but a short step to anticipate that waves in the salt-water fresh-water boundary should cause effects quite similar to those here recorded of "dead-water". It would, however, owing to the small difference of specific gravity between the two water-layers, be rather difficult to believe *a priori*, that the effect should prove so powerful as it actually is.

The experiments, although simple typical conditions were selected so as to allow of a simple discussion, were so carried out as to imitate as closely as possible on a small scale, real cases. A model of the *Fram* and some other boats, were towed in a glass-tank filled with homogeneous water or having water-layers of different specific gravities. The towing force and the velocity of the boat-model, were registered; and one of the water-layers being coloured, the wave-motion at their common boundary could be observed and photographed.

The experiments thoroughly confirmed Prof. V. BJERKNES' opinion: the vessel when moving at low speeds generated large waves at the salt-water fresh-water boundary, and the resistance at these speeds was anomalously increased. At higher speeds, however, the waves disappeared and the resi-

stance was not affected by the fresh-water layer. These facts and another interesting phenomenon observed during the experiments, form the subject matter of Section B, Chap. III.

In Section E of Chap. III, I have tried to show that all other phenomena connected with the dead-water — the appearance of the sea-surface, the loss of steering, etc., may be explained from these boundary-waves. Owing to the small size of the tank in which the experiments had to be made, the explanation of all these points could not be experimentally confirmed, and some details will possibly be subjected to modifications in the future. It is hoped that the Article "How to get free of dead-water" (p. 110) will not only be found to be an explanation of the seamen's experiences, but that it will in the light of our present enlarged knowledge of the phenomenon, be found to contain valuable suggestions as to the best means of getting free from this very inconvenient occurrence.

To make sure of the correctness of the explanation given, it must be proved that the boundary-waves, created, were the essential cause of the increased resistance in the experiments; and further that this increased resistance is large enough to explain the effects of the dead-water. The proof of these points is one of the chief objects of the last section of Chapter III.

The experimental work was performed during the time from the beginning of 1900 to July 1901; the first half of the time was, however, occupied with experiments which now may be regarded as only preliminary.

The expense of apparatus has been defrayed by the *Fridtjof Nansen Fund for the Advancement of Science.* The Council of the *Stockholm's Högskola* made me a grant for the year 1901, from the *Fund to the Memory of the Civil engineer August E. W. Smitt* and renewed it for the year 1902; I am glad of here having the opportunity of expressing my warmest appreciation of this help.

Also I desire to place on record my sincere thanks to my revered teacher Professor V. Bjerknes, who has with the greatest interest followed my work, and has on many occasions given me the most important advice.

Professor Fridtjof Nansen, by an appeal published in several Norwegian newspapers, has rendered me most effective assistance in collecting accounts on dead-water. Dr. S. Almquist, Rector of the *Högre reallläroverket i*

Stockholm, was kind enough to have a most excellent room placed at my disposal, in which to conduct the experiments. Some instruments were lent by *Stockholms Högskola*. *H. M. Foreign Office* were so good as to publish in the autumn of 1901 an appeal by the author in a great number of foreign newspapers with the object of collecting accounts on dead-water. Dr. CHARLES J. J. Fox kindly read the whole manuscript as well as the proofs, and with great advantage improved the English. To all those who have in these and other ways assisted, and last but not least to seamen and others interested, who have left valuable communications on dead-water, I here desire to present my sincere thanks.

KRISTIANIA, *June*, 1904.

V. WALFRID EKMAN.

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CORRECTIONS.

Page 57, line 13, *for* ADERMAN *read* ÅDERMAN.
Plate XI, Fig. 3, *for* wave-resistance *read* dead-water-resistance.
Plate XIII, in the figure below Fig. 1, *for* quadrate *read* square.

I.

DESCRIPTIVE.

A. COLLECTIVE DESCRIPTION.

The following description is based entirely upon verbal and written accounts which I have received from a great number of sailors having experience on the subject. Special weight is laid on such statements as have been unanimously given by several authorities, or as may, for other reasons, be considered trustworthy.

Dead-water only appears near to coasts, in those places where a suitable layer of fresh or brackish water rests upon the heavier sea-water. A vessel, moving in such a place at slight or moderate speed, may happen to feel the influence of this phenomenon; it is then said that *the vessel "has taken dead-water"* or *"got into dead-water"*. It is a very troublesome matter indeed. A sailing-vessel in this plight, generally refuses to answer her helm and becomes unmanageable; steamers, at times sailing-vessels also, keep their steerage, but nevertheless the dead-water is a great hindrance, causing the ship to lose her speed almost entirely. The *Fram*, for instance, though generally capable of making 4·5 knots, along the Siberian coast when heavily loaded, had her speed reduced to about 1 knot in dead-water; and a similar reduction of speed seems to be a common effect of this phenomenon. If, in addition, the vessel has to move up a river or against even a moderate current, her progress may be altogether stopped.

When dead-water is present, the sea-surface around the vessel presents a peculiar appearance, varying under different conditions. When the phenomenon takes place regularly, *a set of stripes will be observed astern* crossing the wake,

or a couple of stripes stretch obliquely aft from the sides of the ship. They are described as "rips" or whirling "current backs"[1]; also as stripes of hopping wavelets. These stripes often stretch far away from the ship. They may be seen in the various sketches Figs. 6—13 of Pl. IV, which are copied from original drawings, appended to some of the mariners' accounts. An indication of similar stripes may be seen in Fig 5. *Fig. 1, Pl. V — illustrating the* same case as the schematic sketch Fig. 10, Pl. IV — gives a more objective representation of the phenomenon. Prof. NANSEN himself does not speak of any such rips or stripes but he observed *very long and low waves,* stretching across the wake of the *Fram;* the crest-lines of these waves are shown on the rough sketch, Fig. 1, Pl. IV.

Round the ship's stern a wake is formed of unrippled, eddying water, which is said to follow the ship and even to advance towards her, so that *a boat in tow will be carried in close up under her stern.* The stripes issuing from the sides of the vessel seem to form the boundary line between the water following the vessel and the water outside. It is commonly believed that a bulk of water is clinging to the vessel; and this proceeds through the water outside, with a roar. It draws off from the vessel with increase of wind, and *vice versâ.* Likewise the stripes crossing the wake are said to come nearer to the stern, the more the wind slackens.

It is a peculiarity of dead-water, that *it always appears quite suddenly.* Its influence upon the steering of a ship, or the appearance of the sea-surface may, in some cases, be subject to gradual modifications; its effect upon the speed of the ship always takes place, however, quite suddenly, the speed being at once reduced from its ordinary magnitude to a small fraction thereof. It very often happens, moreover, that the ship has dead-water at the very outset. Just as suddenly does the ship recover her ordinary speed when she gets rid of the dead-water — "as if cut away from a mooring astern". Sometimes she soon gets free, but on particular occasions vessels may remain helpless in the dead-water for a whole day or even longer.

Usually it is only vessels in tow and sailing vessels in a light breeze (vessels with comparatively small motive power), that are influenced by dead-

[1] Smooth or agitated streaks often seen on the sea, denoting the boundary between two currents with different motions.

water. It has happened, however, that steamers have been caught by it. It is said that vessels do not run the risk of falling into dead-water when moving at above a certain speed, *e. g.* 3, 4, or 5 knots, according to conditions prevailing.

The phenomenon shows its simplest phase, when a *steamer* or a *ship in tow* takes dead-water. There is, however, a difference in steering, between screw-steamers and ships in tow; thanks to the influence of the screw the former are able to maintain their course; towed ships on the other hand, do not answer their helm at all, and often in narrow waterways, the tow-rope must be shortened, to prevent the ship from sheering off, with the consequent risk of running ashore.

If the engine be stopped, a ship in dead-water does not lose her motion gradually, as under ordinary circumstances, but she *stops short*, and may perhaps be sucked astern. In navigating the mouth of the river Glommen similar and still more remarkable effects of dead-water have been observed. As illustrated by Fig. 6, Pl. IV, the vessel is followed by a long train of the dead-water stripes described above. When the tug-boat is stopped and the towed ship in consequence stops short, the dead-water stripes overtake her, *and, as they pass, the ship swings backwards and forwards, once for each stripe that passes.* In the same way, vessels made fast to the shore, will be pushed up and down the river when a ship in dead-water has been towed past them; the power of the stripes being at times so violent as to tear a vessel away from her moorings.

Sailing-vessels, when caught by dead-water, usually do not answer their helm and, in spite of the rudder and various manoeuvres with the sails, take a certain course depending on the direction of the wind; often they run up in the wind with sails shivering. Therefore, the loss of steerage is generally the most troublesome effect of the dead-water on sailing-vessels, although in particular cases, the vessel may be kept to her course with more or less effort. Owing to the wind the aspect of the sea is often asymmetrical, the stripe issuing from the windward side being best developed (see Fig. 12, Pl. IV). When the sea is rippled by the breeze the stripe on the windward side may be invisible, whereas on the leeward side where the sea is smoother, it may may still be seen. Often, however, the dead-water appears in its regular aspect.

In the Kattegat the phenomenon is sometimes less regular (see accounts Nos. 13 and 14 as well as Fig. 14, Pl. IV). As long as the ship's course is between two certain points she may answer her helm quite willingly, but she cannot be made to head a course beyond either of these two limits. The wake and the "dead-water stripes" have often an assymetric direction. The cause of this is, no doubt, to be ascribed to the lighter brackish water running as a narrow surface-current on the top of the heavier sea-water, so that the upper and the lower parts of the ship's body move in water of different velocities. Such surface-currents were directly observed in two cases mentioned below. It is evident that the two currents — especially if the ship draws more water aft than forward — tend to turn her head in a certain direction. To both sides of this direction she may be made to head, though within certain limits, depending on the strength and direction of the surface and under-currents, and of the wind. Thus the currents in the sea are able to considerably modify the effects of the dead-water. It is very probable that particular accidents mentioned by the mariners as "dead-water" are mainly caused by such surface- and under-currents. Some Scandinavian mariners apparently give the name of dead-water to various effects upon their vessels, which cause them to lose their steerage. Thus, for instance, when a vessel in a light wind, is affected by a whirl-pool or when she is brought out of her course by the influence of currents of different directions, she is sometimes said to have got into "dead-water" although the cause of the mishap has nothing in common with this peculiar phenomenon.

It was mentioned above (p. 3) that vessels run the risk of getting into dead-water if moving at below a certain speed depending on the conditions prevailing, but not when proceeding at higher speeds. In particular cases even steamers have "taken dead-water", whereas, in certain places, only sailing-vessels, in a very faint breeze, are exposed to it. It is therefore evident that dead-water may be of varying *strength* in different places and under different conditions. Without further defining this notion, I will give a summary of *the experiences of several Scandinavian mariners as to the strength of dead-water under different conditions.* The explanation of the different statements will be reserved for Chap. III.

First of all, dead-water seems less effective the smaller be *the difference of density* between the surface layer and the sea-water below, or the more

the former has mingled with the latter. In one of the strongest cases of dead-water that we know of — the experience of the *Fram* at Taimur — this difference of density was about as great as possible (fresh drinking water on the sea-surface, and pure sea-water on a level with the bottom-cock of the engine-room). In the Norwegian fjords, where the conditions are unusually good for river-water spreading almost unmixed over pure sea-water, most marked cases of dead-water are quite general. In the Kattegat, the density of the sea-water is not so great, and the river-water, before flowing out over the sea, becomes much mixed with sea-water. Dead-water appears there only with a light breeze, after some days of fine weather, and not at all when the water-layers are stirred by previous storms. (In the neighbourhood of the mouth of the Göta River, however, strong dead-water has been experienced). In the Baltic, where the specific gravity is less than 1·006 or 1·007, dead-water is only exceptionally observed, and then only with very feeble wind.

Furthermore, *the relation between the thickness of the surface-layer and the draught of the vessel* is of great importance as regards the effect of dead-water. At the mouth of the Glommen, for instance, small craft experience dead-water farther out at sea, where the surface-layer has less thickness, than large vessels, by which it is again felt higher up the river (see Account No. 3). According as the outflow of fresh water increases, the region where dead-water appears withdraws from the river mouths seawards, and *vice versâ*. In winter, when there is little water in the Glommen, dead-water principally appears in the river proper, between the town and the Sarp rapids (see the maps, Pl. III); but in summer its region is *below* the town and extends as far as the outermost rocks of the Kristiania Fjord.

Another fact, probably connected with the one stated above, is that dead-water is generally stronger in a moderate sea-breeze, than when a land-breeze is blowing. For, as a result of the sea-breeze, the fresh water is retained at the coast and its depth becomes in concequence greater, while the land-breeze carries most of it out to sea. On the Glommen the dead-water is said to be stronger at flood-tide, than at ebb — possibly owing to the tidal currents or to variations in the depth of the fresh-water layer.

Some of the narrators attribute importance to *the shape of the vessel*: it is said that a sharp built vessel is more exposed to dead-water than one

of bluff design. One of my authorities has stated the contrary. Those that are laden or of deep draught are always more liable than are light ones.

Even after taking into account all the above circumstances, there is nevertheless something highly capricious about the appearance of dead-water; and it is often impossible to understand why, in a certain case, a vessel has not "taken dead-water", and in another case, why she did not escape its influence. A ship may, for instance, be towed without difficulty from one place to another, but another similar or smaller ship being shortly after towed the same way, will be so forcibly held by dead-water as scarcely to be moveable from the spot. On another occasion a sailing-vessel or a vessel in tow may be held by the dead-water, while other similar vessels, with less or equal moving power, may pass quite near to her without being in the least troubled by the phenomenon.

Vessels apparently often "take dead-water" on account of the wind slackening or as a result of the vessel entering water-layers of a different character; sometimes the cause of getting free may likewise be put down to a similar influence. But often the change cannot be explained by any perceivable external cause. Often a vessel gets into dead-water while tacking, or on account of bad steering, or when her speed is from any cause temporarily slackened. If a sailing-vessel takes dead-water on account of the wind slackening to below a certain strength, the wind, as a rule, must freshen to *above* this strength to free the ship again. In general, when a vessels speed is temporarily slackened and she gets into dead-water, she may be unable to recover her head-way for some shorter or longer time, even when the conditions for full speed are present again.

Mariner's opinions of dead-water and its causes vary somewhat, and to judge from their explanations of the phenomenon they generally acknowledge they do not understand it. Most of my authorities, nevertheless agree in the observation, that it depends on the existence of a surface layer of fresh or brackish water. It is a general opinion, that "the fresh water sticks to the vessel" and is dragged along with it on the top of the salt water, thus impeding the speed — the fresh water on account of its forward motion, has no influence upon the rudder. Other narrators declare that the phenomenon is owing to the fresh water running as a surface-current above the salt water; if a ship moves at a slow speed, the rudder becomes too much

influenced by the currents, so that the vessel loses her steerage. Another of my correspondents has advanced an explanation somewhat more correct. (See the end of Account No. 12). Mr. M. LEEGAARD, harbour engineer, explains the phenomenon as owing to the turning effect of currents on the ship's body itself, and, as already mentioned, this is sometimes the case. (See further account No. 15).

With a view to *removing the dead-water*, seamen have made use of all sorts of odd means, which they believed would "loosen" or "rub away" the "dead-water crust", that is, the fresh water. Such means are:

Sheering off from the course,

Running the whole crew forward and aft on deck,

Pumping violently,

Scooping up a quantity of water on deck,

Pouring out petroleum ahead of the ship,

Dragging a hawser under the ship's bottom from stem to stern,

Working the rudder rapidly,

Firing guns into the water (on men of war),

Cutting and beating the water alongside with oars or handspikes etc.,

Dragging a seine along the ship's side, and so on.

Sometimes the two last mentioned methods are said to have been successful, but in most cases all of them are by the narrators themselves, declared to have been of no use. I may mention now, that on account of the explanation of the phenomenon which will be given later they must be expected to be of no appreciable effect, although several of them are not quite unreasonable.

Tug-boats, when their vessels in tow get into dead-water, have more effective means of getting them loose again, and these are said to often succeed (see Accounts Nos. 3 and 26). The simplest method is to let the tug stop for a while, until the dead-water stripes have passed by, and then to go full speed ahead again. Another effective way is said to be, to have the tow-rope as short as possible ,so that the screw violently stirs up the water around the towed vessel: or for the tug-boat to go to and fro along the sides of the ship and then to make full speed again. It is a singular experience, recorded by several observers, that a ship in dead-water may get free if passed by a steamer, even at a distance of several ship's lengths.

It is a difficult thing to state anything certain concerning *the frequency of "dead-water"*; the accounts are much too scanty and incomplete. The map Pl. II is an attempt in this direction, as far as *Scandinavia* is concerned. The places where dead-water has been observed, or is said to occur, are represented by different signs explained on the map. It must of course, be expected *a priori* that the greatest numbers of accounts should be received from those fjords and other waters that are most frequented, and that accounts should be altogether wanting from several small places with very little traffic, or where Prof. NANSEN's request in the newspapers did not become known Taking these circumstances into account, it seems probable, from the results illustrated by the map, that dead-water occurs in all Norwegian fjords into which any considerable quantity of fresh water flows and, although with less strength, in the Kattegat as well. As a general rule the phenomenon seems to appear in particular cases, only when circumstances are favourable, but yet not so very infrequently. In the environs of the mouth of the *Glommen* it is constantly feared; the towing masters must take it into account and not take too great a load at the risk of sticking in dead-water. It must be noted that the *figures* on the map give a too low estimate of the frequency of dead-water. For several narrators declare they have been out in dead-water on many occasions that they do not think it necessary to mention specially. All these cases, not specially described, are left out of account.

Still more difficult would it be to draw any reliable conclusions as to *the occurrence of dead-water beyond Scandinavia.* To my appeal published in at least 36 foreign newspapers,[1] I have not received more than 18 answers, of which number half are of decidedly negative import. *Only two narrators have certainly experienced dead-water themselves,* one in the *Kristiania Fjord* and the other inside *Vancouver Island* (Canada). It is not probable, therefore, that dead-water is at all as decided or as common on other seas as in Scandinavia; for one cannot assume that a phenomenon so troublesome and so characteristic should escape the attention of seamen. On the other hand, four of my Scandinavian authorities mention dead-water

[1] 6 British, 1 German, 2 Dutch, 1 Belgian, 11 French, 2 Italian, 2 Chinese, 2 Brazilian, 1 in Argentina, 4 in United States of America and 4 Canadian.

in the *Mediterranean* and off the great river-mouths of *North* and *South America.* Some old stories also tell us about certain strange adventures on the Mediterranean which doubtless were cases of dead-water, although owing to the narrators' modes of explanation, they would rather seem to be myths. Vessels propelled by oars or running before a fair wind were suddenly stopped in their course by a strong, unknown force. It was commonly believed that this was caused by a small fish the *Remora,* sucking itself fast to the ship's body, and it was said that but *one* of these fishes was required to hold a ship immovably. The belief in this power of the *Remora* seems to have survived far into later times, and for that reason it seems probable that the phenomena in question have appeared occasionally.

From all these circumstances I conclude that dead-water may occur at every place where fresh-water flows out over the sea, but that from some reason or other it is comparatively seldom met with beyond Scandinavia or appears in a less decided manner than in the Norwegian fjords. The causes of this circumstance will be discussed below.

B. ORIGINAL ACCOUNTS.

Effect of dead-water on steamers and ships in tow.

No. 1. The series of narrations may well begin with an account of those famous cases of dead-water which happened to the *Fram* in the autumn of 1893 North of Siberia, and which gave rise to the investigation of the matter. The following representation of these cases is extracted from the narrative to be found in Dr. NANSEN's book "Fartherst North", pp. 172—175 and 177 and from a letter of his to Professor V. BJERKNES, which also contains extracts from parts of Dr. NANSEN's own journal. In arranging this material the original form of the different statements is maintained, and in its present state, the whole has been revised by Prof. NANSEN himself.

"*On Tuesday, August 29th* 1893 the *Fram* got into the open water in the sound between the isle of Taimur and Almqvist Islands and steamed in calm weather through the sound to the north-east. At 6[1] in the afternoon I saw from the crow's-nest thick ice

[1] In "Fartherst North" stands *five,* which is incorrect.

ahead, which blocked further progress. It stretched from near Cape Laptev right across to the islands north[1] thereof. We approached the ice to make fast to it, but the *Fram* had got into dead-water, and made hardly any way, in spite of the engine going at full pressure. It was such slow work that I thought I would row ahead to shoot seal. In the meantime the *Fram* advanced slowly to the edge of the ice with her engines still going at full speed, and there was made fast at half past ten in the evening. The engine stopped but one half a ship's-length from the edge of the ice (according to SCOTT-HANSEN[2], it was one whole ship's-length), but it looked as if the ship was drawn back, at the same moment the engine stopped, and she barely struck the ice.

"The edge of the plain ice, when seen for the first time, at all events, was not more than 8 naut. miles off, and hardly that, because it was quite low and could not have been seen from a greater distance with the naked eye. According to the unanimous calculations of myself, of SVERDRUP in the log, and of SCOTT-HANSEN, the speed must have been reduced to 1·5 or 1 knot in the dead-water. About 6 o'clock the dead-water was still not fully developed.

"The Fram had at that time a draught of 5 m. or more. Her common speed in smooth water, and at full steam pressure, was at that time, fully 4·5 knots. Perhaps it was 5 knots at top pressure. I may add, that I convinced myself that there was really full steam pressure and that the engine was being worked at full power.

"There was but a very slight current to be observed at this time — from 6 in the afternoon to about midnight — as likewise next morning.

"The ice that covered the sound north of Taimur Island was in a state of dissolution and apparently melting very rapidly, and this was probably the main cause of the sea in the sound being covered with a layer of fresh water. Unfortunately, I have no measurements of the thickness of the fresh-water layer. I only say in the journal that the water at the surface was almost fresh (drinking-water), whereas through the bottom-cock of the engine-room we got perfectly salt water[3]. I suppose that the bottom-cock at that time was about 4 m. or more, below the surface of the water, and accordingly the *Fram* struck the salt water.

"*Aug. 30th,* in the morning, we went on to anchor in a bay at Cape Laptev. We now wanted to thoroughly clean the boiler, a very necessary operation. I say in the journal that the bay lay a few miles farther south, and I am sure that it might have been at most 3 nautical miles. But we took 4 hours and more, to steam that little distance.

"I dare not affirm that the steam was at top pressure during the whole time on this occasion. The distance being so short, it might be conceivable that the engineer did not think it necessary to fire up very heavily, but there cannot have been very much wanting on that score, and at any rate the speed under ordinary circumstances (*i. e.* with no dead-water) would have been 4 knots.

"We could hardly get on at all for the dead-water, and we swept the whole sea along with us. It is a peculiar phenomenon this *dead-water.* We had at that time a better opportunity of studying it than we desired. It occurs where a surface layer of fresh water rests upon the salt water of the sea, and this fresh-water is carried along with the ship, gliding on the heavier sea beneath as if on a fixed foundation. Dead-water manifests itself in the form of larger or smaller ripples or waves stretching across the wake, the one be-

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[1] In "Fartherst North" *north* should evidently be read for *south.*

[2] SCOTT-HANSEN, who was at that time in command, says: "When we went in to moor at the ice, I had the engine going at full-speed until we were about 30 or 50 m. from the edge of the ice. I then stopped, and the ship lost her speed altogether while going just this slight distance."

[3] On special inquiry Prof. NANSEN informed me that this latter observation was made *while the ship was in motion.* As will be seen later, this circumstance is of importance.

hind the other, arising sometimes as far forward as almost amidships[1]. We made loops in our course, turned sometimes right round, tried all sorts of antics to get clear of it, but to very little purpose. The moment the engine stopped, it seemed as if the ship were sucked back. In spite of the *Fram*'s weight, and the momentum she usually has, we could in the present instance, go at full speed till within a fathom or two of the edge of the ice, and hardly feel a shock when she touched.

"*On September 2nd*, at last, the boiler was ready. We steamed south in the evening, but were still followed by the dead-water during the whole night. According to NORDENSKIÖLD's map, it was only about 20 miles to Taimur Strait but we were the whole night doing this distance. In two watches (*8 hours*) we hardly advanced *8 miles*, the engine constantly working at full speed. Now, the boiler had been cleaned out and filled with fresh water from the surface of the sea, so that the conditions for good speed were very favourable. It may therefore be supposed that the speed was reduced to about a fifth of what it would otherwise have been.

"At 6 a. m. *September 3rd*, we got into a thin layer of new sludgy ice, that scraped the dead-water off us. The change was noticeable at once (according to the account of SVERDRUP, who had the watch at that time). As the *Fram* cut into the ice she gave a sort of spring forward, and after this, went on at her ordinary speed; and henceforth we had very little more trouble with dead-water[2]. The cause of the last fact evidently was that later there was a much stronger current and wind, so that the fresh-water layer at the surface was swept away. During August 30th and the night between 2nd and 3rd September, the speed was rated as 1 knot, in the dead-water."

No. 2. Even on her way north, the *Fram* met with dead-water; it was on the *By Fjord* on going in to *Bergen*. The *Fram* on this occasion also, was under steam, but prams and row-boats easily passed by her. Captain S. SCOTT-HANSEN, who had the command at that time, writes about this episode:

"On the *By Fjord* at Bergen the *Fram* got into dead-water about one nautical mile off the mole of the "Vaagen". The speed was considerably reduced. Of exterior, visible phenomena the most remarkable was a stripe, resembling the boundary between a current and still water, and stretching from the bow obliquely aft, as indicated in the sketch Fig. 13, Pl. IV. We also had an impression of the surface-water round the stern, and in the immediate proximity of the ship's side, being dragged along with the ship. The wake of the ship also varied from its customary appearance, being broader and not showing the regular central line of defined whirls. It looked as if the water set in motion by the propeller, was spread over a much broader space than it is wont to be, and the whole mass of the surface-water in the wake to a distance of 10 or 20 m. from the stern, gave the impression of having a tendency to hang on to the vessel.

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[1] For the purpose of illustration Prof. NANSEN has drawn a sketch shown by Fig. 1, Pl. IV. The lines stretching to the sides of and aft of the vessel represent the crest-lines of long, low waves.

[2] NANSEN adds in a note: "It is probable that the fresh surface-layer has been rather thin just where the ice occurred. I have found that ice often is formed on the under side of a fresh surface-layer by the cooling effect of the cold water-layers below (see No. 9 of this report: "The Oceanography of the North Polar Basin" pp. 308–309 as well as the footnote on p. 300). In open water this ice floats up as delicate ice-needles, which melt in the warmer surface-layer; but if the latter be thin enough, they will reach the surface before they have had time to melt, and may even form a crust."

"When passing the mole-head, the engine still worked at full-speed; and I only stopped it when the ship was a little way (100 or 150 m.) from the buoy to which we should make fast. It was remarkable how quickly the ship lost her speed, and if my memory does not deceive me, I was obliged to start the engine again in order to reach the buoy.

"Finally I will not omit remarking, that the description given above has been written several years after the occurence, but according to my best recollection of the facts cited."

No. 3. Mr. G. A. LARSEN of Fredrikstad has during many years' experience of towing, struggled with the dead-water on the river *Glommen* and the fjord off it, and he has most kindly, by letters and word of mouth, communicated to the author the result of his practical experience in these waters. His communications are very instructive, based as they are on long experience of an extensive district where dead-water seems to be more common and stronger, than in any other place. The different statements have been arranged below in a consecutive account, and this has been revised by Mr. LARSEN himself.

"As is seen on the maps (Pl. III), the river *Glommen* branches at Fredrikstad into two outlets — the *East Arm* and the *West Arm* — girding *Krager Island*. Most of the water flows out the straight way through the East Arm, but a considerable quantity flows through the channel north of Krager Island and further on to the West Arm. In both outlets, especially in the East Arm, the current is sometimes very strong, but they are both navigable, as is also the main river all the way up to the *Sarp Rapids*.

"On these waters and the surrounding parts of the Kristiania Fjord I have towed ships and lighters with a tug of 200 H. P., and drawing nearly 3 m. The ships and lighters in tow generally have a draught of 3 or 6 m., and on them the dead-water has principally taken effect. The tug-boat may also take dead-water but — on account of her smaller size — very slightly and to no hindrance. The following statements principally concern the effect of dead-water on towed vessels.

"Dead-water only occurs where there is a layer of fresh or brackish water above the salt water. Its effect depends on the thickness of the fresh-water layer. If this be great, it is hard to advance; but if it be only small so that the screw throws dark sea-water up to the surface, we never fear dead-water, it has then very little influence except on very small vessels. At flood-tide, dead-water is always stronger than at ebb. A sharp and fast-sailing ship is always at a much greater disadvantage in brackish water, than a bluff one of bad design.

"The strength of dead-water in different places changes with the season. In *winter*, when there is little water in the Glommen[1], the worst district for dead-water is on the river *beyond Fredrikstad*, and there it reaches right up to the *Sarp Rapids*. In winter there is also dead-water in the *Fredrikshald Fjord*. In spring, as the water rises, the dead-water moves farther off, and in *summer* it does not exist beyond *Vaterland* on the

[1] From the data given in "Hydrologiske meddelelser for Kongeriget Norge, Vandstandsobservationer, B. I", Kristiania 1900, I have calculated the average quantity of water carried by the Glommen during the different months. The mean averages for the different months during the years 1881—1893 are in cubic metres per second: Jan. 180, Febr. 165, March 162, April 265, May 1055, June 1525, July 1045, Aug. 900, Sept. 805, Oct. 740, Nov. 500, and Dec. 260. I am much indebted for these data to Kanaldirector G. SÆTREN, who was kind enough to bring them under my notice.

East Arm[1]. Then, it generally occurs *below Vaterland*, on the *Single Fjord*, *Sæken*, *Leret*, and all the way to *Torbjörnskjær* and *Færder* (see the map, Fig. 1, Pl. III). The smaller the vessel so much the farther out, will it run the risk of being caught by dead-water. On the other hand, only deep vessels stick very fast in it. In sea-water we are able to draw at least four times as much as in brackish water (dead-water).

"On both outlets of the Glommen it may be very prevalent, and if I cannot tow the ship at 5 knots or more, I run the risk of falling into dead-water. On the river and its outlets, however, we do not *fear* dead-water except when going *up-stream*; for when going down-stream we are always carried along with the current. Near *Krosnæs* on the West Arm, it once happened that a 200-ton pilot-steamer got into dead-water; she was of about 60 H. P. and was able to steam at about 4 knots. On the East Arm the dead-water is still stronger than on the West Arm, and sometimes sailing-vessels may there be seen stuck fast in spite of a breeze brisk enough to keep the sails firmly strained. The worst place in this respect, is the sound between *Krager Island* and *Kjögö*.

"Sometimes it happens that one vessel gets into dead-water and another not, though it is impossible to discover any reason for it. Once, for example, — it was flood-tide — I had to move a vessel of 400 tons from one place to another in the channel between the East and the West Arms. The vessel took dead-water, and 3 hours were spent in going a distance of 3 cables' length (600 m.)[2]. Immediately after, I towed a vessel of 800 tons the same way without any similar difficulty or trouble. Another time, in the Fredrikshald Fjord, I towed a lighter of 120 tons and drawing 4·5 m. The lighter took dead-water, and the tug of 12 H. P. could not get it loose. Nevertheless a ship of 400 tons drawing about 5 or 6 m. and towed only by three men in a boat had no experience of the dead-water and easily passed us. There was no wind to be seen on the water, and the stream was with the ships. Twice, when I have towed a lighter in dead-water and have not been able to get her loose, it has happened that she has run aground and then she has got free from the dead-water and has been towed further without any difficulty.

"It is an easy matter to perceive if a vessel has got into dead-water. From the stems and sterns of the tug and the ship in tow, there are then formed 4 pairs of *streaks* or "*dead-water waves*"[3] (see Fig. 7, Pl. IV) indicated by very small and short wavelets. They stretch far away to the sides and, when the distance is not too great, even reach the shores. In the *Drammen Fjord* beyond *Svelvig* I have seen such streaks, extending half a naut. mile on each side. There are not always "dead-water waves" formed from *the tug*. And if there are, they may fall astern and disappear, and then, a while afterwards, new streaks may be formed. It often happens that the streaks issuing from the *stem* of a ship in tow are missing, or that they first disappear, and then recur again. The streaks from the *stern* always remain the longest, especially the streak on the side opposite to that to which the tow-rope is directed. The ship is also followed by a train of similar streaks moving on at the same speed as the ship[4].

"Around and aft the stern the water follows or even overtakes the ship, so that a boat which is following in tow is drawn close to the ship's side and cannot be kept clear of it. The water is likewise pushed before the stem, but along the sides it runs astern at great speed — faster than if there were no dead-water. For example a cutter of about 70 tons lay in dead-water on *Leret* in spite of a SW. wind which under ordinary circumstances would have probably given her a speed of 3 knots. The cutter could not be

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[1] According to Mr. LARSEN, dead-water has been observed beyond Vaterland, during the summer of 1901, which was very dry with little water in the river.

[2] It may be remarked that this does not give the velocity *through water*, because the vessel was tugged upstream.

[3] The sign of quotation is due to the present writer.

[4] At the author's request Mr. LARSEN has made a sketch (Fig. 6, Pl. IV) illustrating this matter. This figure upon the whole, gives a better idea of the streaks than Fig. 7. The two horizontal lines denote the banks.

steered before the wind, but she luffed and steered N. or E. A pilot boat of about 10 tons, which she had in tow, was sucked in to the stern and could not be kept clear of it. Assistance was requested from a passing tug, which steered to the bow of the cutter to receive a hawser, but the two vessels were pushed from each other; the attempt was repeated, in vain, and in order to come near enough to get the hawser aboard, the tug was obliged to *back* astern to the bow of the cutter.

"A vessel which is towed in dead-water does not answer her helm at all, and on the river it is necessary to shorten the tow-rope, lest she should run aground or ashore; she always strives to steer to the side, away from the course of the tug. Nor is it possible to put the "wash" of the tug against the stem of the vessel in tow; it is pushed to the side as shown by the figures. In dead-water it is better to use *two* tugs attached to the vessel by short tow-ropes. Then a separate "wash" may be projected to each side of the vessel and so "cut away or destroy the dead-water layer, so that it cannot cling to the ship's side"[1]. In this manner two boats of 10 H. P. more easily drag a ship of 400 tons than one boat of 20 H. P.

"When the streaks or "dead-water waves"[1], following the shores of the river, encounter a vessel at her moorings, they prove very powerful. When the first streak issuing from the tug reaches the vessel, she is pushed forward (sucked back)[2] until the streak has passed half the length or more of the ship; then she is drawn back (pushed on)[2] until she is reached by the next streak when she is pushed forward (sucked back)[2] again and so on. This is repeated with still greater force by the streaks issuing from the ship in tow; and it has happened that vessels, moored for the winter, have broken their moorings when a large ship has been towed *down* the river (in this case the force of the streaks is greatest). When the tug and the ship have passed, the vessels on the banks continue to swing to and fro, owing to the force of the successive streaks. The motion gradually slackens, being perceivable for half an hour after the ship in tow has passed.

"In the same manner, if the engine be stopped, the towed vessel herself stops short and is then pushed backwards and forwards as she is reached by the successive dead-water streaks. The streaks having passed by, the vessel may, in many cases, be got free from dead-water, by making full speed ahead.

"On the contrary, if, towing with a rather powerful tug, you wished to make a ship fast in dead-water, you must begin to go at slow speed and then increase the pace very gradually. In this manner it is possible to get so firmly fast that the ship cannot be got free by the full pressure of the engine, without first stopping and letting the dead-water streaks pass[3]."

No. 4. Mr. A. E. TØNNESSEN, Instructor at Grimstad, has kindly communicated the following facts:

"On the *West port of Kristiansand* (see the map on the upper left corner of Pl. III) dead-water often occurs in the summer, in calm weather or with easterly wind. Between the town and *Odder Island* there is a channel, 3 or 4 m. in depth, through which the water (river-water and salt water from the East port) almost continually flows out over the West port. The ships, when leaving the town, must pass this stream of river-water and there, athwart the channel, they often in calm weather get into dead-water. Among several cases I have observed, the two following are the most remarkable.

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[1] The sign of quotation is due to the present writer.

[2] In two separate letters these two different successions of the motions were stated. With regard to this order the account is therefore not quite reliable.

[3] When the author in May 1901 was at Fredrikstad to see the dead-water, Mr. LARSEN on account of unfavourable circumstances did not succeed in getting fast in dead-water. Only a couple of very feeble dead-water streaks appeared and disappeared again.

"On Sept. 20th 1884, Wind slight SW., I left Kristiansand with a laden barque, "Carla", 502 tons and drawing about 4·9 m. We had a steamer attached to the bow, and we already had good speed, when all at once, the ship took dead-water off Odder Island. She did not answer her helm, and the water followed at the stern, as if it were clinging to the ship. On account of her great inertia she moved right through it, but her velocity was almost entirely gone when we finally got rid of the dead-water.

"One day in the summer of 1890 (or 1889?) I saw a barque of about 600 tons go out light, towed by a powerful tug. The wind was gentle SE. breeze. Though previously going at a good speed, the barque took dead-water at the same place as the "Carla", and she stopped so quickly that it looked as if she had dropped anchor again. Both the ships were new-coppered, so that the friction must have been a minimum."

Effect of dead-water on sailing vessels.

No. 5. Mr. COLIN ARCHER of Larvik, sent me a letter, from which, by kind permission of the writer, I make the following extract.

"It has often occurred to me that it would be an extremely interesting problem to investigate that very peculiar phenomenon which we call "dead-water", and which has not yet, as far as I know, been made the subject of systematic investigation.

"We have here on our fjord a very good opportunity of observing the phenomenon. Two rivers fall into the fjord; and it often happens, when out for a sail, that the boat suddenly loses her way and is held fast, almost as in a vice, in spite of a fair breeze which, in the open sea, would give her a speed of two or three knots. The boat seems to drag after her a broad belt of water which follows in her wake. At the same time a system of sharply defined but small waves is generated on each side. These may be compared to the side waves set up by a steamer at full speed but, of course, on a diminutive scale. I enclose a sketch (Fig. 11, Pl. IV) showing approximately the shape of this wave-system. The waves are small and short, and resemble, in some degree, the ripples caused by a gentle breeze; but they form a simple succession, as shown by the sketch.

"All this is no doubt caused by the layer of fresh water — which is spread over the salt sea — having a motion of its own independent of the underlying water; but the connection between this cause and the singular effect it produces, is not very evident. The dead-water often appears in quite still water, as in closed basins of ports (though in communication with the sea), where no considerable currents can be supposed.

"Whether the shape of the vessels cross section has any marked influence on the formation of dead-water I am not able to say; but I have observed that the drag (i. e. the difference of draught forward and aft) plays an important part, more particularly as regards steering. If the drag be great, the salt water gets a hold on the deepest part of the vessel's keel, and the surface current will carry her head round in its own direction. The rudder loses all, or nearly all, influence on the steering."

No. 6. Admiral C. SPARRE of the Norwegian Navy, often experienced dead-water in the Kristiania Fjord[1] during cruises in his own yacht. He told my the following and kindly revised it, when written. The sketch (Fig. 12, Pl. IV) is made by Admiral SPARRE himself.

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[1] See the map Fig. 1, Pl. III.

"In the *Moss* Sound, in which a rather full river empties itself, dead-water very commonly occurs, especially in spring and summer time. Skippers of small craft relate that they are often obliged to tow their vessels through nearly the whole sound (about 3 or 4 miles) by a rope from shore, even with a nice little breeze of fair wind. Sailing while the dead-water lasts is impossible, if the wind be not fairly strong.

"Often the phenomenon appears in the manner described below:

"A small vessel sails free (the wind on her quarter) on the starboard tack for instance, as in Fig. 12, Pl. IV. Suddenly a stripe begins to form on the water, issuing from somewhere on the windward side of the vessel and stretching obliquely astern (see the figure). Outside (forward of) this stripe the water, as usual, runs aft by the vessel, but inside it the water forms a kind of wake in which the water seems to follow the ship. This has been observed in wind, sufficient to give the vessel about 2 knots' speed. On the whole the phenomenon presents itself *to the sailor,* as if part of the water separated from the rest, attached itself to the ship, and was dragged by it through the sea [1]. When the stripe is developed, the vessel runs up into the wind in spite of the helm and lies quite powerless with her sails shivering. By rowing with an oar on the windward side she may be brought off again, and sailing is possible for a short time, but soon a new stripe begins to form and the vessel runs into the wind again. The masters of quite small craft often try to free their vessels from the dead-water by stirring the water with an oar, and it is said they sometimes succeed.

"Several years ago, while at gun practice in the Kristiania Fjord the narrator, then a lieutenant, was ordered with a gun-boat to tow away two small vessels, which lay becalmed near each other in the line of firing. One of them was towed away without any difficulty, while the other, though of similar size, got into dead-water, and an extraordinary amount of work was required to get this vessel from the spot."

No. 7. Mr. C. O. NILSEN of Bestum near Kristiania has been kind enough to ask several sailing masters for information on dead-water and to send me a summary of the result obtained. From this summary the following may be cited.

"If a vessel, sailing with an even breeze at a rate of 3 or 4 knots, enters a zone of dead-water, her speed will be immediately slackened, as if she had struck a reef of mud. The water is seen to follow to a distance of several metres from both sides of the vessel, just as if she had suddenly been enclosed by a large ice-floe. As long as the vessel makes head-way, a number of small whirl-pools (eddies) are seen in the boundary between the "ice-floe" and the water outside it. The eddies produce a bubbling sound. The vessel gradually has her speed stopped and then no longer answers her helm, drifting helpless sometimes even *against* the wind.

"If the wind increases, the vessel slowly begins to make head-way, constantly dragging the surrounding water with her. This body of water, reaching 3 or 4 m. before the stem, drops slowly astern, until the vessel has the bow clear of it. Then she begins to get more speed, and the "dead-water" constantly moves aft clinging, at last, to the stern of the vessel and dragged along with it as a float of as much as 20 m. in length. If the wind now decreases, the whole mass of water may rush forward and enclose the vessel once more. If not, the vessel now answers her helm, and after some manoeuvres, all of a sudden, breaks free.

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[1] These details are given with reservation, owing to the uncertainty naturally connected with such an observation when not made carefully, with a conscious and definite intention.

Sailing vessels often lie in dead-water for several days; towing with a boat is of no purpose; at most the vessel may be turned a little, but as soon as rowing ceases, she resumes her own way.

"Sharp and deep-drawing vessels are more apt to take dead-water than those that are bluff, or flatbottomed; likewise laden vessels are more subject to it than are light ones.

"Dead-water occurs mainly in the neighbourhood of river mouths, especially at floodtime in spring and summer. In this respect the following places are specially to be mentioned[1]:

"The *Langesund Fjord*; the *Drammen Fjord* below *Svelvig*, and in the neighbourhood of this fjord; the waters SW. and SE. of *Fredrikstad*; the *Single Fjord* all the way to the *Sæken*.

"Furthermore the *Moss Sound* and several other places in the *Kristiania Fjord*, counting from the port of *Kristiania* and the cove at *Lysaker* and as far outwards as beyond *Færder*.

"Two special cases have been brought to my notice: —

1) A ship-master sailing from Rödtangen for *Drammen* in March 1901, there being little water in the river, felt dead-water only beyond *Svelvig*. (It was at this time uncommonly high water). Somewhat later, when he made the same journey, the river had increased considerably, and this time, dead-water was experienced *below* Svelvig and not beyond it.

2) On May 13th 1901 a sharp-built cutter, sailing for Kristiania, took dead-water on the *Single Fjord*. A yacht of the same size, but of bluff design and sailing somewhat more slowly, came after her at a speed of about 4 knots. The yacht made an attempt to tow the cutter out of the dead-water, but the result was that both vessels became fast in it. They were lying in dead-water from 10 a. m. the first day to 4 p. m. the next, and during this time drifted four miles back again with the current, *against* the direction of the steady, fresh breeze, although they had all sails set."

No. 8. Mr. H. B. Eriksen, shipmaster of Fredrikstad has favoured me with the following very instructive and interesting account. The sketches Figs. 8 and 9, Pl. IV, are made by Mr. Eriksen himself.

"During a series of years I sailed between Finland and England — I have made about 50 voyages between Kotka and London — and in the *Kattegat* and *Skagerack* have been much exposed to dead-water. On this trade I sailed the barque "Martha", 515 reg. tons, about 47 m. in length, 10 m. in breadth and with a draught of about 5·7 m. when laden and 3·8 m. when empty.

"In my opinion dead-water may be met with at any hour of the day and independent of the direction of the wind, and it only occurs while the wind is decreasing in a light breeze, when the ship has but a low speed. When there has previously been a fresh breeze from W. or NW., the dead-water is less felt, whereas, after a long spell of fine weather, it is much stronger. For this reason dead-water is worst in June and July, and becomes more feeble in the autumn with its heavy westerly storms. If her speed were so small as 3 knots, and she were laden, my ship easily got into dead-water, and then her speed was considerably slowed.

"When the ship takes dead-water, as a rule (not always) three "rips"[2] are seen, stretching across the wake aft of the stern (Fig. 8, Pl. IV); the steering becomes bad. If the wind decreases, the speed of the ship gets slower, and then the rips come nearer and nearer, and finally come right up to the ship and then disappear. In their place one rip appears on each side of the ship forming with the latter an acute angle and moving farther forward to the bow (Fig. 9, Pl. IV). With a very slight wind from due aft both rips are visible, but if the wind blows from the side, the phenomenon is commonly disturbed on

[1] See the map Fig. 1, Pl. III.

[2] See the note at the bottom of p. 2.

the windward side, where the water is rippled by the breeze. The surface of the water between the rip and the ship's side is smooth and unrippled. The steering is now entirely lost, and the ship turns her stem in the direction of the surface current. It feels as if something were fastened to the ship and holding it back.

"If the wind freshens again, the phenomenon reoccurs in the same manner, but of course, the order is reversed. First, the single rip (Fig. 9) is seen; it moves aft, and when it leaves the stern, the three rips (Fig. 8) are formed. As the speed of the ship increases, these rips draw farther and farther off, and when they are $1^1/_2$ or 2 ship's lenght from the stern, the ship gets rid of the dead-water. The rips however remain, and as the ship sails away, they disappear in the distance to the great satisfaction of the seamen.

"That the vessel may get free, a stronger wind than that which prevailed when it got into dead-water, is generally required. A *sudden* freshening more easily frees the vessel, than a *gradual* increase of the wind, so that in the latter case a greater increase is necessary than in the former.

"When empty, my ship less often took dead-water; the breeze then had to be very feeble and even with the least increase of wind she was free again. This, no doubt, depends on the smaller draught of the ship. I also observed that vessels of smaller draught were less liable to dead-water; deeper vessels, on the contrary, more so. A good sailer sooner gets free than a worse ship of the same draught.

"Both when taking dead-water and in getting free, the change seemed to take place suddenly, although, certainly, the influence of the dead-water (or of the current) made itself felt on the *steering*, even if the ship kept her ordinary speed.

"In *the Sound*, by beating against a fresh, northerly breeze, and with the current going strongly northward, the ship took dead-water every time she tacked, and then kept it during some minutes until she got up her speed again. In a more feeble breeze and with the same current, the ship could not go about, whereas, by veering, one could get her round. In some cases, I have also seen vessels incapable of going round either way, beat back again with backed sails and stern ahead.

"When sailing out of the Baltic, dead-water is not encountered before past *Falsterbo*, and it then gradually grows stronger until out in the Kattegat. There, it is probably strongest between *Læsö* and *Skagen* where it then again diminishes, in some degree, in getting out into the *Skagerrack*. During feeble easterly winds dead-water can be perceived over the whole Skagerrack, and stretches as far as the westgoing current along the Norwegian coast; but farther in the North Sea and off England I never observed it. In the *Gulf of Finland* I have perceived dead-water in the months of June and July, but very feebly.

"I have observed that dead-water always occurs in connection with more or less of a current and that this has been strongest at the surface, whereas, below, there has been either no motion at all or the current has taken an opposite direction. Probably the light water from the Baltic has moved upon the salter North Sea water.

"If sailing out through the Kattegat before a decreasing SW. breeze, the ship, when losing her steering, commonly turns her stem towards NE.; and as the current commonly has the same direction, the ship is soon brought into critical proximity to the Swedish coast, and often we have been forced to drop anchor on this account so as not to drift ashore. I cannot say whether the vessels everywhere turn their stems in the direction of the current, but I have observed it off the Norwegian coast.

"Several methods have been tried to get rid of the dead-water. Thus I have poured oil before the stem, have had a hawser laid under the bottom of the ship and drawn it from stem aft, have worked the rudder, but everything was of no avail. Twice it has happened in the Kattegat that a steamer has crossed our wake just astern, and curiously enough, the ship both times got free from dead-water, without my perceiving any increase of wind."

No. 9. A letter from Mr. A. AANONSEN, shipmaster of Bergen, contains the following statements, as well as the sketches Figs. 2—5, Pl. IV.

"Although, during twenty years practical experience as a sailing-master, I have often had occasion to observe dead-water, I am sorry to say I can offer but scattered and incomplete information.

"I have never experienced dead-water in the open sea, but have done so several times in the *Baltic*, the *Sound*, the *Kattegat*, and the *Skagerrack*. I have experienced it only in spring and summer, and then only in smooth sea and calm weather, or immediately after a calm. The dead-water seems to be limited to scattered areas of rather small extent and with greater intermediate spaces of "living water"[1]. I have observed this especially in the Kattegat, when after some days of calm weather, several sailers were steering in company before a gentle westerly breeze. In such cases, one or more vessels might suddenly lose their steering and remain on the spot, while the others passed freely through the midst of them at a distance as short as two or three ship's length. After a while it was the turn of other vessels to get into dead-water.

"A good sailer is as much exposed to it as the worst tub.

"On the surface, a line of distinction can often be observed between the dead-water round the ship and the "living water"[1] outside. It forms a line, more or less curved, one or two metres from the ship's side. (Fig. 5, Pl. IV). There is however no difference in the colour of the water. The direction of the wake is quite incalculable; sometimes it points to windward, and sometimes to leeward, and sometimes it seems to lead off simultaneously in both directions. The sketches, Figs. 2—5, Pl. IV, are intended to illustrate the appearance of the phenomenon; the wind is supposed to be the same — a light breeze — in the case of all the sketches.

"Fig. 2: The appearance of the wake under ordinary circumstances; speed of the vessel about 5 knots.

"Figs. 3—4: The vessel is influenced by dead-water, she answers her helm, but with difficulty; speed about 1 knot.

"Fig. 5: The vessel lies in dead-water. Agitated spots on the sea, separated by intermediate smooth areas; broken lines of distinction appear and disappear; the direction of the wake undeterminable; speed about 1 knot.

"The worst case of dead-water which I have experienced happened when sailing in to *Bergen*, in the last days of June 1882. My ship was about 40 m. in length and drew $3^3/_4$ m. After previous calm weather and calm sea off the coast, we went in on the fairway. The sea-breeze was fairly up, and we made good speed. At *Kvarven* (see the map of *Bergen* at left hand upper corner of Fig. 1, Pl. III) we were caught by dead-water, and it held us, all the way to *Nordnæs*. We scarcely glided along and were forced to have all sails set, until we were quite near our anchorage. Then the dead-water suddenly let go its hold. Believe me, they were both in a hurry, the ship and the pilot. Braces and falls ran a race together, and we only just got the anchor dropped without any misfortune."

No. 10. Kommandörkaptein JOH. KROEPELIEN, late of the Norwegian Navy, gives the following account of a case of uncommonly strong dead-water:

"I have several times sailed in ships exposed to dead-water both in Norwegian and foreign seas, but only on one occasion did the dead-water so powerfully affect the handling of the ship, that she became almost unmanageable. I remember that at the time both myself and my fellow-officers were quite taken aback at this phenomenon, and the chief, an

[1] The sign of quotation is due to the present writer.

old sailor of great experience did not remember any other similar occasion. Our teacher of Navigation — the late Admiral v. KROGH — always told us that a ship sailing at a rate of 5 knots was sure not to take dead-water, and on this occasion we were making even more speed.

"In the summer of 1857 I was a young officer on board H. M. Schooner *Sleipner*. She was a large, finely modelled, sailing schooner with an uncommonly large sail-area (610 m.^{2}) and famed for her capability of beating to windward. Her principal dimensions etc. at the time were:

Length on water line	28 m.	Draught aft.	4·15 m.
Breadth moulded	8·1 m.	— forward	2·95 -
Displacement	280 m.^{3}	Drag.	1·2 -

"On the morning of the 3rd of July we were lying at anchor in South Varanger in the *Bög Fjord* (see map at right hand upper corner of Fig. 1, Pl. III), at the outlet of the *Pasvik River*, south of *Renö*; the place is indicated on the map by an anchor. At 6·30 a. m. we weighed anchor. From 4 a. m. to 2 p. m. the wind was blowing from the north (strength = 2, perhaps a little more). The log for the preceding day states gentle breezes, (strength = 1) southwesterly in the forenoon, veering to north in the evening. So far as I remember there was no indication of dead-water before we weighed anchor[1]; had that been the case we should scarcely have sailed at all, because with the wind dead against you, it is rather a difficult task to beat out of the Bög Fjord. To accomplish this a strong breeze and other favourable circumstances are needed.

"We had main, top, top-gallant sail and jib set, and we soon made quite a good speed, to the WNW., sailing close to the wind towards *Skoger Island*. The sails bellied out in the fresh breeze, and I remember for certain that the ship, though rather stiff, heeled somewhat. A little after starting, however, the ship all of a sudden, lost her headway without any perceptible external cause, and the turning power of the rudder became nil.

"We then perceived that the ship had taken dead-water. From about amidships, and outwards to both sides and to a considerable distance aft, she was surrounded by a mass of dead-water, smooth as glass, as if the surface were covered with oil. The line between this smooth surface and the water farther out, looked like boiling "rips"[2] and, was quite distinct, the outer surface being strongly rippled by the breeze[3]. The roar caused by the dead mass of water which, clinging to the ship, was dragged along through the water outside, was so loud that it might well have been deemed we were in the vicinity of a rapid. I do not remember the appearance of the wake, nor I believe, was there anything remarkable about it. The rudder was of no use; we were forced to handle the ship by means of the sails and our two boats towing from the bow, and thus we proceeded at a speed of one or two knots.

"In this manner we went on for a couple of hours. All of a sudden, without any known cause, we were set free from the dead-water. The wind had been very steady the whole time, and we had constantly endeavoured to keep the ship in the same course. After being freed from the dead-water the ship got headway, and after a while we logged 7 knots, going close to the wind.

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[1] At the author's request, Mr. KROEPELIEN explains the meaning of this statement in the following manner: "As far as could be seen from the ship, the surface of the sea was evenly rippled by the breeze, without being broken at different spots by smooth, oily areas; as far as I know, seamen have no other indication of dead-water."

[2] See the note at the bottom of p. 2.

[3] This is illustrated by two free-hand sketches made by Mr. KROEPELIEN. The position of the lines of distinction was, according to the remembrance of the narrator, something like that on Fig. 10, Pl. IV. The appearance of the whole phenomenon would be best illustrated by Fig. 1, Pl. V.

"The only reason I can give for the change is that neither at our harbour at *Renö* nor west of the point at *Ellinghavn* had we been so strongly exposed to the fresh-water stream from the Pasvik River as in the middle of the channel. As after leaving Renö we did not keep our reckoning, I cannot verify in the log, the speed given, but I am not far wrong in what I have stated above, for the matter was thoroughly discussed at the time and therefore became fixed in my memory.

"Going towards Skoger Island we went about, but in tacking we got into dead-water again; we, however, succeeded in going about, but the dead-water stuck to the ship even after this. In the same manner as before, we were forced to work our way back again by the help of the sails and the boats towing. We proceeded at a snail's pace and after much struggling we dropped anchor at our last harbour at noon, while a steady fresh breeze seemed to scoff at our helplessness.

"The next day a southerly wind was blowing, and we got safely out of the Bög Fjord and across Varanger Fjord to Vadsö without any more trouble from dead-water."

No. 11. Mr. G. PEDERSEN, shipmaster of Arendal, writes:

"The Brig "Trio", 309 reg, tons and drawing 4·9 m., was on April 12th 1901 at 5 p. m. about 4 miles north of *Lappegrunden*'s lightship (*Helsingör*). Headed SSE. $^1/_2$ E. at a speed of 4 knots through the water, for a westerly, uniform wind. The current in the Sound ran northward.

"The brig got into dead-water — I am unable to say whether it happened suddenly or by degrees. The speed was lost, and the ship was as if nailed to the spot, but she still answered her helm. She did not leave any wake farther aft than about 8 m. from the stern, and the water round the ship was somewhat stripey. A barque sailed past us, without being influenced by the dead-water. To get free, I poured half a gallon of petroleum over the stem — but with no effect. Then I laid the helm hard a port and let the ship get a good swing, and then laid hard starboard, and continued in that manner for a while — with no effect.

"At 8·45 p. m. the ship refused to answer her helm and headed about SE. for half an hour. After that she suddenly went through east to north. I then braced round; the ship steered again, and I veered round through east and headed for the light-ship with no feeling of the dead-water.

"During the whole time from 5 to 9 p. m. we had a fresh westerly breeze, which, under ordinary circumstances, and with all sails set, would have given the ship a speed of 4 or 5 knots.

"The day before, we had dead-water for awhile off *Kullen*, but on this occasion there was only a very light wind.

"The worst regions of dead-water in which I have sailed, are the *Kattegat*, the *Sound*, and the *St. Lawrence River*."

No. 12. Mr. O. OLSEN, late shipmaster, of Kristiania, in the following manner describes a case of dead-water, which he experienced in the spring, several years ago.

"I entered the *Drammen Fjord* with a laden schooner, 140 reg. tons and drawing 4 m. or a little more. The vessel was short, broad, and bluff, but generally steered well.

"We passed *Rödtangen* in the forenoon with a fine breeze from about due aft; the mainsail to port, veered off right up to the shrouds; bonnets starboard. Athwart *Holmsbo* she refused to answer her helm. We tried to manoeuvre with braces and sails, but to no purpose. Then I let brace hard a port, had the bonnets hauled in, laid the helm hard a port; and the ship went up in the wind on the starboard tack. As the whole area of the sails could then work, it all at once appeared as if the vessel had been cut loose from a mooring aft; and, after some manoeuvres, we sailed up to *Svelvig* at a rate of about 5 knots

(the vessel was capable of making 8 knots). The force of the wind was constant during the whole time." —
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"When the ship is moving ahead, leaving a void space astern, the fresh-water on the surface rushes in from aft to fill the wake, thus tending to reverse the action of the rudder; the steering of the ship is, in consequence, destroyed. In fact the fresh-water can be seen rushing after the ship and it makes a great noise."

Effects of dead-water and surface-currents combined.

No. 13. I am indebted to Kommendörkapten F. W. L. SIDNER of the Swedish Navy, for a verbal account, which he has been good enough to revise, of the following interesting accident.

"In July 1899 I was on my way with the corvette *Freja* from *Carlscrona* to *Leith.* The *Freja* is a ship of 2000 tons' displacement, 66 m. in length, 12 m. in breadth and drawing 6 m. Besides sails she is furnished with engine and screw-propulsion, but this was not used in the case mentioned here below.

"On July 11th in the evening, the *Freja* was running before a southerly breeze from *Læsö* towards *Skagen.*

"The breeze, being rather light, was dying down but still gave a speed of 3 or 4 knots. At 7 p. m. — the ship was then probably about 7 miles SE. of *Skagen* — she got into dead-water, which at first mainly manifested itself by bad steering. At midnight the dead-water became more decided, and the speed was reduced. The ship, then about 5 miles east of *Skagen,* was heading northward before the wind, and her course had to be changed to west. She answered her helm and turned through two points, heading NNW., but she could not be brought further westward. An attempt was made to turn her round the other way through north, east, and south to west. She answered till she got two points from the wind — SSE. —, but then the sheer stopped, and she fell off in spite of the action of sails and rudder. These attempts were repeated several times, with just the same result; *within the semicircle NNW.—E.—SSE. the ship could be steered, but not beyond these limits.* She could not be brought to head westward, and we got farther and farther from *Skagen.*

"During this involuntary northward sailing, peculiarities were observed in the surrounding water (see Fig. 14, Pl. IV)[1]. Before the bow on the port side, a low hillock or bank of water was formed at a short distance from the ship's side. Inside this bank and along the whole port side of the ship, the water was "boiling". All the way starboard of the ship the water had the appearance of an eddying current. Furthermore, the ship was followed by *two* sharply defined *wakes,* the one slightly curved to the port side and the other more strongly bent to starboard; between them the water was agitated.

"At half past two next morning, a steam-trawler was seen, heading straight for the *Freja,* which lay still helpless, though the wind had increased. As sailors believe that a steamer can shake another vessel free from the dead-water, we waited with interest to see whether the trawler would come near enough to do us this service. She had the trawl in tow and, therefore, could not have made more than about 3 knots. She advanced to a distance of about $1^1/_2$ cable's length (300 m.) and then turned round and went back again; and actually, when she was closest to us, the *Freja* suddenly got rid of the dead-water and sailed at a good speed, which after a while was logged as 6·2 knots."

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[1] The sketch was made by the present writer and afterwards corrected by Kommendörkapten SIDNER.

The Nautico-meteorological journal of the *Freja* gives the following data, for the time during which the above mentioned accidents occurred.

Date July	Hour	Position of the ship		Wind		Surface-water	
		Lat. N.	Long. E.	Direction (Subject to compass error 11° W.)	Force	Spec. gravity	Temp.C.°
		° ′	° ′				
11	4 a. m.	56 46·7	12 0	SSW.	3	1·009	19·0
	8 a. m.	57 5·0	11 45·7	SW.bS.	3	·011	20·0
	Noon	57 17	11 29·1	South	3	·008	20·0
	4 p. m.	57 21·2	11 8·0	—	3	·009	20·5
	8 p. m.	57 43·2	10 50·0	—	1	·010	20·0
	Mid *t.*	57 46·8	10 51·5	—	1	·010	20·0
12	4 a. m.	—	—	S.bE.	3	·020	19·0
	8 a. m.	57 51·7	10 27·3	SE.	2	·022	20·0

The data in the 7th column are of special interest. They show that the surface-water had a spec. gravity 1·010 in the Kattegat and 1·020 and more in the Skagerack. To judge from the usual conditions prevailing in the Kattegat and Skagerrack, it is probable that the water of spec. gravity 1·010 had formed a shallow surface layer in the Kattegat, reaching out as a shallow tongue north of Skagen, upon water of spec. gravity 1·020. As the transition of spec. gravity at the surface seems to have been rather sharp, the tongue of light surface-water might also have been distinctly separated from the heavier water below; and near the end of the tongue the *Freja* might have sailed in a sharply defined surface-layer of no greater depth than she drew. The surface-layer apparently has had a motion of its own (to the east) upon the water below; this is quite clear from the description of the appearance of the sea round the ship.

An officer on the corvette, Lieutenant O. WALLANDER, has made a somewhat different sketch of the phenomenon (Fig. 15, Pl. IV). This sketch shows no asymmetry, which, however, need not depend on negligence; for when the ship during her manoeuvres headed in the direction of the surface-current, the asymmetry must have been lost, and on such an occasion the appearance illustrated by Fig. 15 might have been observed.

No. 14. Count M. HAMILTON, Kommendörkapten in the Swedish Navy, has communicated to me the following experience, in which a strong surface-current was proved to have played an important part.

"On May 28th 1895 at 2·30 p. m. H. M. Brig *Gladan* was about 3 or 4 miles NE. of *Skagen*, heading WNW., at a speed of about 2 knots; wind ESE. At the hour mentioned sail-drill began, and simultaneously with the shortening of sail, the ship suddenly sheered two points to port. Believing that it depended on the sails being shortened, I intended to bring the ship back to her course after they had been set again. However, the ship could not, by any means be brought to starboard. I then tried to go round to port and in this way get into the desired course. The ship, also, answered willingly; she went round by the wind and fell off on the starboard tack — but only north, and there she stayed. I tried to turn her the first way again, and as before she went by the wind willingly and fell off to leeward to the west, but no further. She could not be brougth into the quadrant between W. and N., while within the 3 other quadrants she could be manoeuvred, and could make about 2 knots. A schooner in the neighbourhood seemed to have

fallen into the same mishap as we. During an hour and a half I endeavoured to get the ship into her course, but to no purpose; then, it became calm.

"In the evening a sounding was taken, and I then found that a strong under-current was running just below the keel of the ship or else that she was carried away by a surface-current of considerable velocity. As soon as the lead was about half a meter below the keel, it was forced under the ship, and the line pressed against the ship's side. Although it was calm during the whole night, the brig next morning was close to the *Pater Noster* light; thus she had been carried by the current across the Kattegat from Skagen.

"In the morning (May 29th) a gentle SW. breeze sprang up, strong enough to enable the fishing boats to sail past and around us, at a good speed; but the brig could by no means be brought out of her course straight towards *Pater Noster*. It was only at about half past five that we, all at once, felt as if the ship had broken loose, and from that moment we had her in our power. We headed without difficulty close to the wind and thus beat southwards at 3 knots, which speed was increased in the course of the day to 4 or 5 knots.

"Several vessels were seen during the day, most of which seemed to feel the dead-water. In such cases they lay with the wind athwart or on the quarter, with head-sails shortened and usually the rudder made fast. A ship could be seen making good head-way; she suddenly fell two or three points to leeward, the steerman worked the rudder to and fro — to no purpose —, then the head-sails were shortened, and we knew that the vessel had got into dead-water.

"Several times the *Gladan* got into dead-water during the day, especially in the neighbourhood of other vessels that lay in dead-water, but she always soon got rid of it again."

No. 15. Mr. MICHAEL LEEGAARD, engineer to the Norwegian Harbour Administration, has kindly communicated to me the following facts with the idea of giving an explanation of the phenomenon of dead-water.

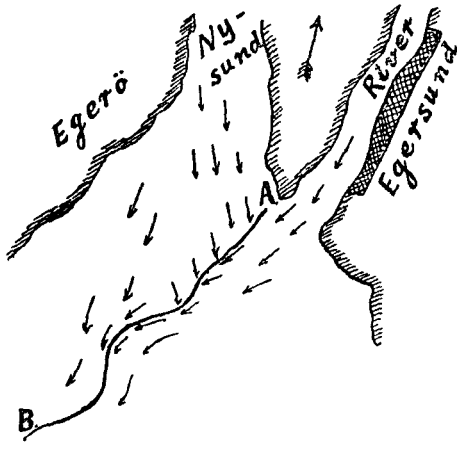

Fig. 1.

"At *Egersund* the river-current and the tidal current from *Nysund* sometimes encounter one another as in Fig. 1. Along the line of encounter there is formed a "rip" or "current-back" *A B*, where the water is "boiling". This rip has the form of a curved line, and remains stationary as long as the conditions are unaltered. If a boat be steered for some distance on either side of the rip and then be left to herself, she will be carried in towards the rip and will lay herself *along* it. Then she will be carried downwards along the rip, following even its smallest windings, all the while being followed by a "boiling" wake aft. The boat cannot be made to turn *across* the rip without some considerable effort, and she immediately turns back into her former direction along the rip." — — — — — — —

— —

"That a sailing vessel with slight wind is inclined to take "dead-water" might be explained with the aid of the facts mentioned. The vessel will be carried by the current into the "rip", there — by the effect of the current impelling her from both sides — she will take her own course along the rip and be slowly carried down stream."

Collection of various accounts of dead-water in Scandinavia.

No. 16. Mr. J. P. POSTI, of Hammerfest, who has navigated the Arctic Sea for 40 years with small sailing vessels of about 30 reg. tons, has favoured me with the following information:

"Dead-water occurs in *the arctic regions* in places where there is fresh water, off river-mouths, or where the snow is thawing on land and ice. It may influence vessels in a slight wind or calm weather. In the *Kara Sea* and *Kara Bay* I have been most exposed to it. – In the Kara Bay it has happened that 8 men in two boats could not tow the vessel loose, before we got help from the wind.

"In the *Alten Fjord* I have also experienced it; inside *Alten-næsset* it is very bad, and it has happened that 4 men could not tow the vessel free, in calm weather."

No. 17. Mr. J. ELLINGSEN, of Kristiania, informs me that dead-water occurs in *Ranen Fjord, Vefsen Fjord, Vel Fjord, Namsen Fjord,* North of *Kristiansund, Romsdal Fjord,* on the north entrance to *Bergen,* and generally off the mouths of great rivers. It occurs in the months of May, June, July, and August.

No. 18. Kommendörkapten J. A. EKELÖF, late of the Swedish navy has told me that once, several years ago, he experienced dead-water in the *Trondhjem Fjord,* when sailing in to Trondhjem with a corvette.

No. 19. The narrative No. 2 (by Captain S. SCOTT-HANSEN) had the following supplement:

"Our pilot told me that he had several times been exposed to dead-water; he specially mentioned one occasion on the *Pudde Fjord* at *Bergen,* when the ship – an 8 knot cargo-steamer – according to the pilot's own phrase, hardly moved from the spot."

No. 20. Mr. M. LEEGAARD, in addition to account No. 15, sends me the following:

"Since the year 1885, I have each summer navigated the coast between *Fredrikstad* and *Bergen* with a cutter, 12 m. long and drawing 2·3 m.

"During these cruisings the vessel has often got into dead-water and almost always at the same spots, mainly at places where there is fresh water, such as off *Larvik* and *Arendal,* off *Kristiansand* (near Odder Island) and in the port of *Egersund.* The dead-water is especially felt when setting sail at such places.

"I have heard, that off the town of *Bergen* dead-water is often met with between *Kvarven* and *Stangen* (about 3 miles south-west of *Kvarven*); between *Kvarven* and *Stangen* there is often strong current."

No. 21. Mr. BERNT LARSSON of Fede, navigation Instructor in Haugesund, has reported the following:

"There is often very strong dead-water in the *Fede Fjord,* especially where the fjord becomes narrow at *Avaren.* It receives water from two rivers, and this fresh water, according to my measurements, forms a surface-layer of half a metre or a little more, in thickness.

"The dead-water is more severe in the first part of the year — till Midsummer — than later. It is mostly met with in westerly or south-westerly winds (sea-wind), probably because a greater quantity of fresh water is then heaped up in the fjord. At all events, *dead-water does not appear in the Fede Fjord during a land-wind.*

"A sharp-built and deep-drawing, laden vessel is more exposed to dead-water than one flat-bottomed and drawing little water. Often, galeases of about 50 reg. tons, drawing laden, 3 or 4 m. of water and sailing at a speed of 6 or 8 knots, take dead-water and run straight towards the shore; while at the same time modern, flat-bottomed vessels of 80 reg. tons drawing only 2 or 2·5 m. are not influenced by it. A vessel most easily takes dead-water through bad steering, or on changing her course."

No. 22. The account No. 38 below (by Mr. OLSEN RÖD) had the following supplement:

"I have several times, before and afterwards, experienced dead-water, though in a moderate degree. Thus I have met with it in the *Kristiania Fjord*, especially between *Fulehuk* and *Rödtangen* and in the *Frier Fjord*. On the North Sea, in the Channel, or on the Atlantic, I have never observed it, and only slightly in the *Baltic*."

No. 23, A Dutch naval officer, Lieutenant G. van HULSTYN, has reported a case of dead-water in the *Kristiania Fjord*:

"In the month of July 1888 H. M. "Nautilus" sailed northward past *Færder* for *Kristiania*. In the fjord we had fair wind of strength 6 (*Beaufort*) and made 5 or 6 knots. At a certain moment the ship lost her head-way, although the sails stood filled. The Norwegian pilot called the phenomenon by the name of "dead-water". After some time the ship again recovered her head-way."

No. 24. According to Mr. L. H. LARSSEN, of *Drammen*, they are much troubled by dead-water there, in spring and autumn. In summer and winter there is dead-water only after strong SW.-winds.

No. 25. Mr. H. HALVORSEN, of Kristiania, writes:

"In the month of July, probably in the year 1887, I was towing a 600 tons' lighter up the East Arm of the *Glommen*. The wind blew astern, and the lighter was being towed at 5 or 6 knots, when off *Futeröd*, she fell into dead-water and did not progress as much as one cable's length (200 m.) during fully three hours. She could not be kept on her course, either by rudder or by help of the towing-rope. This was also the case with another lighter in the same situation. The dead-water let go its hold suddenly."

No. 26. From a letter from Captain A. MACFIE, of Falkenberg, the following may be extracted:

"On *the west coast of Sweden* dead-water is often experienced in feeble wind or calm weather. Very often it occurs off the two mouths of the *Göta River*, but also in other places, where no river disembogues. Among several cases, I have experienced, the following may be mentioned:

"1) With a cargo-steamer of feeble engine power, off *Elfsborg* on the mouth of the *Göta River*. Head-way almost lost.

"2) When towing a large vessel near *Kättan* (inside *Marstrand*). Made no head-way but drifted back with the current. Got free through stirring up the water before the vessel with the screw-propeller, and after that, made 7 knots.

"3) Also when towing a large vessel. Head-way almost lost. Got free through shortening the tow-rope."

No. 27. Captain S. SCOTT-HANSEN has reported a case of dead-water in the *Kattegat*, about 16 miles SE. of *Skagen*.

"It happened to the Norwegian corvette the "Nordstjernen" in the night between May 21st and 22nd 1897. During the time the ship had dead-water, she turned right against the rudder, although the sails stood filled. Aft in the wake there was a splashing sound, as when the wavelets strike a shore — a peculiar sound, which is not heard when the ship makes head-way."

No. 28. Admiral H. KOCH, of Kjøbenhavn, relates:

"The corvette "Valkyrien", probably in the summer of 1853, met with a case of dead-water off *Fornæs*, in Jutland. The wind was southerly — a light breeze; current southerly. For several hours we could not advance by beating, but drifted to leeward with the dead-water to windward."

No. 29. Lieutenant O. WALLANDER relates, that the Swedish frigate "Wanadis" got into dead-water north of *Anholt* in the *Kattegat* on May 13th 1889. Slight, north-easterly winds. After some manoeuvres — all to no purpose — the screw-propeller was set in motion, and then the dead-water disappeared."

No. 30. On Aug. 4th 1899 the corvette "Freja" (see Account No. 13) met with dead-water in the *Kattegat* between *Anholt* and *Kullen*. The wind was very slight, but the ship soon got free again.

In the same summer H. M. brig "Gladan" is said to have met with a case of dead-water in the *Kattegat*.

No. 31. An old sailor, Captain ISAKSEN, of Fredrikstad, has told me that even in the *North Sea* he has observed dead-water; but only under feeble "Catspaws" which, under ordinary circumstances, would have given the ship a slight motion.

No. 32. Mr. SANDSTRÖM, of *Sundsvall* (on the Bothnian coast of northern Sweden) has mentioned the following event that happened there, several years ago:

"A vessel was to be towed away from her haven in the Sound inside *Alnö*; but the tug could hardly move the vessel from the spot; it was as if she were stuck in the mud. Only when they came farther out from the haven did the speed increase, and the towing became easy. Many people had seen the struggle of the tug and were much astonished. Possibly this case may have been owing to dead-water. At the ends of *Alnö Sound* two rivers empty themselves into the sea."

Collection of accounts of dead-water beyond Scandinavia.

No. 33. According to Kommandörkaptein JOH. KROEPELIEN, the late Rear Admiral G. von KROGH, formerly instructor of seamanship, used to speak of dead-water and its causes, in his lectures. He often dwelt on *the troublesome dead-water off the great river mouths of South America.*

No. 34. Admiral H. Koch, to whom I am indebted for account No. 28, also writes:

"In the autumn of 1859 I was mate on a little merchant barque the "Emma Arvigne". When we had got out a little way to sea, off the mouth of the *Orinocco*, sailing for *St. Thomas*, we met with dead-water. The wind was easterly, weather fine. The ship drifted almost crosswise to leeward with the dead-water to windward. She could not go about, but we succeeded two or three times by help of manoeuvres with the sails, in tacking. At last we were obliged to anchor. Some hours afterwards we weighed anchor again and were prepared for the necessity of going west of Trinidad, although we had no chart of this water; but we were now free, and so we sailed to the east of Trinidad."

No. 35. Mr. H. Hansen, shipmaster, of Kristiania, mentions the *Gulf of Mexico* among the waters in which he has been out in dead-water.

No. 36. Mr. G. Pedersen, the narrator of Account No. 11, mentions the *St. Lawrence River* as among the worst regions of dead-water which he has navigated.

No. 37. Mr. P. M. Land, of *Nanaimo* (inside *Vancouver Island*, British Columbia) writes as follows:

"My only experience of the phenomenon occured at the entrance of this harbour, in the light draught twin screw steamer "Mermaid", of which I am master, and under the following circumstances.

"During the winter or rainy season, of 1900—01, while entering the harbour, my steamer, which was drawing 2 m. aft and 1 m. forward, entered a belt of water in which she became almost unmanageable, though she is capable of a speed of 8 knots, and is under ordinary conditions, remarkably easily handled. The best description I can give of the effect produced on the "Mermaid" is, that it was as if a long floating spar were lying fair across her stem, causing her to lose her headway almost completely and making her answer her helm with difficulty.

"Owing to unusually heavy rains, the *Nanaimo* and *Chase Rivers* were discharging a great volume of muddy fresh water, which lay upon the sea water to a depth of half a metre or more. The region of dead-water was about 200 m. across, and I was about 10 minutes in passing through it. The phenomenon is known here by the name you give it, "dead-water". It is frequently met with in the *Straits of Georgia*, off the mouth of the *Fraser River*, and I have heard accounts of it from at least two of the Nanaimo pilots, who experienced delay and annoyance from it while bringing large steamers past that place."

No. 38. Mr. A. Olsen Röd, late shipmaster, of Kristiania, has informed me of a case of dead-water to which he was exposed on *the Mediterranean*. From his letter the following is extracted:

"Late in the autumn of 1857 I arrived in the Gibraltar Straits with the brig "Elise" (29 m. long and drawing 3 m.) and sailed eastward for the Black Sea. After having had much rain of late[1], we arrived in the neighbourhood of *Cape Matapan* with fine weather for several days.

[1] According to a statement in "Beiträge zur Physicalishen Geographie von Griechenland" von J. F. Julius Schmidt, Band I, Athen 1861, it rained during Nov. 1857, 62 French lines (= 140 mm.). For Dec. 1857 no data are given.

Mr. Olsen Röd writes in a second letter: One or two weeks before Christmas we got rainy weather, very welcome because our store of fresh water was quite rotten. It rained almost every day for about a fortnight, sometimes very hard, and we collected 3 large barrels of rain-water.

"On January 2nd 1858 we were between *Cape Matapan* and *Cerigo* and sailed eastward for the Archipelago. The wind was WNW. a gentle breeze, and water quite smooth. We had all sails set and made about $3^1/_2$ knots. At 10 a. m., when we were about 12 naut. miles SW. of *Cerigo*, the brig no longer answered her helm and began to go up northward to the wind. We worked the helm but to no avail. We backed the yards and shivered the braces and made all conceivable manoeuvres, but the ship only turned a little and went back again. The little wind that we had, seemed to be the same as before, and there were many ships in company both to port and starboard of us, which sailed away, whilst we were lying as if at anchor. Yet there was one sail about 3 miles to port of us, in the same predicament.

"In this manner we lay for $1^3/_4$ hours, when the ship began to glide and fall to leeward a little. We then got the head-sails filled and had the aftersails shivering, and without any command of the helm the vessel got down into its course. The most remarkable thing was, however, that when I stood, afore, I saw a long stripe stretching from the bow far over the water on each side dividing the water into two parts. The water aro undthe ship was light gray, but ahead of the stripe it was wholly dark. These stripes seemed by and by to move aft — of course it was the ship that began to glide slowly onward — and after 5 or 6 minutes, when the stripes had passed along the ship and had left the stern and the rudder, then, at that same moment, the ship again answered her helm and made head-way. The wind was about the same — WNW. by W. a gentle breeze. We made 3 knots, but no more, in the afternoon.

"When we approached *Cerigo*, the ship was about to get into dead-water again, but by working the rudder to and fro, we steered again, and after that, we did not feel the dead-water any more.

"The ship, during its long voyage had become very dirty and overgrown with barnacles of 10 or 15 cm. in length, which may have had some effect."

I have received information of several other cases of annoyance experienced by vessels at the mouths of rivers in *America, Africa, Europe,* and *Asia.* I cannot decide, however, whether "dead-water" or currents and whirlpools have been the main cause of these accidents, as they are too scantily described. In general it is mentioned that the vessels have lost their *steering,* but as a rule nothing is said about changes in the speed. The phenomena in question are possibly the combined effect of several causes; for it is obvious that a vessel in dead-water, being deprived of her head-way and steering-power, should become more easily influenced by the disturbing effects of currents and whirlpools.

Four of these cases only, will be inserted below; the others are too incompletely described to be of any value.

No. 39. Captain H. E. PUREY-CUST, R. N., York (England), has kindly sent me the following information:

"It is a common complaint, the difficulty of steering at the mouth of the *Congo River.* The captain of one of the steamers trading there, told me that he always experienced that difficulty after being at anchor there, when on weighing he endeavoured to turn *down* river, and that it was an utter impossibility to do so when going ahead from anchoring,

the only way being to reverse the engine and go astern. I found, myself, great difficulty in steering when going *slow* and at all *across* the current. With the ship stopped across the current, the water would be seen making past the bow and stern down river at a considerable rate, giving the appearance of the ship drifting headside on against the current. On making some rather detailed observations, the reason of this became apparent. The fresh water of the *Congo* running at 3 or 4 knots, on meeting the salt water of the sea some miles inside the actual mouth, passes over it with increased velocity – 5 or 6 knots – and diminished depth, in some cases only a few feet, the salt water below having only a faint tidal motion, so that a vessel's keel may be in still water and her water line in a current of 5 or 6 knots, which amply accounts for the difficulty experienced."

No. 40. M. DECHAILLE, "directeur des signaux de sauvetage", *Havre*, has — in "Journal de Havre" — given the following particulars of a phenomenon, known at the mouth of the *Loire*, by the name of "*les bournes*".

"When a vessel is influenced by this phenomenon, it is said that "le navire est embourné"; she then does not answer her helm until she has got out of the region of the phenomenon.

"Once, I was in command of a 600 ton sailing-ship and had left *Nantes* with a good easterly breeze, able to give the ship a speed of 6 knots. At the mouth of the *Loire* the ship became "embourné" and turned all round two or three times. I succeded in coming out of the "bourne", and then my ship again answered her helm and sailed without any further accident. I do not believe that the "bourne" would have had any effect on a bigger ship, and not at all on a steamer."

An old pilot says about this phenomenon, that it effects vessels at even a velocity of 5 or 6 knots, and even large steamers. He has not observed it except after heavy rains, or when the snow has been melting, or when the *Loire* rises above its banks after a series of storms.

No. 41. Mr. F. PICAMILH, *Bordeaux*, has kindly sent me the following information respecting the river *Garonne*, in the neighbourhood of *Bordeaux*.

"The river is always very muddy, so that objects left on the shore during one high tide may become covered with several millimetres of mud. In the summer, which is the dry season, the water in the river is considerably reduced, and its level sinks. Then the rising tide stirs up enormous quantities of the deposits from the bottom, giving to the river the appearance of a river of mud.

"Seamen then say: "*les eaux sont lourdes*". Vessels steer with difficulty; but more particularly lighters cannot navigate the river in the usual way by the help of the current.

"For navigating the river, these lighters, having an anchor dragging at the bottom, are left to drift with the current. If the vessel should be moved in a direction different to that of the current, she is made to swing out by the help of the rudder, as indicated by Fig. 2. But when "les eaux sont lourdes" it is, sometimes, impossible to perform this manoeuvre: The lighter does not answer her helm, and in spite of a strong current impelling the rudder, she remains immovable in the direction of the cable (Fig. 3)."

No. 42. Mr. PICAMILH has told me of another peculiarity experienced in the basin at *Arcachon* west of Bordeaux.

"This basin is filled with sand-banks, which during the high tide under water, are at low tide above the water-level, and are separated by deep, navigable channels. The water in the basin is pure Atlantic water, and the rivers disemboguing there, carry but little water.

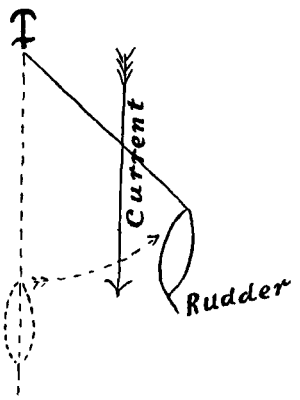

Fig. 2.

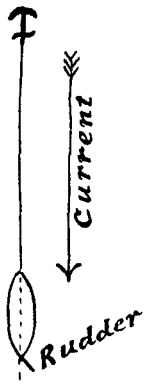

Fig. 3.

"In this basin it sometimes happens that sailing-vessels in a slight breeze, cease for some minutes to answer their helm. This phenomenon is in particular observed by yachtsmen, during the summer-regattas in the basin. Then, in slight wind and smooth water, they fear the places, "where the yachts do not answer their helm". These places are, however, of very small extent, and the phenomenon is rather rare."

Accounts from ancient authors.

No. 43. Several old Roman authors especially *Pliny the Naturalist,* tell us of events when ships have been suddenly stopped and deprived of their head-way.

In his *Historia naturalis,* Book IX, Chapt. 41, PLINY narrates that a ship, carrying the boys of noble families, who by the order of Periander the despot should be castrated, was checked on her way, though she had all sails set. The cause was attributed to a species of Mollusc (*Murex*), which by affixing itself to the vessel, stopped her speed and thus saved the boys.

Commonly the checking power was attributed to a fish, *Echeneis Remora.* This fish has on its back a haustellum by means of which it can affix itself to the rocks or to floating bodies as for instance, to vessels. PLINY says that but one of these fish kept back a vessel even in the strongest wind; and to produce this effect it only needed to affix itself to the body of the vessel. Thus in the naval battle of *Actium* it held ANTONIUS' own ship fast, so that he was obliged to board another vessel. (PLINY. Historia Naturalis, Book XXXI, Chapt. 1).

At the same place PLINY mentions another event which happened in his own time: CALIGULA the emperor, on his way from *Astura* to *Antium,* was detained, because one single vessel of his whole fleet was stopped and could not move from the spot. The vessel — a quinquereme — was overhauled, and a *Remora* was found sticking to the rudder. When the fish was loosened and brought before the emperor, to the emperor's great astonishment, its power was gone, and the ship could be rowed again.

No. 44. A similar story is narrated by BARTOLOMEO CRESCENTIO ROMANO in his "*Nautica Mediterranea,* printed in Rome in the year 1607. This

narrative, which has been kindly sent me by Mr. SALVATORE RAINERI, secretary of the journal "*Lega Navale Italiana*", Genoa, when somewhat abridged, runs as follows:

"— — — — and I must tell you about another deed of the devil, because you must know in how many ways this enemy of mankind works against poor seamen. — — — —

"On a voyage from *Gaeta* to *Napoli*, the Galley "S. Lucia", when sailing before a fresh wind and being two miles from Port, stopped quite immovable in spite of her sail being strained. The steerman examined the rudder to see whether there was some rope or net fastened to it, and as nothing was found, he commanded the oars to be got out and the galley-slaves to be forced on with hard blows. But the galley did not move from the spot, and when she had been lying motionless for a quarter of an hour and more, the other galleys, which had sailed on, shortened sails, waiting. Then a man named CATELANO, told the captain — — to have three monks removed from the deck of the galley and averred that the galley would then immediately begin to move; and when the captain had had them removed, the galley certainly did begin to speed like an arrow.

"Then all the men were about to throw these three poor fellows into the sea, saying that they were excommunicated; but the same man CATELANO helped them saying, that this was a stratagem of the devil to the detriment of the monks; and he obtained permission that they should only be taken from the vessel.

"This occurrence would have caused scientists to suppose that a very small fish, resisting the progress of the vessel, had got the better of the force of sails and oars and made the vessel stop. — — —"

No. 45. In TACITUS, "Agricola", Chapt. 10 (treating on the geography of Britain), it is said:

"Thule[1] was also seen, previously hidden by snow and winter; but *the sea is said to be tough and hard for the rowers* (sed mare pignum et grave remigontibus) and to be little stirred by the winds. — — —"

If search were made all through ancient literature, there is no doubt that a more ample collection of tales would be obtained similar to the few here inserted. There are moreover, stories about submarine, magnetic rocks, or supernatural forces fettering the vessels in certain places on the sea, and possibly, some of these have their real basis in cases of dead-water.

[1] Probably *Norway*.

II.

PRELIMINARIES.

A. THE RESISTANCE TO VESSELS UNDER ORDINARY CIRCUMSTANCES[1].

Imagine a vessel towed on a hawser. When her speed has become uniform, the resistance is evidently equal to the stress in the tow-rope; its immediate cause is the sternward resultant of the water pressure, against the vessel. In this pressure is then included, the "tangential pressure" or *friction* of the water against the sides of the vessel. Further, the resistance is equal to the work done in towing or propelling the vessel a unit of distance; this work is heaped up as energy (heat and *vis viva*), communicated by the vessel to the surrounding water. The resistance may consequently be regarded from two points of view: either, as *the resultant of pressure* (and friction) *of the water against the vessel* or, as *equal to the energy communicated to the water* per unit of distance covered by the vessel. One must, of course, be careful, not to use these two views simultaneously, so that the resistance, or part of it, be not taken into account twice.

If the water were frictionless, and if its surface could be held plane as under a sheet of ice, *vessels would,* according to a well known hydrodynamical theorem of KIRCHHOFF[2], *experience no resistance,* when moving

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[1] Part of the substance of this section is drawn from Chapter XI of Sir W. H. WHITE's very interesting "Manual of Naval Architecture", London 1900, and from Chapter IX of the excellent "Hydrodynamics" of Prof. HORACE LAMB, Cambridge 1895. To these books reference may also be made for further information on the subject.

[2] Vorlesungen über Mathematische Physik, von G. KIRCHHOFF. Mechanik. Neunzehnte Vorlesung § 2.

at a uniform rate in open water. The physical reason for this is, shortly expressed, that under these suppositions the motion of the water depends only on the actual velocity of the vessel; the *vis viva* of the water consequently remains unchangeable, and as mechanical energy cannot, in a frictionless fluid, be transformed into heat, no energy at all is communicated to the water by the vessel.

In actual practice *the water has a free surface.* The pressure of the vessel against the water then causes a disturbance of level, which persists in the form of waves. These gradually spread over all the sea behind the vessel, and the *vis viva* of the water thereby continually increases. The work necessary to generate this *vis viva* must be spent by the vessel and causes a certain *"wave-making resistance"*.

Further, water is a *viscous* fluid, and this circumstance will in a twofold way cause an alteration of the motion, and the addition of new causes of resistance. Firstly, the water, flowing past the ship's sides, exerts on them a sternward tangential stress — *"frictional resistance"*. Secondly, the motion which, though unstable, would theoretically take place in a perfect fluid, is disturbed by the viscosity; the wake, which would otherwise be smooth and even, becomes irregular and eddying, and the steady supply of energy required for creating new eddies is a cause of resistance — *"eddy-making resistance"*.

Elaborate experiments have been made for determining the relative importance of each of these factors in contributing towards the sum-total of resistance. The *frictional* resistance is found to be quite preponderant at low speeds, and even for fast vessels going at full speed, it amounts to nearly a half of the whole. The *eddy-making* resistance is rated at about 10 per cent of the former (in well-formed vessels), and the rest is *wave-making* resistance. These numbers are under the supposition that the water is not shallow (see the next section of this chapter). The *frictional* resistance is approximately proportional to the square of the vessel's velocity.

The wave-making resistance is of special interest to us, because it is the chief cause of the speed-reduction in "dead-water"; the following general remarks, though referring particularly to wave-making resistance under ordinary circumstances, are, with some modifications, applicable to the case of a vessel moving in dead-water.

The waves following a vessel in rapid motion consist, as we are accustomed to seeing, of two different systems, which are both easily seen in the photographic illustration Fig. 3, Pl. XII. One set of waves, the *transverse waves* (best seen on the left-hand side of the figure), stretch right across the wake, bending slightly aftwards; the other, the *diverging waves* (best seen on the right-hand side) stretch out from the stem as wedges each one more extensive than the preceding one.

The shape of these waves has been calculated mathematically under the simplifying supposition that the vessel producing them is infinitely small[1]. The error caused by this simplification is, as far as interests us at present, without appreciable influence on the shape of the waves at a distance from the vessel. Fig. 3, Pl. V shows the crest-lines of both systems of waves calculated in this way, in the case of deep water. The vessel must be imagined to be at the point at the extreme left of the figure. Theoretically the diverging waves should fill the whole space to both sides of the middle line, but in the proximity of this middle line there would be an infinite number of infinitely short waves which cannot be drawn on the figure. As a matter of fact, the diverging waves are never perceptible in the middle of the wake.

Really, the stem and the stern of a vessel each produces both these systems of waves; but owing to the shape of the vessel, the transverse bow-waves and the diverging stern-waves are very low and are easily overlooked.

The wave-length depends upon the velocity v, of the vessel and upon the direction of the crest-line of the wave considered: in order that ship and wave-system may keep the same position relative to one another, the velocity c of a wave must be in a fixed ratio to v. — The *transverse* waves, for instance (or more properly the central part of them) must have the same velocity v as the vessel; the diverging waves, because moving in different directions to the vessel, must have smaller velocities. If α be the angle between the wave-crest and the course of the vessel, the velocity of the wave must evidently be $c = v \sin \alpha$ (see Fig. 3, Pl. V). — On the other hand, c is a function of the wave-length, varying, as the square root of the latter, in deep water. The transverse waves are consequently longer than the diverging waves (longer, the greater is α), and the lengths of all the waves increase if the velocity of

[1] See LAMB, Hydrodynamics, Art. 228.

the vessel be increased. If the waves were shorter than indicated by the above rules, they would move more slowly than the vessel, and would gradually move away from her; whereby the wave-length and the velocity of the head-most wave would increase until the wave be long enough to follow the vessel. Similarly, the next waves, one after the other, would have their wave-length and velocity increased.

In calculating the work done by the vessel in maintaining the above described wave-system, it must be observed, that part of the energy wanted, is constantly transmitted from behind, by the waves already created; but another part of the energy of the latter is always left behind, and a corresponding quantity of energy must constantly be procured by the vessel. This may be illustrated by a very common phenomenon. If a stone be thrown into smooth water, there is initially formed *one* circular wave which quickly expands; but one will very soon observe that there are *two* waves, one in the place of the original wave and one outside it. Both these waves are lower than the original one. This partition of the waves proceeds incessantly; the wave-rings become larger and larger in number and smaller and smaller in height, the middlemost wave being always the highest. But if the motion of the original wave-ring be carefully followed, one will find that it always remains the outermost one. Thus, the average velocity of transmission of the wave-energy, is smaller than the velocity of the waves themselves — really, the ratio is exactly one half in water of infinite depth[1]. This ratio — *i. e.* the ratio of the energy really transmitted across an imagined vertical plane in the water, to the energy which *would* be transmitted if the waves propagated themselves undivided with their actual velocity — may be called *the ratio of transmission of wave-energy*[2]. The ratio of transmission of wave-energy is consequently 0·5 in water of infinite depth.

From the waves following a vessel in uniform motion, the wave-making resistance may then be estimated for the case of *deep water*, in the following simple way. Imagine two straight lines AA' and BB' (Fig. 3 Pl. V) stretching in the water-surface at a distance l from each other across the wake of the

[1] See LAMB, Hydrodynamics, Art. 221; see also a highly interesting paper on the subject by Prof. O. REYNOLDS, "Nature", XVI p. 343 (1877).

[2] Not to be confounded with "the *rate* of transmission of wave-energy", which is another notion.

vessel, and fixed in space; for simplicity let l be a multiple of the wave-length of the transverse waves. The energy of the *transverse waves* between AA' and BB' may be $l \times E_1$. As the height of the waves and their position relative to the vessel are invariably the same, the energy of transverse waves ahead of BB' is increased by $l \times E_1$, when the vessel has moved a length $= l$. In the same time nearly *half* this energy[1] is carried by transverse waves across BB', and consequently the rest or about $\frac{1}{2} l \times E_1$ must be supplied by the vessel. Secondly consider the *diverging waves.* The energy of them between the lines AA' and BB' may be $l \times E_2$. The average velocity c of the diverging waves is $c = v \sin \alpha$, and its component in the direction of the vessel's keel-line is $v \sin^2 \alpha$, v being the velocity of the vessel, and α some mean value of the angle between the diverging wave-ridges and the keel-line of the vessel. When the vessel moves forward l units of length ($= AB$), the energy $\frac{1}{2} E_2 \times l \sin^2 \alpha$ is therefore carried across BB' by diverging waves; the energy of diverging waves ahead of BB' is increased by $l \times E_2$, because the motion is steady, and consequently the energy $\frac{1}{2} l \times E_2 (2 - \sin^2 \alpha)$ must be supplied by the vessel. Thus, when the vessel moves a distance l, the wave-energy to be created by her is, in all, about $\frac{1}{2} l \times E_1 + \frac{1}{2} l \times E_2 (2 - \sin^2 \alpha)$, and the value of the wave-making resistance consequently approximates to $\frac{1}{2} E_1 + \frac{1}{2} E_2 (2 - \sin^2 \alpha)$. Greater accuracy could be obtained by further dividing the wave systems into smaller parts, so that within each of them, the extreme values of α differ less from each other. The analysis above, gives, however, an idea of the degree in which the waves contribute to the resistance of a vessel; it shows that diverging waves cause comparatively more resistance than transverse waves, because the energy of the former is in a smaller degree propagated with the vessel.

It is well known that a vessel moving at slow speed, creates but very low waves, and as the energy of waves varies as the square of their heights, the wave-making resistance is in this case inconsiderable. At moderate speeds the wave-making resistance increases, in deep water, approximately as the square of the velocity, but at higher speeds it increases as the cube — or even according to an higher power.

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[1] It would be exactly one half, if the transverse waves were perpendicular to the keel-line of the vessel throughout their whole length.

B. RESISTANCE TO VESSELS IN SHALLOW WATER.

The general principles explained in the preceding section are quite independent of whether the vessel is moving in deep water or in shallow water; but nevertheless the law connecting velocity and resistance is essentially different in the two cases.

This subject, investigated more than 60 years ago in a number of canals in Scotland, is of great interest to us, because it offers a remarkable analogy to the laws of resistance in "dead-water". SCOTT RUSSELL describes in the following manner the discovery of the peculiar fact which induced further investigations upon the matter.[1]

"As far as I am able to learn the isolated fact was discovered accidentally on the Glasgow and Ardrossan Canal of small dimensions. A spirited horse in the boat of WILLIAM HOUSTON, *Esq.*, one of the proprietors of the works, took fright and ran off, dragging the boat with it; and it was then observed, to Mr. HOUSTON's astonishment, that the foaming stern surge which used to devastate the banks had ceased, and the vessel was carried on through water comparatively smooth, with a resistance very greatly diminished. Mr. HOUSTON had the tact to perceive the mercantile value of this fact to the Canal Company, with which he was connected, and devoted himself to introducing on that canal vessels moving with this high velocity. The result of this improvement was so valuable from a mercantile point of view, as to bring, from the conveyance of passengers at high velocity, a large increase of revenue to the Canal proprietors. The passengers and luggage are conveyed in light boats, about sixty feet long and 6 feet wide, made of thin sheet-iron, and drawn by a pair of horses. The boat starts at a slow velocity behind the wave, and at a given signal it is by a sudden jerk of the horses drawn up on the top of the wave, where it moves with diminished resistance, at a rate of 7, 8 or 9 miles an hour."

SCOTT RUSSELL was appointed to investigate the matter and arranged great experiments[1] with vessels 10—20 m. in length and weighing 600—9000 kgr. Fig. 4, reproduced from SCOTT RUSSELL's paper, represents the resistance at different velocities according to these experiments. The velocities are plotted along the horizontal *Y*-axis in statute miles[2] an hour, and

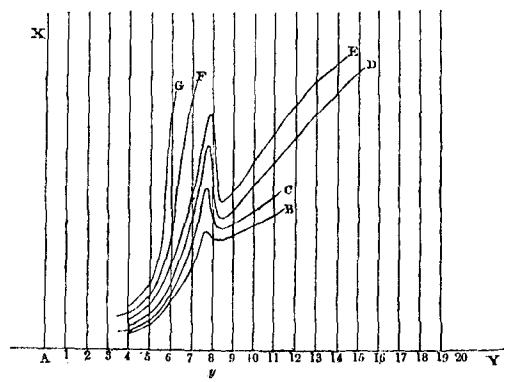

Fig. 4.

[1] JOHN SCOTT RUSSELL, "Experimental Researches into the Laws of Certain Hydrodynamical Phenomena that accompany the Motion of Floating Bodies, and have not previously been reduced into conformity with the known Laws of the Resistance of Fluids". Trans. of the Roy. Soc. of Edinburgh. Vol. XIV, 1840.

[2] One statute mile is 1609 m.

the resistance (against 6 different vessels) vertically with an arbitrary unit. As shown by these curves, *the resistance was a maximum at a certain critical speed,* which SCOTT RUSSELL found to depend upon the depth of the canal. *If the speed was further increased, the resistance rapidly fell down to a minimum and then again increased*[1].

The decrease of resistance at the critical velocity was accompanied by remarkable changes in the general features of the motion, illustrated by Figs. 5—7 below, which are reproduced from SCOTT RUSSELL's paper. Fig. 5 represents the vessel at rest or with a slow motion. *When the vessel was forced*

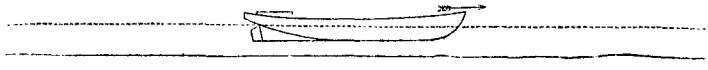

Fig. 5.

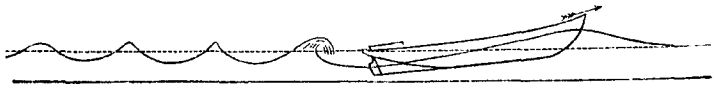

Fig. 6.

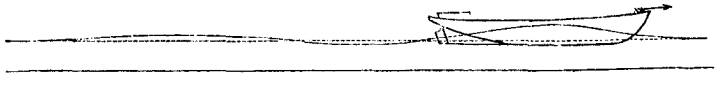

Fig. 7.

up towards the critical velocity (see Fig. 6), *the waves aft of the stern, increased strongly, and the vessel pushed before her stem a large wave of the type called by* SCOTT RUSSELL "*Solitary wave*", a single elevation of water travelling along the canal. *When the critical velocity was exceeded, the breaking stern-waves disappeared, and the vessel moved on the top of* the "*solitary wave*" (Fig. 7); at the same time her stern, which was before sucked downwards, was now raised so that the vessel drew considerably less water than when at rest.

The above described, peculiar facts depend on the laws of motion of waves in shallow water. As was discovered by SCOTT RUSSELL, waves of any length

[1] SCOTT RUSSELL believed the existence of a second maximum and of a second minimum of resistance at certain speeds, to be greater than could actually be produced by experiments; but the theoretical speculations from which these conclusions are drawn are, as far I can see, entirely erroneous.

cannot move spontaneously at more than a certain speed $\sqrt{gd}$, d being the depth of the canal and g the acceleration due to gravity[1]. This maximum velocity of waves is just the critical velocity at which the resistance of a vessel began to diminish; when moving at a greater velocity, the vessel cannot be followed by any transverse waves (compare p. 35), and as the work required for creating these waves is then saved, there must be a corresponding decrease of resistance[2]. If resistance were not caused by friction and by the diverging waves, the vessel would move without resistance, at velocities above the critical one. It will be made clear, in the next chapter, that it is just the *transverse* waves, which are of preponderant influence on the resistance in shallow water, as well as in "dead-water".

The influence of shallowness is practically insensible, as long as the depth of the water is more than half the length of any wave actually created by the vessel. The velocities of the waves are, in this case, in practically the same ratio as the square roots of their wave-lengths, and the ratio of transmission of wave-energy is $1/2$ — just as in deep water. But when the velocity of the vessel is increased, the wave-velocity and the wave-lengths correspondingly increase; when the wave-length becomes more than twice the depth of the water, the laws of motion of the waves are sensibly altered: the wave-velocity increases more and more slowly, approaching assymptotically its maximum value $\sqrt{gd}$, when the wave-length increases. At the same time the ratio of transmission of wave-energy gradually increases from 0·5 to 1; *i. e.*: the longer be a wave, the smaller part of its energy will be left behind, and the more slowly will it diminish in height while travelling. Very long waves consequently may travel long distances spontaneously without diminishing very much in height and without leaving waves behind them; in this respect SCOTT RUSSELL's "solitary waves" comport themselves as very long waves. If the velocity of the vessel be a little below the critical velo-

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[1] SCOTT RUSSELL particularly studied a peculiar sort of wave which he called "*solitary wave*". It consists of a single elevation travelling spontaneously without altering its shape. If the depth of the canal be d and the height of the elevation be h, the velocity of such a wave is $\sqrt{g\,(d + h)}$, that is, somewhat more than the critical velocity $\sqrt{gd}$.

[2] This is more rigorously established by a mathematical investigation of Lord KELVIN "On Stationary Waves in Flowing Water", *Phil. Mag.*, Oct., Nov., and Dec. 1886.

city, the transverse waves, in order to follow the vessel, must be very long, and a comparatively small part of the wave-energy, is therefore left behind. In consequence, the transverse waves, at this velocity, cause a comparatively smaller resistance than equally high but shorter waves at a smaller velocity, would do. On the other hand, as they follow the vessel spontaneously to a greater extent, they will, by accumulation, reach a greater height, than they would at the smaller velocity. The result of these two circumstances, acting in opposition, is, as mentioned on p. 39, that the resistance increases to a maximum at the critical velocity $\sqrt{gd}$, and then suddenly falls off to a lower value. Lord KELVIN's mathematical investigation (*l. c.* p. 40) gives substantially the same result, as will be mentioned below.

C. WAVES IN THE BOUNDARY BETWEEN SALT AND FRESH WATER.

When a layer of light fluid rests on the top of a heavier one, waves can be created in the boundary between the two fluids, as well as in the free surface. The mathematical theory of such waves has been developed by Sir G. G. STOKES[1]. The chief properties of them, as well as a few simple approximate formulae for which we shall have use later, are collected here below; the analysis not inserted here, may be found in STOKES' paper or in LAMB's "Hydrodynamics" Art. 223. All the formulae are founded on the supposition that the waves are of small or moderate heights, so as to be approximately conformable to *linear* dynamical equations.

Such "boundary waves" may easily be demonstrated as follows: half fill a tumbler (or better, a square glass-vessel) with a salt solution, and lay on the surface a thin disc of cork. Then, by help of a glass rod or a lead-pencil, pour fresh water in a fine stream on to the disc, and, with a little care, there may be obtained a quite sharp boundary between the two water layers. If now the glass be given a suitably slow oscillatory movement to the right and to the left, the boundary between the two water-layers is put into great uninodal oscillations while the upper surface remains almost still. The less the difference of specific gravity between the two water layers, the slower are the

[1] On the Theory of oscillatory waves, Math. and Phys. papers, vol. I, pp. 212—219.

oscillations; and if the salt-water has a specific gravity of only 1·02 or 1·03, one can produce even binodal and trinodal oscillations in the boundary, without disturbing the upper surface.

In a basin of larger dimensions, progressive waves, moving in the same manner as ordinary surface waves, may be created in the boundary[1]. The waves in the boundary produce a similar wave-motion in the surface, although of considerably diminished amplitude; the surface has elevations immediately above the depressions in the boundary and *vice versâ*, as is illustrated by Fig. 4, Pl. VI. The former places, where the fresh-water layer is thickest, may be called *wave-crests*, and the latter *wave-hollows*. The motion of the water particles at the wave-crests and the wave-hollows, is represented in the figure by small arrows, the large arrow denoting the direction of the wave-motion.

The relative height of the surface waves depends on the difference of density between the two water-strata. If

q = the density of the fresh-water,
$q + \Delta q$ = - - - - salt-water,
l = the wave-length (from crest to crest),
d = the depth of the fresh-water layer,
D = - - - - salt-water layer,

and if D is great compared with d, the ratio of the amplitudes, h and H, of the surface-waves and of the boundary-waves is

$$\frac{h}{H} = \frac{\Delta q}{q} e^{-\frac{2\pi d}{l}} \quad \ldots \ldots \ldots \ldots \ldots \quad (1)$$

If the depth of the surface-layer is very small compared with the wave-length, the ratio of the amplitudes is approximately

$$\frac{h}{H} = \frac{\Delta q}{q}, \quad \ldots \ldots \ldots \ldots \ldots \quad (2)$$

i. e., there is statical pressure-equilibrium in the salt water. If the depth of the surface-layer is considerable, compared with the wave-length, the ratio $h : H$ is, according to (1), smaller than $\Delta q : q$.

[1] For the sake of brevity, I shall call the upper water-layer *fresh water*, and the heavier water below *salt water*, although, in reality, the two water-layers may consist of salt water of different densities. Likewise by the *surface* we may always understand the upper, free surface, and by the *boundary*, the common boundary of salt and fresh water.

In all cases actually concerning us, *the difference Δq between the densities of the two water-layers, is very small,* the specific gravity of sea-water being always less than 1·03. In what follows this should always be assumed to be the case. *The surface-waves are then,* according to equations (1) and (2), *very low,* compared with the boundary-waves by which they are caused. In Fig. 4, Pl. VI, the vertical scale of the *surface*-waves is exaggerated about 30 times, if we assume the water-layers to be made up of fresh-water and pure sea-water of spec. gravity 1·03.

The most peculiar property of the boundary-waves, in contradistinction to ordinary waves on the surface of homogenous water, *is their very slow velocity* (Δq assumed to be small). With moderate wave-lengths it varies as the square root of Δq and stands to the velocity of equally long waves in homogenous water, as $\sqrt{\Delta q} : \sqrt{2q}$ [1]. As the wave-length increases, the velocity approaches the value

$$v_m = \sqrt{\frac{\Delta q}{q} \frac{g}{1/d + 1/D}}, \quad \ldots\ldots\ldots\ldots \quad (3)$$

which is its maximum value; or, if D be great compared to d,

$$v_m = \sqrt{gd} \times \sqrt{\Delta q/q}. \quad \ldots\ldots\ldots\ldots \quad (4)$$

This formula shows an obvious analogy between boundary-waves and waves in shallow homogenous water, the maximum velocity of the latter being $\sqrt{gd}$. There are indeed several such analogies, as will be shown below.

The average energy per unit area *of the wave-systems considered,* is also small, being proportional to the difference of density Δq, in cases in which the heights of the waves are the same. It is exactly twice the *potential* energy of the waves and consequently varies, as in the case of ordinary surface-waves, *as the square of the wave-amplitude.*

Just as in the case of waves in shallow water, the *ratio of transmission of wave-energy* (see p. 36) is 1/2 for short waves and increases asymptotically to 1, when the wave-length increases. In Fig. 1, Pl. VI, it is represented as a function of the ratio v/v_m of the wave-velocity v and the maximum velocity v_m of long waves in the same water. The ratio of transmis-

[1] This formula, as well as (3) and (4), are exact under the supposition that Δq is infinitely small; if $\Delta q/q$ is small, they are approximately true.

sion of energy is plotted vertically. The faint curve is valid for waves in *shallow water*, as well as for *boundary-waves* when *the two water-layers have the same thickness*. If the two water-layers are of different thickness, the ratio of transmission of energy becomes somewhat altered, and is represented by the heavy curve, if one of the water-layers — say the salt water — is by comparison, *infinitely deep*. In either case the curves show that for waves moving with not more than half the maximum wave-velocity, the ratio of transmission of energy is almost exactly 1/2, just as in deep water. Only when the velocity of the waves is further increased, do their laws of motion begin to be influenced by the shallowness of the water-layers.

The pressure in the water is, of course, altered by the wave-motion, but differently in the salt and in the fresh water-layer. p may be the pressure when the water-layers are at rest, and $p + \Delta p$ when there are waves. Then Δp is positive at the wave-crests and negative at the wave-hollows, in the *fresh-water* layer, and *vice versâ* in the *salt-water* (see Fig 4, Pl. VI). In the fresh-water layer

$$\Delta p = \tfrac{1}{2} g h' q \left(e^{\frac{2\pi(y-d)}{l}} + e^{-\frac{2\pi(y-d)}{l}} \right),$$

and in the salt water, when D is infinite,

$$\Delta p = -\tfrac{1}{2} g h' (q + \Delta q)\, e^{\frac{2\pi y}{l}} \left(e^{\frac{2\pi d}{l}} - e^{-\frac{2\pi d}{l}} \right),$$

where y is the height of the particular point considered, above the mean level of the boundary (y negative in the salt water), and h' is the height of the water-surface above its mean level, in the same vertical as the considered point. The other symbols have the same signification as before.

If the wave-length is very great compared with the depth of the surface-layer and with the distance between the boundary and the considered points in the water-layers, $\frac{2\pi d}{l}$ and $\frac{2\pi y}{l}$ are small quantities. Then the formulae above take the following simple forms; in the fresh-water layer,

$$\Delta p = g h' q, \quad \ldots \ldots \ldots \ldots \quad (5)$$

and in the salt-water

$$\Delta p = -g h' (q + \Delta q) \frac{2\pi d}{l}. \quad \ldots \ldots \ldots \quad (6)$$

From equation (1) p. 387 in LAMB (*l. c.* p. 33), *the velocity of a water-particle in its orbital motion,* may be calculated. It follows that it is proportional to the wave-height, and is always less than the velocity of propagation of the waves, *i. e.* the wave-velocity. The velocity of a fresh-water particle in a wave-crest would, in the case of *very long waves,* be equal to the wave-velocity, only if the wave-height H (from the mean level of the boundary, to its highest level) be equal to the mean depth d, of the surface-layer. As this is impossible, it only shows that in the case of long waves, the velocity of the water-particles is always less than the wave-velocity, as long as the wave-height is moderate enough for the waves to be approximately conformable to *linear* dynamical equations. In the case of short waves (wave-length less than two or three times the depth of the water-layers) the greatest horizontal velocity of the water near the boundary, should be equal to the wave-velocity, only if the wave-height H be $1/6$ (or $1/2\pi$ exactly) of the wave-length; and the velocity of the water near the surface is then considerably smaller. This wave-height — $1/6$ of the wave-length — is about the extreme limit of the height of waves, if they are not breaking.

It is very easily proved that, when restricting us to two-dimensional waves of a permanent type, the orbital velocity of the water-particles can be as great as the wave-velocity, only when the waves have their extreme height; or in other words, the orbital velocity can never be *greater* than the wave-velocity, as long as the waves do not break. For when in the contrary case, the wave-motion is reduced to steady motion by superposing a velocity equal and opposite to the wave-velocity, the fresh-water at the very wave-crests, would have a velocity *opposite* to the general flow of water; and this is inconsistent with the representation of the steady motion by means of stream-lines. In the extreme case in which the velocity of the fresh-water (in the imagined steady motion) vanishes at the crests, these must be infinitely curved so as to form sharp angles. This extreme case, however, cannot be attained by boundary-waves. For, as can be immediately seen by drawing the stream-lines in the immediate proximity of a wave-crest; the velocity of the salt-water would then be infinite there. The velocity of the water-particles in the boundary-waves, is therefore always *slower* than the wave-velocity.

D. THE EFFECT OF A LIGHT SURFACE-LAYER, ON THE RESISTANCE OF A VESSEL.

As long as we confine ourselves to a merely qualitative examination, it is not difficult to see in what manner a lighter surface layer resting upon the salt water, may influence the resistance of a ship.

First consider a simple case, which can be treated by mere statics. The vessel may be supposed to be dragged slowly in a narrow square-sectioned channel, and her bottom to reach into the salt water. She may be quite flat-bottomed and with vertical sides, fitting to the walls of the channel, so as to allow no water to pass except than under her bottom. When the vessel begins to move, the fresh-water must then be heaped up ahead of the vessel and become thinner aft of her (see Fig. 6, Pl. VI), and as there must be statical equilibrium in the salt water below the vessel, the upper surface rises ahead and sinks aft. The vessel is in consequence, resisted by a sternward pressure easily calculated. The depth of the fresh water, fore and aft, may be h_1 and h_2 respectively, and the densities of fresh and salt water q and $q + \Delta q$ respectively. In consequence of the statical equilibrium, the difference of level of the salt water, fore and aft, is then found to be

$$h_3 = (h_1 - h_2) \times q/(q + \Delta q),$$

and the difference of level of the upper surface

$$h_1 - h_2 - h_3 = (h_1 - h_2) \times \Delta q/(q + \Delta q).$$

The breadth of the channel being b and the acceleration due to gravity g, the resultant of pressure against the vessel is when $\Delta q/q$ is small, then found to be approximately

$$gb\Delta q \frac{h_1{}^2 - h_2{}^2}{2} \quad \ldots\ldots\ldots \quad (7)$$

As the vessel moves along, the difference of levels — and the resistance — grows larger, until the fresh water ahead of the vessel reaches a little below the bottom of her. It then runs aft below the vessel, also resisting her by friction. If the speed of the vessel be not too small, the fresh water will not come to rest when buoying up aft of the stern, but, on account of its inertia, continue to swing above and below its level of equilibrium thus giving rise to a series of waves in the boundary (Fig. 7, Pl. VI). As energy is needed for the creation of these waves, they contribute to the resistance (by increasing the sternward pressure-resultant).

Now suppose that the vessel does not reach, although nearly, the salt water, and that the vessel begins to move slowly. Then, the fresh water passes freely by the vessel, and the latter cannot be resisted by any *statical* pressure as in the former case. But the passage for the fresh water below

the vessel being narrow, its velocity there becomes increased, and consequently the *friction* against the bottom of the vessel becomes increased, by the effect of the surface-layer. Further, because the fresh-water is hinderd in its aftward motion, the surface rises a little — and the boundary is lowered — ahead of the vessel. Thus the vessel becomes influenced by a *sternward-pressure,* which may be calculated approximately by formula (7). When the vessel is moving at some greater speed, and the inertia of the water consequently becomes of greater importance, the difference of density of the two water-layers is insufficient to hold their common boundary nearly plane. The fresh-water displaced by the vessel, presses down the salt-water below, and the resulting disturbance of boundary level, persists as waves giving rise to *wave-making resistance.* Finally, when tne vessel moves at velocities higher than the maximum velocity of the boundary-waves, she cannot be followed by any train of boundary-waves, and the corresponding part of the resistance accordingly disappears. The vessel when moving at such high velocities, may leave only a very small disturbance, in the boundary behind. For the water-particles, on account of their inertia, then move in nearly the same paths as if the water were homogenous; the gravity has not time to appreciably alter the motion of a particle, before it has already passed by and is at rest behind, in the same level as it had ahead of the vessel. As the water-particles move past the vessel in nearly the same paths as if the water were homogenous, the increase of frictional resistance will, of course, also vanish at these high velocities.

If the vessel does not move in a narrow channel but in open water, the influence of the surface-layer may be discussed in a similar manner. Whether the vessel reaches the salt-water or not, the fresh water has free passage past her. If the vessel is moving slowly, the boundary between the two water-layers will therefore remain nearly horizontal, and the frictional resistance will be increased just as if the vessel moved in shallow homogenous water of the same depth as the actual surface-layer. Really, it may be increased even more than in shallow water, for the depth of the latter, down to the bottom, must be greater than the draught of the vessel, which on the other hand, can proceed in an even shallower water-layer resting on the top of heavier water. At greater velocities the inertia of the water becomes of more importance, and waves will consequently be created in the boundary between the fresh-water and the salt-

water, and will give rise to wave-making resistance. In the present case, the vessel may be followed, not only by transverse waves but also by diverging waves, and as these move with a smaller velocity than the vessel herself, the wave-making resistance will not completely disappear at the before mentioned critical velocity. But the influence of the stratification of the water, on the *frictional* resistance, may be expected to diminish and disappear at these high velocities, for the same reason as was mentioned in the former case (p. 47).

Figs. 2—5, Pl. V, illustrate the different forms of the wave-systems corresponding to different ratios between the vessel's velocity and the maximum wave-velocity[1]. The lines represent the wave-ridges seen from above, and the vessel must be imagined to be at the point at the left end of the figure. In Fig. 3 the vessel is moving at a considerably slower velocity than the maximum velocity of the waves (slower than at half this velocity). In Fig. 4 the velocity of the vessel is nearly as great as (1·2 times smaller than) the maximum wave-velocity; in this case the wave-length of the transverse and diverging waves, is greater, and the waves stretch farther out to the sides, than at a slower speed. In Fig. 5 the vessel moves at twice the maximum wave-velocity; in this case the transverse waves cannot exist and have entirely disappeared, but the diverging waves still exist and form a certain angle with the course of the vessel. Fig. 2 gives on a larger scale, and for several different velocities, the wave which is nearest to the vessel. In this figure, d denotes the ratio of the maximum wave-velocity to the velocity of the vessel, the latter velocity being the same in the case of all the curves. The dotted curve inside the curve for $d = 2$, corresponds to $d = \infty$. If the heights of the waves be known, the resistance caused by them may be calculated in a similar way as on pp. 36—37, except that the "ratio of transmission of the wave-energy" is not the same in both cases.

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[1] The curves are drawn according to calculations, quite similar to those found in LAMB's Hydrodynamics, p. 402, for ordinary ship-waves.

E. ON THE APPLICATION OF SMALL SCALE EXPERIMENTS.

It is obvious that experimental investigations of the phenomenon of dead-water cannot be made except on a considerably diminished scale. It is of importance, therefore to see under what circumstances we may draw trustworthy conclusions from such small-scale experiments.

The possibility of answering a question in hydrodynamics by means of experiments on a reduced scale, depends upon the possibility of so arranging the circumstances that the motion in the two cases — the real, full scale case, and the small scale experiment — become "geometrically similar"; that is to say, that all linear dimensions in the one case have a constant ratio to the corresponding lengths in the other case, that the velocities in corresponding points are in a constant ratio to one another, and similarly for the other quantities concerned. The conditions for such "geometrically similar motions" may — by a simple method given by HELMHOLTZ — be found directly from the general hydrodynamical equations[1].

Let the ratio of the linear dimensions		in the two cases, be	λ
—»—	velocities	—»—	v
—»—	densities of the fluids	—»—	δ
—»—	fluid pressures	—»—	π
—»—	extraneous forces, *e. g.* gravity	—»—	γ
—»—	coefficients of viscosity	—»—	$\varkappa$
Then the ratio of the time-intervals		—»—	is λ/v

The dynamical equations to be satisfied in water, which may be regarded as incompressible, are three, of the type

$$q\,\frac{du}{dt} = -\frac{\partial p}{\partial x} + q\,X + k\left(\frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} + \frac{\partial^2 u}{\partial z^2}\right), \quad . \quad . \quad (8)$$

where q is the density, k the (dynamic) coefficient of friction (not the kinematic coefficient, used by HELMHOLTZ), u, v, w the components of velocity

[1] H. v. HELMHOLTZ. Wissenschaftliche Abhandlungen, Vol. I, p. 158. "Ueber ein Theorem, geometrisch ähnliche Bewegungen flüssiger Körper betreffend, nebst Anwendung auf das Problem, Luftballons zu lenken".

along the axis of coordinates x, y, z, and X, Y, Z, the components along the same axis, of the extraneous forces. In addition, u, v, w, must satisfy the "equation of continuity"

$$\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} + \frac{\partial w}{\partial z} = 0 \quad . \; . \; . \; . \; . \; . \; . \; . \quad (9)$$

as well as the initial- and boundary-conditions, which may be of different forms.

If the linear dimensions be multiplied by λ, the velocities by v, and so on, the four terms in (8) are multiplied by

$$\frac{\delta v^2}{\lambda}, \; \frac{\pi}{\lambda}, \; \gamma\delta, \text{ and } \varkappa \frac{v}{\lambda^2}$$

respectively. To satisfy (8) for both cases, it is therefore necessary that these four quantities be equal, and the quantities λ, v, δ, π, γ, and $\varkappa$ must then satisfy the equations

$$\pi = v^2\delta, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (a)$$

$$\gamma\lambda = v^2, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (b)$$

$$\varkappa = \delta v\lambda, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (c)$$

Equation (9), does not involve any restriction in the choice of λ, v, δ, π, γ, $\varkappa$, but the initial- and boundary-conditions must accord with the choice of λ, v, and π. The boundary-conditions, are always fullfilled, if the boundaries consist of: (1) perfectly wetted, rigid bodies; either immovable and satisfying the ratio λ, or moving so as to satisfy the ratios λ and v; (2) free surfaces, influenced by extraneous pressures satisfying the ratio π. The initial conditions in the fluid are in accordance with any choice of λ, v, π, if the motion be steady, or if the water be initially at rest, in both cases.

If all the above conditions be satisfied, the motion will be geometrically similar in the two cases, and the ratio of the resultants of pressure upon corresponding surfaces is then

$$P = \lambda^2\pi = \lambda^2 v^2 \delta \quad . \; . \; . \; . \; . \quad (d)$$

In the case specially interesting us, the boundary-conditions are then practically satisfied if: (1) the linear dimensions of the two vessels are in the constant ratio λ; (2) the linear dimensions of the basins, in which they move, are in the same ratio λ; *or* the basins are so wide and deep that their boundaries have no influence upon the motion of the vessel; (3) the velocities of the two vessels are in the ratio v to oneanother. It is *not* necessary,

that the condition concerning the pressure on the free surfaces be satisfied. For, as the water is practically incompressible, the motion is, in both cases, unaltered, if the pressure in the water be diminished by a constant quantity (for instance, the atmospheric pressure), so that the pressure be *nil* in the free surface. It is then obviously necessary to leave the air-pressure *consistently* out of account. Equations (*b*) and (*c*) give

$$\lambda^3 = \frac{\varkappa^2}{\gamma\delta^2}.$$

As the possible variations of the density, of the gravity, and of the coefficient of friction, are very limited, the right hand side of this equation cannot be made many times greater or smaller than unity; and it is consequently impossible to reproduce exactly similar motions on very different scales, if gravity and viscosity have both to be taken into account. The gravity is of essential importance, in cases where there is wave-motion. *We must therefore at first neglect the viscosity, and afterwards consider its influence as best we can*; equation (*c*) is consequently left out of consideration for the present.

Gravity is furthermore practically constant, and so $\gamma = 1$. If the densities of the water-layers are also the same in the two cases, $\delta = 1$; and (*a*), (*b*), and (*d*) take the form

$$\pi = v^2;\ \lambda = v^2;\ P = \lambda^2 v^2 = \lambda^3.$$

That is: *If the linear dimensions be increased in the proportion λ, and the velocity of the vessel be increased in the proportion $\sqrt{\lambda}$, the motion of the water, on the larger scale and on the smaller scale, will be geometrically similar to oneanother, and the resultant of pressure against the vessel (the wave making resistance) will increase in the proportion λ^3.* This is the well-known rule which was used by FROUDE for his experiments with ship-models, and which still bears his name.

By an artifice, the equations (*a*), (*b*), (d) may be made to allow an important variation of the experiment, other than that which is indicated by FROUDE's rule. At the slow speeds which come under consideration, the waves in the surface are quite insignificant, and the surface-disturbances caused by boundary-waves, are also very small, because the difference of density between the different water-layers, is small. The motion in the water will therefore not be appreciably altered, if, by some means, *the water-surface be held rigidly*

plane; the pressure in the surface will, of course, no longer be uniform, but the differences of pressure in a horizontal plane will be almost exactly unaltered. We may therefore, with very small error, assume the water-surface to be held rigidly plane by suitable extraneous pressures. The gravity would then have no influence upon the motion, provided the density of the water were uniform; and in the case of a fresh-water layer of density q resting on the top of salt-water of density $q + \Delta q$, the motion will take place just as if the gravity did not act at all in the fresh-water, and in the salt-water only upon its excess of density Δq. The small difference between the *inertia* of fresh- and salt-water is of very little influence on the motion. *We may therefore assume the density to be the same ($= q$) in the whole fluid, but, assume instead, that the gravity varies, being nil in the fresh water and $\Delta q / q \times g$ in the salt water*[1]. In this way we are virtually able to change the gravity to a considerable degree, by just varying the specific gravity of the lower water-layer. We then obtain, by putting $\lambda = 1$ in equations (*b*) and (*d*) p. 50, the following rule, which — as far as the pressure exerted by simple harmonic boundary-waves is concerned — is a consequence of STOKES' theory of these waves, namely:

If the difference of density Δq of the two water-layers (Δq always supposed to be small) be increased in the proportion γ, and if the velocity of the vessel be increased in the proportion $\sqrt{\gamma}$, the geometrical similarity of the motion will be kept, and the resultant of pressure experienced by the vessel (the wave-making resistance) will increase in the proportion γ.

The two rules above were deduced on the assumption that the viscosity of the water could be neglected. We may therefore — in the same way as W. FROUDE[2] — determine the *frictional* resistance separately, and add it on to the pressure-resultant deduced according to the above rules. Rigorously the wave-making resistance, the eddy-making resistance and the frictional resistance cannot be quite independent of each other, and the method should consequently not be allowed; but the experiments seem to show — as might be expected *a priori* — that it is practically correct.

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[1] This artifice may often be of use in solving problems concerning the motion of the water-layers in the sea.

[2] See WHITE'S 'Manual', p. 479 seq.

It may be pointed out that the two rules above, hold approximately even if *the whole* resistance be considered. For the frictional resistance varies *approximately* as the wetted area of the vessel and as the square of the velocity, so that it is obvious it will vary approximately according to the two rules given. The eddy-making resistance is of smaller importance; the same rules hold true for it, provided the motions are really "similar", because it is a resultant of pressure. It could not, however, be taken into account when deducing the rules, because it should not exist at all in a frictionless fluid.

Besides the viscosity, we must also take account of the *capillary* forces, which influence the small scale experiments more strongly than those on a full scale, and thus disturb the similarity. On the one hand, these forces prevent the creation of ordinary surface waves in the experiments; on account of capillarity these waves cannot move with a velocity slower than 20 or 23 cm. per second, and the boat-models were towed at still smaller velocities. This effect of capillarity is, however, of no importance, because, at the slow speeds at which dead-water occurs, even full-sized ships do not produce any appreciable surface-waves. The secondary waves in the surface which are produced by the boundary-waves, are — on account of their greater length — not appreciably influenced by the capillarity.

On the other hand *the surface-tension* directly impels the vessel. If it were the same on all sides of the vessel it should have no effect; if, however, the water is even very little contaminated with grease, its surface-tension is reduced, but *in a smaller degree in the wake of the vessel,* where the contaminated surface film becomes ruptured by the eddies. A superficial resistance then arises, which although quite insignificant in the case of full-sized ships, might be of considerable importance in experiments on a small scale. It will be shown later, that even this effect of capillarity is of no essential influence on the experimental results.

III.

EXPERIMENTAL INVESTIGATION.

A. APPARATUS AND METHODS.

The first series of experiments was made with a model of the *Fram* on the scale of 1 : 200 in a glass tank 120 cm. long, 25 cm. deep, and 15 cm. broad. The results, however, seemed to show that on such a small scale the influence of viscosity was rather too great, and especially that the length of the tank was too small to allow the velocity of the model to become uniform. In the summer of 1900 a new tank, 350 cm. long, was therefore made, and in it, it was possible to experiment with a model of the *Fram* on the scale of 1 : 100. From this time all experiments were made in this large tank. As its construction required considerable forethought and care, and it has since proved very suitable, I think it is well to begin the account of the experimental arrangements with a description of this tank.

The *tank* (Figs. 1—3, Pl. I) is built up of five panes of plateglass (Fig. 1, *g*), forming its bottom and four sides, which are mounted in a wooden frame (Figs. 1 and 2). Its inside dimensions are 350 cm. in length and 40 cm. in breadth and height. Owing to the considerable length, care was required to prevent the long panes from breaking. To prevent warping, the beams of the frame were therefore made up of well-seasoned spruce-fir, sawed into boards and glued together in such a way that the structure of the whole was the most symmetrical possible. To reduce the give of the tank under the weight of the water, as much as possible, it was on Prof. V. Bjerknes' advice supported at eight points, the ends of four wooden springs, instead of at four points only. Two of these springs *ss'*, *ss'* are seen in Fig. 2; the position of

the 4 points of support s, s', s, s' are so chosen that the greatest deflection at any point of the beam should be the least possible. Two horizontal iron screw bolts (b, b, Figs. 1 and 2) join the bottom beams together and receive the pressure against the sides; similarly an iron cramp c holds the upper beams together. By these arrangements it was calculated, that the deflection of any part of the beams to which the panes are cemented, does not exceed 0·1 mm. The wooden springs (ss') also protect the tank from too great twisting forces; if the two tables which support the tank be not exactly in one plane, two of the springs would bend a little more than the two other one's and would sustain only a few kilogrammes more pressure.

The panes are so put in that, when broken, one may easily be taken away and replaced by a new, without disturbing the others. They were laid, one at a time, in spacious channels of tin-plate[1] (t, Figs. 1 and 3) filled with "marine glue" (m, Fig. 3). The glass having been left for some hours to sink down into the pitch, the wooden laths l were laid directly on the glass and screwed to the beams. The elastic "marine-glue" allows the panes to expand freely by heat. Other details and dimensions are shown by the figures.

The wooden frame was made by Mr. E. G. Ekstrand in Stockholm, and is of excellent workmanship. The tank has now been in use for 3½ years, and as far as can be seen, the frame has not warped the very least.

The towing-apparatus, attached to one end of the tank, is arranged for towing with constant force, not at constant speed. It is shown in Fig. 4, Pl. I. The towing-thread — a cocoon thread weighing about 3 mgr. per meter — is laid over the wheel w_1, which also serves to record the distance covered by the boat-model and therefore has a circumference of exactly 10 cm. Further, the thread is brought under the aluminium wheel w_2, on which towing-weights t are hung, and its end is fixed to the hook h. Thus, the boat-model is dragged twice the distance descended by the wheel w_2.

An additional "starting weight" s can be hung directly on the axis of w_2, to bring the boat-model more quickly to its uniform speed. This starting weight is lifted off on reaching the platform p, which is movable by soft friction along the rail r and can be placed at different heights according as a greater or lower initial speed is desired.

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[1] Copper channels, being much more durable, would have been far preferable.

The wheel w_2 alone, weighs 1·68 gr. To get towing forces less than half this (0·84 gr.), counter-weights c were used in the manner shown by the figure (actually, the cocoon thread of the counter-weights was always unhooked, when — as in the figure — the boat was being towed by *more* than the aluminium-wheel's own weight). The weights and counter-weights were made so as to produce towing forces of even decigrammes, attention being paid to the friction in the wheels.

The registration of the velocities of the boat-model was effected on a telegraphic tape, which received two sets of marks side by side — one set of "time-pricks" at equal intervals of time, and one set of "way-pricks" at equal intervals of distance covered by the boat-model. The apparatus employed for this purpose is represented by Fig. 5, Pl. I. The current from an electrical battery b of 4 volt passes through anyone of the two induction-coils i_1 and i_2, when the concerning circuit is closed. The circuit (1) is closed each 0.435 of a second, through the point of a pendulum p_1 touching a mercury meniscus m_1. The circuit (2) passes through the spindle of the registering-wheel (w_1, Fig. 4) and is closed when a steel point p_2 on the wheel, touches the mercury meniscus m_2. Except when the boat-model moved at very high speeds, two such points were used, placed on the wheel, diametrically opposite to oneanother. As the circumference of the wheel was 10 cm., the circuit was then closed once for each fifth cm. covered by the boat-model. The telegraphic tape t moves between the secondary poles s_1, s_2, s_3 of the induction-coils (the cylindrical pole s_3 is common to both the coils) and is pierced by the sparks. The spark-length is considerably diminished, and the effect of the sparks increased, by a condenser c of three metal discs, connected with the poles s_1, s_2, s_3. If the meniscus m is not too extensive, the interruptor of the induction-coil has time enough to break the circuit but not enough to close it again, before the point p has left the mercury. With this arrangement the current is never broken at the meniscus, which remains clean and in working order very long. By friction against a short piece of India Rubber tubing, the vibrations of the hammer-interruptor was each time stifled, before the primary circuit was closed the next time. The pieces of tape belonging to the separate experiments, were registered and were read off at convenience. The punctures were easily seen when the tape was held before an illuminated mirror. The "time-pricks" — or usually each fifth one — were read off on the scale formed by the "way-

pricks"; in this way the distances covered by the boat-model in equal intervals of time, were directly obtained.

For getting a sharply defined layer of fresh water above salt water, the following simple method was used (Fig. 6, Pl. I). A plane disc *d* of tin-plate, cleaned with acid, is suspended horizontally from one end of a lever *l*; the other end of the lever is properly loaded, so that the disc, when lowered on to the surface, adheres to the water which is raised a little above its free level. The fresh water, running from a siphon *s* upon the disc, flows softly out over the salt-water, and the disc is maintained on the upper surface by the lever. With two such discs, 25 cm. in diameter, a sharply defined fresh-water layer of 5 cm. thickness could be formed in less than one hour, even if the salt water had a specific gravity of 1·01 only.

The specific gravity of the water layers was measured with an ADERMAN hydrometer. These determinations were not made with as much care as they might have been; in particular I did not pay much attention to the changes of temperature of the surface-water. The differences of density of salt- and fresh water are for this reason, in most cases *smaller* than the values actually given (see section *D* of this chapter).

It was necessary *to have the surface of the water freed from dust.* If this was not done, the dust was gathered up in front of the stem of the boat and caused resistance. The dust was skimmed off by a strip of thick blotting-paper, supported by a metal plate and reaching over the whole breadth of the tank (Fig. 7, Pl. I). It was moved slowly from one end of the tank to the other, by means of a sort of sleigh, and as the paper protruded only one or two mm. into the water, it had no disturbing effect upon the surface of separation of the water-layers. After the dust was skimmed off and collected on the blotting-paper, this was washed under the water-tap and moved once more from one end of the tank to the other; there it was left standing, while the experiments were being made.

To make the different water-layers visible, two methods were employed. In most of the experiments, the salt-water (or the fresh-water) was blackened with liquid Chinese ink[1], before the water-layers were prepared. In salt-water of 4 % salinity or less, good Chinese ink keeps floating for several days; in

[1] Fuchsine would be very good if it did not diffuse so quickly from one water-layer into another. Chinese ink, of course, does not diffuse at all.

water of 10 % salinity it settles out in streaks and sinks a good deal in the course of two or three hours. About 20 cm.^{3} of it, make an area of one m.^{2} of water almost impenetrably dark. A white screen *s*, illuminated by a lamp *l*, was placed behind the tank (Fig. 9, Pl. I), and against this screen even very small waves could be observed at the boundary between the blackened and the clear water.

In this way, only a *silhouette* of the waves is obtained. To get a better idea of their true shape, they must be observed from above or from below by reflected light, so as to appear in *relief*. For this purpose, the salt water was carefully filtered free from all solid particles, and the fresh-water was made milk white. After several trials the most convenient substance for this purpose was found to be silver chloride as precipitated from a solution of 3 gr. silver nitrate in 20 liters of water[1]. When this fluid was spread out in a layer upon the salt water, the boundary, observed from below, appeared quite opaque and of a pure milk-white colour, and so it remained tolerably sharp for one or two hours. The greater the salinity of the water below, the longer the boundary remained sharp. Fig. 10, Pl. I, shows how the experiments were arranged; no screen was used, and the boundary was lighted directly by the lamp. As there was some difficulty in obtaining good "relief-photographs", and the necessary preparations required a good deal of time, as a rule only "silhouette-photographs" were taken.

In photographing the waves — in either of the two above ways — a flash-lamp (*f*, Fig. 10, Pl. I) burning a mixture of 3 parts magnesium powder, 2 parts potassium chlorate, and 1 part antimony sulphide, was employed.

When an experiment was to be begun, the boat was kept at its starting place by a hook *h* (Fig. 2, Pl. I), the towing weights and starting weight (if such was being used) were hung on, and the platform *p* Fig. 4 was placed in an appropriate position. The Grenet's elements were connected up, the pendulum and the telegraphic tape were put in motion, and when everything was in order, the boat was unhooked by the action of the electromagnet *e*

[1] Milk, or milk and water, does not float very long above salt water. The salt diffuses into the milk; and milk being heavier than fresh water, salt milk is heavier than salt water and consequently sinks. New quantities of milk come into contact with the water, become salt, and sink in their turn; and after a little while, the milk is seen to fall through the salt water as a shower of small vortex-rings. The same thing takes place in the case of all precipitates in fresh water, but at different velocities.

Fig. 2. When it arrived at the other end of the tank, the boat glided into a sort of slip to avoid thrusts and agitations in the water-layers.

The boat-models were made of solid wood and loaded to the weight stated for each of them. The screw-propeller and rudder-post (but not the rudder) were omitted, as well as most of the vessels body above the water-line. The small Fram-model (1 : 200) used in the small tank, was of painted fir; the small Fram-model (1 : 200) as well as all the other models used in the large tank, were of polished mahogany. The shape of each of them is described below in section *F* of this chapter. The towing-thread was fixed to the boat, to a sort of bow-sprit, so as always to be nearly horizontal. If the boat moved freely, it made greater and greater *sheers* and finally struck the side of the tank. To avoid this inconvenience, a little fork fixed to the stem of the boat was made to glide along a fine silvered copper-wire, stretched tight along the mid-line of the tank. The friction against the wire increased with the velocity of the boat and was between 5 and 20 cgr.

When the boat-model was in motion, it created small *oscillations in the water-level,* and these were measured by particular experiments. As level-gauge a disc, 5 cm. in diameter, was used. It was suspended from the shorter end of a light balance, so as to adhere to the water-surface, which it lifted a little above its free level; by means of levers, the vertical motion of the disc was communicated 50-fold to a pointer which allowed the variations of level to be recorded with an accuracy of 0·02 mm. The apparatus was fastened in a suitable place to the upper beam of the tank. The disc was very insensible to the horizontal motions in the water, provoked by the boat passing — probably because it was raised above the free level of the surface. Horizontally it never moved so much as half a mm., which would have caused an error of 0·001 mm., only, in the observation of the level. When all waves in the water had disappeared and the level-gauge was adjusted with its pointer at zero, the boat-model was unhooked. As the boat was moving along the tank, a free-hand curve on squared paper representing the subsequent indications of the level-gauge as ordinates with the corresponding positions of the boat as abscissae was drawn by me. By a second experiment the curve was completed and, if necessary, corrected. Provided the motion can be considered as stationary, these curves, drawn on a suitable scale, as well represent the profile of water-level following in an invariable position relative to the boat.

B. QUALITATIVE RESULTS.

The connection between resistance and velocity.

The influence of the fresh-water layer upon the speed of the vessel appeared distinctly even in the first preliminary experiments in the small tank. The results of some of these are represented graphically in Fig. 8, Pl. VI. The velocities of the boat-model in cm. per second, are plotted as abscissae and the corresponding resistances in grammes, as ordinates.

The curve (1) relates to the case in which the boat moves in homogenous water (salt or fresh) of the full depth of the tank. The resistance varies approximately as the square of the velocity. The dotted curves (2) and (3) relate to the cases of homogenous water of smaller depths, 5 and 2·5 cm. respectively. As might be expected, the resistance is greater in the cases in which the depth is small; but it seems to increase with the velocity according to an exactly similar law as when the water is deep[1].

When there is a layer of fresh water on the top of the salt-water, the result is quite different. Curve (4) is characteristic for such a case, the spec. gravity of the salt water being 1·030 and the depth of the fresh water 2 cm. In this case the resistance is, at velocities below 6 cm. per second, more than twice as great as in homogenous water of 2·5 cm. depth. At about this velocity, however, *the resistance becomes a maximum, and when the velocity is further increased, the resistance decreases* and becomes smaller than in homogenous shallow water. At great velocities the resistance seems to be approximately the same as in homogenous deep water.

There is, of course, no stable motion possible, corresponding to a point on that part of the curve (4) which slopes to the right (at velocities between 7 and 11 cm. per second); for if the velocity be increased a little, the resistance decreases, and the velocity continues to accelerate still faster. If the

[1] The actual velocities in these experiments were too small (below 16 cm. per second) for the peculiar phenomena discovered by SCOTT RUSSELL (see pp. 38—39) to take place. Indeed, the maximum resistance discovered by him, should appear at a velocity of 50 cm. per second, when the water is 2·5 cm. deep; at half this velocity, *i. e.* 25 cm. per second, the shallowness has no sensible influence upon the waves and wave-making resistance (see p. 44).

towing force is increased a little above the maximum resistance 0·29 grammes[1], the boat begins to be slowly accelerated; it is accelerated faster and faster as the velocity increases right up to 11 cm. per second, and continues to accelerate as long as the resistance is below 0·29 grammes, *i. e.* up to 15·5 cm. per second. Thus, if the towing-force be gradually increased, an almost sudden change of velocity from 6 to nearly 16 cm. per second, will at a certain moment occur. Similarly when the towing-force is again being gradually diminished, a sudden decrease of velocity will take place, from 11 or 12 to about 4 cm. per second. From this point of view the curve of resistance ought to consist of *two* curves connected by a horizontal straight line representing, in the case of increasing towing-forces, 0.29 grammes[1], and, in the case of decreasing towing-forces, 0·21 grammes. It may be here mentioned that the peculiar shape of the curve (4) thus affords the explanation of the *sudden* decrease or increase of velocity, which characterizes respectively the appearance and the disappearance of the dead-water phenomenon.

There is an apparent similarity between curve (4) and the curves in Fig. 4, p. 38, representing the law of resistance in shallow water, discovered by SCOTT RUSSELL.

A glance at curves (1) and (4) leaves the impression that the resistance in salt-water covered with a layer of fresh-water, is made up of *two* parts of different origins. One part is independent of the fresh surface-layer and is identical with the resistance in homogenous deep water. The other part, which must depend on the difference of density between the two water-layers and may be called "*dead-water resistance*", is practically restricted to a certain range of velocities — below 12 or 14 cm. per second, say. We shall see, that this latter part of the resistance mainly corresponds to the loss of energy due to generation of waves in the boundary between salt and fresh water. When calculated according to the formula (3) p. 43, the maximum velocity of the boundary-waves is, in the case considered, 7·3 cm. per second, which velocity is indicated by a ♀ at the top of the figure. The "dead-water resistance" is a maximum at about the same velocity (6 cm. per second), just as it should be in accordance with the discussion in Chap. II, p. 47. It will be

[1] Owing to the way in which the experiments were made, this value does not indicate the greatest resistance at constant speed. The maximum resistance in this simplest meaning is somewhat higher than the value 0·29 gr., indicated by the curve (4). See later.

shown later, that the agreement between theory and experiment is in this respect, really, very good.

The laws of resistance will be more fully investigated, from a quantitative point of view, in section F of this chapter.

The waves generated by the vessel.

The photographic illustrations will give a better idea of the waves in the boundary, than any description will do; they must, however, be accompanied by some critical remarks. The waves appeared quite distinctly even in the first experiments on the small scale. Figs. 1—6, Pl. XIV, show the aspect of these waves as they were photographed from the side. — The salt-water, as well as the boat, is seen quite black; the shallow fresh-water layer is brighter and almost exactly similar to the air, and between them the water-surface is seen as a dark line; the boat is moving to the left. Figs. 2 and 3 are characteristic of the shape of the waves when the boat is in "dead-water"; the salt-water is then pressed down by the fresh-water in front of and under the stem and is raised behind the stern — just as it should be according to the arguments brought forward in Chapter II (p. 47). These disturbances of level give rise to a train of waves following aft of the boat, which are even more distinctly seen in the photographs taken from the large tank (Figs. 2 and 3, Pl. XV—XVII.).

All these photographs only show *silhouettes* of the waves, and consequently cannot give a complete idea of their shape. To get a better representation of the waves, some photographs were taken obliquely from below, in the way described above (p. 58). One of these (Fig. 1, Pl. XIII) is projected in a frame representing the glass tank; and Figs. 2 and 3, Pl. XIII, may be imagined projected in the same frame. By help of Fig. 10, Pl. I, which shows how the photographs were taken, these latter can then be understood without any difficulty. The greatest part of the imaginary glass tank is filled with clear salt-water, and above this latter is a shallow layer of fresh-water (dark in the figure). The effects of the refraction of light in the water are not taken into account, as this would not make the figure clearer. As the light falls from behind, each wave-crest in the fresh-water layer is illumined on top and behind, while its fore-side, as well as each wave-hollow, is shaded. Bearing this in mind, the shape of the waves can

easily be imagined. Owing to the photographs being taken *obliquely* from below — as also on account of the refraction in the water — their breadth is seen 4·1 times reduced in proportion to their length. In order to see the length and breadth of the waves in their true proportions, the figure should consequently be seen sideways, so that its length becomes 4·1 times shorter; this is attained, when the quadrangle below Fig. 1 looks like a square. All details in the figure, except the waves, are, of course, then seen quite erroneously.

The boat, a model of the *Fram* (scale 1 : 200) is, in the figures Pl. XIII, entirely hidden in the opaque fresh-water, and we only see the diverging train of waves, the head of which indicates the position of the boat. The photograph is reproduced on such a scale, that the boat's length in the figure, is 36 mm., or just the same as in the figures Pl. XV—XVII. If this be noted, it can be understood, on Fig. 3, Pl. XIII, that the fresh-water is pressed down under the bow of the vessel, there forming a little hollow in the salt water, whilst at the stern the boundary is raised, so that the keel becomes visible in the clear salt-water. The disturbances thus described, spread as diverging and transverse waves very similar to the well-known wave-systems following a steam-launch in smooth water (see for instance Fig. 3, Pl. XII). The diverging waves are very clearly seen in the figures, Pl. XIII; the transverse waves, on the other hand, do not appear very distinctly. This is evidently due to the light having come *from the side* of the tank, so that the transverse waves have no shadows. I have observed steam-launches from a hill athwart, and their waves have then had a striking resemblance with those in the figures on Pl. XIII; unfortunately I have never had an opportunity of taking a photograph of such an instance. In Figs. 2 and 3, Pl. XIII, the section of the boundary becomes visible along the glass walls of the tank, as a sinuous curve evidently indicating the transverse waves, as well as an oblique section of the diverging waves (somewhat exaggerated through their reflection against the walls). The velocity of the boat is smallest in Fig. 1 and greatest in Fig. 3. The towing forces were in the three cases 0·25 gr., 0·50 gr., and 0·75 gr., respectively; and the maximum resistance, calculated according to the rules given below, should be 1 gr. One sees that the transverse waves increase very much in length and height as the velocity of the boat is increased.

While the "relief-photographs" (Pl. XIII) chiefly establish the *diverging* waves, it might be supposed that the waves shown by the "silhouette-photographs" (Pl. XIV—XVII) are only the *transverse* waves. Actually, however, diverging waves would look like transverse waves on these photographs; aft of the launch Fig. 4, Pl. XII, for instance, three sharply marked diverging waves are seen, and it is obvious that if the launch be regarded right from the side, they should look like the section of a train of transverse waves. A similar illusion will in a still greater degree take place in the glass-tank; for the diverging waves become reflected against the walls of the tank and, in consequence, have their height there increased. Further it is a consequence of the total reflection of light in the boundary between salt and fresh water, that every place where the dark-coloured salt-water only licks the wall of the tank, appears quite dark, because the light reaching the camera from these places must have passed merely through salt-water from one side of the tank to the other (supposing that the boundary between salt and fresh water is smooth, and feebly inclined). On the other hand, where the salt-water is raised in the middle of the tank, it will not appear quite dark, because the water is there stirred by the vessel so that the light might pass through it under diffuse refraction. The silhouette photographs must therefore chiefly show the wave-profile along the walls of the tank and consequently give the appearance of transverse waves. On the other hand, the transverse waves must have really been comparatively more developed in just those cases in which "silhouette-photographs" were taken, than when the relief-photographs were taken. For in the former cases either a smaller tank (Pl.XIV) or a larger boat-model (Pl. XV—XVII) was used than in the latter cases; the tank was consequently relatively narrower, and it is known that the waves become in that case more quickly transformed into transverse waves.

A photograph taken in June 1901 happened to show very clearly the true shape of the waves which are shown by the silhouette-photographs. The salt-water was rather strong (spec. gravity 1·16), but it was clear and not coloured with Chinese ink. This photograph is reproduced in Fig. 1, Pl. XII. To make it more realistic, it is completed by a sketch of the upper part of the *Fram* and her tackle. A relief-photograph of the small *Fram*-model in the small tank, is reproduced below (Fig. 2, Pl. XII), on such a scale that it corresponds in all its details to Fig. 1. Figs. 1 and 2 examined together, give

a very clear idea of the shape of the waves. From the boat's stern a couple of diverging, wing-shaped wave-hollows (elevations of the boundary) stretch out, and where these waves strike the walls of the tank, the first stern-wave is seen on the silhouette-photograph. After these waves, follows a couple of diverging wave-crests (depressions of the boundary) which likewise strike the wall. The waves following seem to approximate to the shape of transverse waves. In Fig. 1, Pl. XII, one may easily follow the profile of the waves along the wall of the tank, and then recognize the shape of the silhouette photographs (compare, for instance, Fig. 2, Pl. XVI). In addition, the salt-water in the middle of the channel is visible between the two first waves at the wall.

The most important point which the photographs described above clearly show, is that *the waves largely increase in height when the velocity of the boat increased towards the critical velocity, but when this is passed, and the boat is free from the dead-water, the waves disappear;* quite in accordance with the reasons given in Chap. II, and with the analogous phenomena discovered by SCOTT RUSSELL. The waves which are seen at a distance aft of the boat, in the bottom figure on Pl. XV—XVII, might possibly be explained as a section of diverging waves, but probably they are simply waves remaining from an earlier moment when the boat was still moving at below the critical velocity.

Figs. 7 and 8, Pl. XIV, illustrate the case in which the vessel moves much faster than at the maximum wave-velocity; in this case the difference of specific gravity between the water-layers has no appreciable influence upon the motion, which then takes place almost exactly as in homogenous water. To show the motion of the water at different levels, these experiments were made with three water-layers forming two boundaries 1 and 2 cm. below the free surface; in the case of Fig. 7 as in the experiment, the middle water-layer is black and the others are clear, and *vice versâ* in Fig. 8. The difference of spec. gravity between each two of the water-layers is about 0·0005 only, and the maximum wave-velocity is less than 1·4 cm. per second. The velocity of the boat-model, is about 5 cm. per second in Fig. 7 and about 7 cm. per second in Fig. 8. (In the latter case spirals of black water were seen in the middle layer, arising from vortices).

It was mentioned in Chap. II (p. 42) that in consequence of the wave-motion in the boundary, there must arise very low *waves in the surface* of

the water — a slight depression just over each elevation in the boundary, and *vice versâ*. This was verified by a few experiments described above (p. 59). Fig. 3, Pl. IX, shows some of the wave-profiles in the water-surface, drawn in these experiments. The numbers 1—9 to the left of the figure, denote 9 separate experiments. The horizontal line which is on a level with each of the numbers, indicates the undisturbed water-surface in the corresponding experiment; the boat-model is drawn black, on a scale of 1 : 20, in its due position relative to this line. The disturbance of surface-level in the neighbourhood of the boat is represented by the curved line; the vertical distances between it and the line of undisturbed surface-level, are 25 times as great as the disturbances of level observed in the experiments; the horizontal scale of the wave-profile is the same as that of the boat itself, and the scale of heights is consequently 500 times the scale of length.

A comparison of these profiles, with photographs of the boundary-waves in similar cases, shows quite clearly the above mentioned connection between the wave-motions in the surface and in the boundary. Compare, for instance, the profile 6, Pl. IX, and the photograph Fig. 2, Pl. XVI — both taken in the case of a 5 cm. deep surface-layer, in which the boat moves at somewhat below the critical velocity. Similarly, compare the profile 7, Pl. IX, and Fig. 4, Pl. XVI (the water-layers the same as before, but the velocity of the boat *above* the critical velocity). In both cases the shape of the surface is — when leaving out of account the scale of heights — a reversed image of the shape of the boundary. By calculation it can be shown that the ratio of the height of the surface-disturbances to that of the boundary-waves, is of the same order of magnitude as it should be according to the theoretical formula (1) p. 42. A more rigorous testing of the agreement between theory and experiment in this respect, is impossible, because the latter were not made with towing-forces corresponding to oneanother.

The disturbance of the surface-level gives a very simple explanation of the increase of resistance which is caused by the boundary-waves. Figs. 1 and 2, Pl. IX, are intended to illustrate this; they show the same surface-disturbances as the profiles 6 and 7, Fig. 3, but on less exaggerated vertical scale (50 times the horizontal scale). The boat is sketched in her true position relative to the actual (disturbed) water-surface, and the illustrations then become more realistic. In both cases illustrated, the towing force is 3 grammes,

but in Fig. 1 the boat is drawn by these 3 gr. at a velocity of 9·5 cm. per second only, whereas in Fig. 2 the boat has from the beginning a higher velocity, and is steadily towed at a speed of 27 cm. per second. *At the lower velocity the boat pushes a mass of water before her stem, and at the stern she provokes a wave-hollow; her resistance is in consequence, increased just as if she constantly had to rise on an incline. She is then "in dead-water". At the higher velocity, on the other hand, the boat moves on the top of the low hillock of water, which she provokes, and she consequently moves on a nearly horizontal surface.* The analogy with the phenomena in shallow canals, described by SCOTT RUSSELL (see pp. 38—39) is apparent (compare Figs. 1 and 2, Pl. IX, with Figs. 6 and 7, p. 39). But there is the difference that the waves illustrated in Pl. IX are not free waves in the water-surface, but an effect of the boundary-waves below, arising at a much slower velocity than the wave-phenomena in shallow water illustrated on p. 39.

The energy which is represented by the disturbance of level, is, at the lower velocity, constantly left behind as wave-motion; and the consequent loss is refunded by the work done by the boat in pressing into the wall of water before her. At the higher velocity, there is very little energy left behind, and consequently very little work necessary for refunding the loss.

A disturbance of the water-surface similar to that in the case of "dead-water", takes place also in homogenous water (see curves 1 and 2, Fig. 3, Pl. IX), but at much higher velocities than in dead-water. It will be shown in section *F*, that the pressure-resultant influencing the boat-model in consequence of this disturbance of water-level, makes an important part of the resistance, in homogenous water as well as in "dead-water". In the latter case by far the largest part of this pressure-resultant is due to the boundary-waves.

The oscillations in the velocity of the boat; conclusions regarding the wave-making resistance.

Before closing the description of the qualitative results, one very interesting effect of the dead-water must be mentioned, although it probably does not occur so markedly on the sea, as it did in the experimental tank. *In spite of the boat-model being drawn through the water by a constant weight, I never succeeded in giving to it a steady speed, when a layer of fresh water covered the salt water in the tank.* It was on the contrary, quickly

accelerated up to a higher velocity than its mean velocity, it then slowed down, took a new leap, and so on; in this way the velocity performed oscillations, which where sometimes very considerable.

The diagrams Pl. VII—VIII, showing the changes in the velocity with the time, during some selected experiments, may illustrate this. The number on the left of each diagram, refers to the number of the experiment, found in the second column of the tables (in the next section of this chapter). The time is reckoned horizontally from left to right, the distance between each two adjacent vertical lines representing an interval of 1·09 seconds (in diagram No. 288, it represents 0·436 seconds). The first part of the curve is left out on every diagram, so that the epoch $t = 0$, at which the boat started, must be imagined to be somewhere to the left of the figure. The velocity of the boat is reckoned vertically, and the scale, in cm. per second is given to the left of each figure; in all diagrams except No. 288, two intervals are equal to 1 cm. per second. The velocity is, at equal intervals of time, marked by small circles, and through them the curves are drawn.

The diagrams 18—47 refer to experiments in homogenous water. They show a velocity, gradually increasing, if the initial velocity given to the boat was too slow, or decreasing if it was too fast. Only in the experiments in which no steering string was used (30—35, 41—47) did it sometimes happen, that the velocity increased and again slackened, on account of the boat making sheerings from its straight course.

When there was a layer of fresh-water on the top of the salt-water, the motion of the boat took place quite differently. The changes of velocity in such a case, are shown typically by the diagrams 77—83 (depth of the fresh-water layer 5 cm.; spec. gravity of the salt-water 1·032). Experiment 77 was made with a towing-force of 0·8 gr. (about one third of the maximum resistance); in the course of about 40 seconds, the velocity goes through 3 complete and quite distinct oscillations, but the amplitude of the variations is not very great (0·4 cm. per second). The oscillations become more and more marked, as the towing force is increased, and simultaneously, they take a longer time. In experiment 80 (towing force 2·0 gr.) the velocity dropped from a maximum of 9 cm. per second to a minimum of 5 cm. per second, and one oscillation seems to have required about 25 seconds. When the towing-force was 2·2 gr. (diagram 82), which was very near to the maximum resi-

stance, there was not time enough, while the boat was going the whole length of the tank, for the velocity to perform a complete oscillation; after having attained a maximum of 10 cm. per second, the velocity steadily slackened and dropped down to 3 cm. per second. In experiment 83, the towing force was only 0·1 gr. more, and the velocity increased at first in very nearly the same manner as in the former case. But now the towing-force is sufficient to overcome even the maximum resistance, which corresponds to the top-point on the resistance-curve (4) in Fig. 8, Pl. VI, and in the present case takes place at a velocity of about 10·5 cm. per second; and the velocity, after having passed this value, is more and more rapidly accelerated. This is easily seen by the curve bending upwards when it has passed the horizontal line 10·5.

The changes or oscillations in velocity, were followed by peculiar changes in the wave-motion, which clearly showed the cause of the former. When the boat was unhooked and began to move, the boundary between the salt- and the fresh-water was at first quite even. Gradually, quite small waves arose, as in Fig. 1, Pl. XIV, and the boat continued to be accelerated. After a few seconds, the waves were greatly increased, the boat ceased to be accelerated, and a "solitary wave"[1] became visible under its stem (Fig. 2, Pl. XIV). If the towing-force was smaller than the maximum-resistance, the boat was then rapidly *retarded* and the solitary wave, moving steadily at the highest speed which the boat had, went on in advance. When it had reached a position ahead of the boat (about as in Fig. 3, Pl. XIV), the velocity of the latter was nearly a minimum; the solitary wave steadily distanced it, and the waves abaft came nearer, broke and disappeared. Then, the boat took a new leap, the waves began to increase, and the whole phenomenon was repeated. If the towing-force was greater than the maximum-resistance, the boat overtook the solitary wave, so that it gradually dropped astern from the position in Fig. 2, Pl. XIV, to that in Fig. 4, Pl. XIV. After that, the boat had her velocity rapidly accelerated, and simultaneously the height of the solitary wave under her keel, was more and more diminished (Figs. 4, 5, 6, Pl. XIV). She was then rid of the "dead-water".

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[1] A single permanent wave-crest moving along in the surface-layer (see Fig. 5, Pl. VI) and quite similar to SCOTT RUSSELL's solitary wave in shallow water, mentioned on p. 39. Just as this latter, the solitary boundary-wave seems to move with about the same velocity as long periodic waves.

The photographs Pl. XIV, which were taken in the small tank, do not represent very well, the waves behind the vessel, which are on the other hand, better seen in Pl. XV—XVII. Fig. 3, Pl. XVI, shows the moment when the waves begin to break; the velocity is just a minimum, and has dropped from its maximum 9·4 cm. per second to 4·6 cm. per second (see diagram 81, Pl. VII). A few seconds after the photograph was taken, the boat had its speed accelerated again; and if the length of the tank had allowed it, the same phenomena should have taken place once more.

The whole process took place in just the same way, whether the boat reached down into the salt-water or not. Even if the fresh-water reached so much as 2 cm. below the keel of the boat-model, the oscillations in the velocity were rather great (see diagrams 103—106, Pl. VIII). Figs. 1—3, Pl. XVII, show that the waves were, at the same time, very considerable. (The three last-mentioned figures show the situation of the waves just after the velocity had been a minimum; before the minimum was attained, the elevation of the boundary, which is now seen under the keel of the boat, had a position more astern).

If different velocity-diagrams be compared, it will be seen that the oscillations take place more slowly, the smaller is the difference of spec. gravity between the two water-layers. Compare, for instance, the diagrams 134 and 141 (difference of spec. gravity 0·01 and 0·02, respectively), 77—81 (difference of spec. gravity 0·032), 148—150 (difference of spec. gravity 0·07), and 160—162 (difference of spec. gravity 0·16). Indeed, according to the rule, p. 52, the period of the oscillations should alter inversely as the square root of the difference of spec. gravity, if, at the same time, the towing force were altered in the same ratio as this latter. This rule is approximately verified by experiments 141, 79, 149, and 161, in which the towing-forces stand to each other approximately as the differences of spec. gravity.

The *amplitude* of the oscillations, relative to the mean velocity, seems on the whole to be just as great when the difference of spec. gravity is 0·032, as when it is 0·16 (see diagrams 77—81 and 160—162). This fact will be made use of, later.

Some important conclusions regarding *wave-making resistance* may be drawn from the above-described phenomena. *They show that the corresponding changes of the waves and the resistance, do not take place simul-*

taneously with the changes of the vessel's velocity, but the waves and the resistance increase gradually towards their final values, after the boat has acquired a given speed; and this circumstance was evidently the reason of the oscillations. It will be shown that the diverging waves are less apt to cause velocity-oscillations, but the transverse waves more so especially if the boat is moving at a speed near to the critical speed at which the resistance is a maximum.

To make all this clear, first suppose that the boat at each moment creates all the wave-energy which we find within the space moved through during that moment. The wave-motion near the boat and the resistance to her, would then depend only on her *instantaneous* velocity, so that, directly after the boat has her velocity increased, waves of the height corresponding to the new velocity, are generated. In this case no oscillations in the velocity would be possible; the boat would, on the contrary, be accelerated by a force, equal to the difference between the towing-force and the resistance corresponding to her actual velocity, and she would gradually attain the velocity commensurate with the propelling force. She would never exceed this velocity, for as soon as it were attained, the resistance would be exactly equal to the propelling force, and her velocity would, in consequence, not be further accelerated.

Actually, the waves once created, propagate themselves to a certain extent in the direction of the vessel's motion, and thus the wave-energy is partly supplied from behind, the rest, only, being directly generated by the vessel. It is then clear that the waves will be smaller at the beginning, when there are no waves behind them; and if the boat be moved at a steady speed, some time must elapse, before the waves have obtained their final height. For the same reason, if the boat has her speed incessantly accelerated, the waves never reach the height corresponding to her instantanous velocity, but the changes of wave-motion must always follow on the changes of the vessel's velocity. Similarly, when the velocity is being retarded. With regard to the changes of the resistance, when the boat begins to move at a steady speed, three cases are conceivable: either it depends only on the speed of the vessel and not on the waves already created, or it is greater at the beginning when the vessel must create waves in water, before motionless, or it is smallest at the beginning and increases with the height of the waves. In either of the two first cases there would be no velocity-oscillations: for as soon as the boat ceases to be accelerated, the resistance then does not further increase, and therefore a higher speed than can be constantly sustained, cannot be given by a constant force. It is therefore proved by the velocity-oscillations, that *the resistance increases with the height of the waves*, when the boat begins to move at a stationary speed, and that in consequence *the changes of resistance follow on the changes of the vessel's velocity.*

The velocity-oscillations are now easy to understand. When the boat, initially at rest, is set in motion by a constant force, the resistance does not balance the moving force and stop the acceleration, before the boat has already a larger velocity than could be continually sustained by the moving force. The waves and resistance continually increase, and the velocity begins to be retarded; the resistance increases until it has reached a value corresponding to the actual velocity of the boat (but larger than the moving force). After that, the velocity and the resistance both diminish. As the changes of the latter are continually behind hand, the resistance continues to be diminished even after it is equal to the moving force, and the velocity has ceased to be retarded; the boat consequently begins to be accelerated again. The velocity-oscillations are then a necessary consequence of the law of wave-generation; only that they will be more or less stifled by viscosity. The matter will really be somewhat more complicated, because the waves during the oscillations alter their position relative to the vessel. When making the experiments, I had the im-

pression that this circumstance increased the velocity-oscillations, but a detailed discussion seems to give a different result; in any case it has not altered the oscillations essentially.

By simple considerations, the conditions under which the velocity-oscillations will be small or great may easily be found. For this purpose, we may regard a wave which follows immediately after the vessel, as made up of two waves, one of them being the wave which should at the actual moment be there only by the effect of the transmission of waves from behind; the other wave is then the vessel's direct contribution to the wave-motion. The sum of the heights of these two waves is equal to the height of the actual wave. Let us now make the very reasonable assumption, that the wave-height of the wave-contributions directly generated by the vessel, depends only on the instantaneous velocity of the vessel (and on her shape, etc.) but not upon the waves already created. [This is true, as long as the motions are small, so as to be conformable to *linear* differential equations. In general, the assumption may therefore be expected to be approximately true]. The wave-making resistance depends only upon the contribution which the boat must pay to the *wave-energy, i. e., to the square of the wave-height.* Now suppose the vessel to move at a steady speed and the instantaneous height of the transverse wave nearest behind the stern of the vessel, to be H. After having been propagated one wave's length, this wave will have a height $= H\sqrt{r}$, r being the ratio of transmission of wave-energy (see pp. 36 and 43)[1]. During the same time, the boat has generated a transverse wave of a certain height h, depending upon her instantaneous velocity v; and as the waves have invariably the same position relative to the boat, the wave nearest behind the stern will have a height equal to the sum of the above elementary waves, *i. e.* $h + H\sqrt{r}$. *The resistance due to transverse waves consequently varies as*

$$(h + H\sqrt{r})^2 - (H\sqrt{r})^2 = 2\,Hh\sqrt{r} + h^2 \quad . \quad . \quad . \quad (a)$$

As h and r depend only on the vessel's velocity, the resistance then increases with the actual wave-height H, when the boat is moving at a steady speed. When the motion has become stationary, the wave-height should also be invariable, and consequently $h + H\sqrt{r} = H$. If the value of h or of H, found from this equation, be put in the expression (a), the resistance will be as

$$H^2\,(1 - r) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (b)$$

or as

$$h^2 \frac{1 + \sqrt{r}}{1 - \sqrt{r}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (c)$$

Equation (b) shows that waves of a given height, cause less resistance, the greater be the ratio of transmission of wave-energy; equation (c) shows that when the wave-generating effect of the vessel is given, the wave-making resistance *increases* with the ratio of transmission of wave-energy.

The effect of the *diverging waves* upon the resistance, is quite analogous. Their velocity-component in the direction of the vessel is, however, only $v \sin^2\alpha$, α being the angle between the boat's keel-line and the crest-lines of the waves (see p. 37). That proportion of the whole wave-energy moving along with the vessel, is in consequense, $r \sin^2\alpha$, and this must be put instead of r, in the expressions (a), (b), and (c). The expression (a) then shows, that the resistance due to diverging waves, increases in only a comparatively small degree with the height of the actual waves, and more exclusively than the resistance due to transverse waves, depends upon h, *i. e.* upon the velocity of the vessel. The diverging waves therefore contribute less than the transverse waves, to the velocity-oscillations. The transverse waves, again, will give rise to the greatest oscillations, in the case when r is nearly 1, *i. e.* when the boat is moving at nearly the critical velocity.

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[1] This and the following conclusions are, obviously, but approximate. For the laws of propagation of simple harmonic waves, hold exactly true, only in the case of an endless series of waves, or, practically, for the middle waves of a large series.

The same result can be obtained in another way. If the boat, previously at rest, be moved uniformly at a speed v, the transverse wave first produced will have the height h; the next wave — that which is nearest to the vessel after she has proceeded 2 waves' length — will have got a contribution $h\sqrt{r}$ to its height from behind, and will therefore have a total height $h(1+\sqrt{r})$. Similarly, the third wave will have a height

$$h(1+\sqrt{r})\sqrt{r}+h=h(1+\sqrt{r}+\sqrt{r^2}),$$

and so on; and gradually, the height of the wave at the stern, approximates to its final value

$$h\sum_{n=0}^{\infty}\sqrt{r^n}=\frac{h}{1-\sqrt{r}}.$$

For diverging waves, it is only necessary to put $r\sin^2\alpha$ for r, just as above. The greater the fraction r (or $r\sin^2\alpha$), the more slowly does the series $\Sigma\sqrt{r^n}$ converge, that is: the more slowly will the wave-motion and the resistance reach their final magnitudes. Again, the more slowly the resistance reaches its final magnitude, the more will the changes of resistance follow after the changes of velocity, and the greater will be the oscillations, in the case of the boat being put in motion by a constant force.

This agrees well with the experimental result, that the velocity-oscillations, as well as their time-periods, increased rapidly with the towing-force, *i. e.* with the velocity [1]. For as the vessel's velocity was increased, the waves seemed to become to a greater and greater extent transverse waves and comparatively less, diverging waves; and at the same time the wave-length and consequently the quantity r, increased. In addition, the velocity-oscillations were more stifled by friction the slower the velocity; for, as will be shown in section F of this chapter, a relatively larger part of the resistance is due to friction, at slower velocities, than is the case in the neighbourhood of the critical velocity.

The velocity-oscillations of the smaller *Fram*-model in the large tank, were somewhat smaller than those of the large model (see diagrams 284—287, Pl. VIII). This partly depends upon the friction being in the former case comparatively greater; but it also depends upon the different wave-motion. For the smaller boat-model moved in a relatively wider channel than the large model, and the waves created by it, were therefore to a greater extent *diverging waves* than in the case of the large model. The same will be the case with vessels on the sea, as the water is there practically unlimited in horizontal direction; and this possibly explains why the velocity-oscillations, although very strong and evident in the experiments, are not observed by the sailors.

[1] In contradiction to this result, the diagrams 147 and 156 show slower oscillations than 148 and 160 respectively. I believe, however, that the variations of velocity in experiments 147 and 156 depended on wave-motions which remained in the water from the preceeding experiment and influenced the motion of the boat. I often required much patience in waiting for the wave-motions in the boundary to subside, otherwise they would disturb the motion. It has happened that by the effect of such residuary waves the boat-model has got free from the dead-water, although, with the same moving force it would have remained in dead-water if there had been no waves when it started.

The chief reason, for the latter fact, is, however, probably a much simpler one. For as a rule, a sailing-vessel will be put in motion more *gradually* than was the case with the boat-model in the experiments; and in the case of a screw-steamer the propelling-force diminishes when the velocity is increased, which circumstance prevents or reduces the velocity-oscillations.

To the above investigation, one more remark may be added, which will be of use later. The wave-making resistance depends, from one point of view, on the work spent by the vessel in generating waves; and in this way, formula (*a*) p. 72 is deduced, giving the wave-making resistance in the case of approximately stationary motion. On the other hand, the immediate cause of the resistance, is the water-pressure which is exercised on the vessel by the waves. The instantaneous resistance therefore depends only on the shape of the waves and their position relative to the vessel — whether they are generated by the vessel or not. [In this case we disregard the frictional resistance as well as the resistance to the vessel's *acceleration*]. A vessel might consequently experience resistance even when at rest, if she be only surrounded by waves. In this case, however, the waves will not have any permanent effect upon the vessel, for by passing the vessel, they will consequtively take every position relative to her, and their only effect upon the vessel will therefore be an oscillatory motion, insensible if the waves are short compared to the vessel and of small time period. The dependance of the resistance upon the waves only, seems to be in contradiction to formula (*a*) p. 72, as the latter also contains terms depending on the vessel's velocity. The latter formula, however, is based upon the supposition that the vessel's velocity is uniform, and that the waves follow her at the same velocity. In this case, the waves obviously take such a position relative to the vessel, that the work done in overcoming the sternward water-pressure, is equal to the work required for the generation of new waves.

The difficulties which the velocity-oscillations introduced into the determination of the connection between velocity and resistance, will be discussed explicitly in section *D* of this chapter. The most important one may, however, be shortly mentioned here. The *maximum resistance* in a given water-layer, represented by the top-point of the resistance-curve, was determined by a series of towing-experiments. A constant towing-force, each time greater than in the preceding experiment, was applied to the boat-model, which was

then unhooked and put in motion; *the maximum resistance was supposed to be equal to that towing-force which was just able to give to the boat the high velocities at which it was not influenced by dead-water.* One would, however, obtain higher values of the maximum resistance, if the boat were towed at different, *steady speeds,* and the corresponding resistance measured. Under the conditions of the experiments, the velocity of the boat-model was rather quickly accelerated, by the constant towing-force, up to the critical speed; and as the changes of resistance take place *after* the changes of velocity, the resistance was not fully developed even when the boat was already brought beyond that range of velocities, at which it is influenced by dead-water. It will be shown later, that the influence of this circumstance on the determination of the maximum resistance, is rather significant.

The maximum resistance may, in consequence, be defined in two ways — according to the results of experiments with constant towing-forces, or at steady speeds. Reasons will be given in section *D*, for the choice actually made.

Some other observations of various kinds, on the motion of the boat-model and the water around it, will be mentioned as they arise in the discussion.

C. NUMERICAL RESULTS.

The following tables form an extract from my experimental journal, which contained accounts of 744 experiments in all. In this extract only such experiments are included as are actually used in drawing the resistance-curves, or as are in other respects, of interest. The experiments not recorded here, were either less instructive or were not satisfactorily performed, and were therefore repeated.

The different columns give:

First and second columns, Date of the experiment and its number in the table.

Third and fourth columns, The specific gravity $S\frac{t}{15^{\circ}\cdot 0}$ of the salt-water (the bottom layer), and the depth in cm. of the fresh-water layer on top.

Fifth column, The additional "starting force" by which the constant towing-force was augmented at the beginning of some experiments, in order to give the vessel its constant velocity more quickly; it is half of the additional "starting weight" suspended.

Sixth column, The distance traversed by the boat-model before the additional starting-force was removed; it is twice the distance during which the starting-weight was allowed to act.

Seventh column, The constant towing-force on the vessel in grammes.

Eighth column, The force which is necessary to give to the vessel its actual acceleration at a given, definite instant. (For the calculation of this force, see the next section of this chapter). In this column, a value is given only in those cases in which the velocity gradually increases or decreases towards its final value. When there were oscillations in the velocity, the eighth column is blank.

Ninth and tenth columns, Corresponding values of resistance and velocity under uniform motion. *Ninth column* gives the towing force diminished by the quantity in the eighth column, *i. e.* the force necessary to draw the vessel *uniformly* with its actual velocity; this latter is given in cm. per second, in the *tenth column.* Both quantities then refer to a definite instant during the experiment. "Maximum resistance" written in the eleventh column, denotes that resistance and velocity are given for that moment, during the whole experiment, at which the acceleration of the boat-model was smallest. In several cases (when there were velocity-oscillations) the tenth column gives the *mean* value, about which the velocity seems to have oscillated; and the accelerations are disregarded. In these cases, the eighth column is blank, and the ninth column gives the same value as the seventh column. When the estimation of the mean velocity in the tenth column is uncertain, the value is enclosed in brackets. From some experiments it was quite impossible to determine a value of velocity and corresponding resistance. Experiment No. 55 is such a case, and here will be found in the tenth column, $< 8{\cdot}1$; this indicates that the velocity increased up to 8·1 cm. per second and then slackened again.

In some cases, the diagrams representing the velocity of the boat-model during the whole experiment, are inserted (in Pl. VII—VIII). These experiments are denoted in the tables, by an asterisk with the number in the second

column; and the corresponding velocity-diagram in Pl. VII or VIII, is denoted by the same number.

The photographs of some of the experiments, are reproduced (in Pl. XII-XVII). This is stated under "remarks" in the eleventh column of the tables.

Table I. Experiments in the small tank.

Model of the "Fram": Scale 1 : 200

Date	Number of experiment	Water-layers		Starting force		Towing force	Accelerating force	Resistance under uniform motion		Remarks
		Spec. g. of salt water	Depth of fresh water	grammes	centimetres			Resistance	Velocity	
May 14 1900	1	(no salt water)	23 cm.	0·5	5	0·1	0·01	0·09	7·5	Experiments 1—17, made without steering-string.
	2			0·5	7	0·1	0·00	0·10	8·2	
	3			0·5	15	0·2	0·02	0·18	12·4	
	4			0·5	20	0·2	0·00	0·20	13·1	
April 24 1900	5	(no salt water)	5·0 cm.			0·2	0·01	0·19	8·9	
	6					0·3	0·05	0·25	11·4	
	7					0·4	0·06	0·34	13·7	
April 24 1900	8	(no salt water)	2·5 cm.			0·2	0·02	0·18	7·6	
	9					0·3	0·00	0·30	10·2	
	10					0·4	0·00	0·40	12·2	
June 5 1900	11	1·030	2·0 cm.			0·1		0·10	3·0	
	12					0·15		0·15	3·7	
	13					0·2		0·20	4·1	
	14					0·25		0·25	4·9	
	15			0·5	1	0·3	0·02	0·28	7·3	Maximum resistance.
	16			0·5	5	0·3	0·07	0·23	9·6	
	17			1·5	5	0·4	0·15	0·25	14·3	

Table II. Experiments in the large tank.

Date	Number of experiment	Water-layers: Spec. g. of salt water	Water-layers: Depth of fresh water	Starting force: grammes	Starting force: centimetres	Towing force	Accelerating force	Resistance under uniform motion: Resistance	Resistance under uniform motion: Velocity	Remarks
Boat-model No. 1: the "Fram", scale 1 : 100										
Oct. 15	18*	(no	37 cm.	2·5	20	0·8	0·10	0·70	14·4	
1900	19*	salt		2·5	30	1·0	0·13	0·87	16·5	
	20*	water)		2·5	40	1·5	0·07	1·43	21·0	
	21			2·5	60	2·0	0·16	1·84	23·8	
Oct. 16	22			2·5	30	0·8	0·00	0·80	14·6	
	23			2·5	40	0·8	– 0·05	0·85	15·3	
Nov. 6	24*			2·5	12	0·25	0·00	0·25	7·4	
	25*			2·5	25	0·50	– 0·03	0·53	11·5	
	26*			2·5	30	0·75	0·00	0·75	14·5	
	27*			2·5	40	1·0	0·00	1·00	15·6	
	28*			2·5	55	1·5	– 0·03	1·53	20·1	
	29*			2·5	67	2·0	0·00	2·00	23·5	
Oct. 15	30*			2·5	20	0·8	0·11	0·69	14·3	No steering string used in
1900	31*			2·5	30	1·0	0·13	0·87	16·0	experiments 30–35.
	32*			2·5	40	1·5	0·16	1·34	20·0	
Oct. 16	33*			2·5	25	0·8	0·18	0·62	14·0	
	34*			2·5	35	0·8	0·05	0·75	15·5	
Oct. 12	35*					0·8				
Nov. 4	36*	1·024	0	2·5	6	0·15	0·00	0·15	5·5	The water was mixed with
1900	37*			2·5	3	0·1	0·00	0·10	4·2	Chinese ink from pre-
	38*			2·5	12	0·25	– 0·03	0·28	7·4	vious experiments, and
	39			2·5	25	0·5	0·00	0·50	11·5	its surface could not be
	40			2·5	35	0·75	0·00	0·75	14·1	kept quite as clean as usual.
	41*					0·25	0·07	0·18	7·0	No steering string used in
	42*			2·5	12	0·25	– 0·02	0·27	7·2	experiments 41–47.
	43*			2·5	25	0·5	– 0·05	0·55	11·3	
	44*			2·5	6	0·4	0·09	0·31	8·3	
	45*			2·5	6	0·15	0·00	0·15	5·4	
	46*			2·5	3	0·1	– 0·01	0·11	4·0	
	47*			2·5	3	0·1	0·01	0·09	4·0	

Date	Number of experiment	Water-layers		Starting force		Towing force	Accelerating force	Resistance under uniform motion		Remarks
		Spec. g. of salt water	Depth of fresh water	grammes	centimetres			Resistance	Velocity	
Febr. 20 1901	48	1·030	1·0 cm.			0·15		0·15	3·2	
	49					0·25		0·25	3·9	
	50					0·5		0·50	6·8	
	51					0·25		0·25	3·8	
	52					0·75	0·2	0·55	8	Maximum resistance.
	—					—	0·3	0·45	11	
Oct. 24 1900	53	1·032	3·0 cm.			1·0				The registration of velocity failed. Phot. Pl. XV, Fig. 1.
	54*					1·2		1·2	5·8	Phot. Pl. XV, Fig. 2.
	55*					1·6		1·6	< 8·1	Phot. Pl. XV, Fig. 3.
	56*					2·0		2·0	< 9·3	Phot. Pl. XV, Fig. 4.
Oct. 25	57*					1·0		1·0	5·1	
	57½			2·5	20	1·3	0·4	0·9	15·0	
Dec. 1 1900	58	1·030	3·0 cm.			0·15		0·15	2·9	Temp. of water 13° C.
	59			2·5	2	0·25		0·25	3·6	
	60			2·5	4	0·5		0·50	4·3	
	61*					1·0		1·0	(5·5)	
	62*			−0·1	50	1·0		1·0	5·6	
	63*			−0·25	50	1·0		1·0	(5·2)	
	64*			−0·3	50	1·4		1·4	(6·9)	
	65*					1·4		1·4	6·5	
	66					1·6		1·6	8·1	
	67*					1·7	0.1	1·6	10·5	Maximum resistance.
	68*			2·5	15	1·5	0·0	1·5	10·6	
	69*			2·5	25	1·2	− 0·05	1·25	10·0	
	70*			2·5	25	1·3	− 0·05	1·35	11·3	
	71			2·5	40	1·5	0·2	1·3	16·1	
Febr. 24 1901	72	1·030	4·0 cm.			1·5		1·5	6·5	
	73					2·0		2·0	(7·5)	
	74					2·25	0·1	2·15	10·5	Maximum resistance.
	75					2·15	0·1	2·05	11·0	Maximum resistance.
	76			2·5	40	1·5	0·0	1·5	13·3	

Date	Number of experiment	Water-layers		Starting force		Towing force	Accelerating force	Resistance under uniform motion		Remarks
		Spec. g. of salt water	Depth of fresh water	grammes	centimetres			Resistance	Velocity	
Oct. 23 1900	77*	1·032	5·0 cm.			0·8		0·8	5·8	Phot. Pl. XVI, Fig. 1.
	78*					1·2		1·2	6·0	
	79*					1·5		1·5	6·4	
	80*					2·0		2·0	6·9	Phot. Pl. XVI, Fig. 2.
	81*					2·1		2·1	(6·9)	Phot. Pl. XVI, Fig. 3.
	82*					2·2		2·2	< 9·9	
	83*					2·3	0·1	2·2	10·5	Maximum resistance.
	84*			2·5	25	1·5	0·0	1·5	12·1	Phot. Pl. XVI, Fig. 4.
Oct. 24	85			2·5	30	1·3	0·2	1·1	14·0	The same water used as on Oct. 23.
Dec. 2 1900	85½	1·030	5·0 cm.			0·25		0·25	4·3	
Dec. 3	86					0·15		0·15	2·4	The water was standing from Dec. 2. Temp. of water 11·5° C.
	87					0·25		0·25	3·9	
	88					0·5		0·5	4·9	
	89					0·7		0·7	5·5	
	90*			−0·1	75	0·9		0·9	5·5	
	91*			−0·2	75	1·2		1·2	6·2	
	92*			−0·2	75	1·5		1·5	(6·5)	
	93*			−0·4	75	1·5		1·5	6·6	
	94*			−0·4	75	2·0		2·0	(7·5)	
June 14 1901	95*					0·9		0·9	6·0	Fresh-water coloured with Chinese ink; salt-water clear.
	96					2·0		2·0	< 9·7	
	97					2·1	0·05	2·05	10·5	Maximum resistance.
Dec. 4 1900	98	1·030	7·0 cm.			0·15		0·15	4·1	Temp. of fresh-water 10° C.
	99*					0·25		0·25	5·0	
	100*					0·5		0·50	6·6	
	101					0·75		0·75	7·2	
	102					1·0		1·0	7·6	
Oct. 27	103*					0·8		0·8	7·0	Phot. Pl. XVII, Fig. 1.
	104					1·0		1·0	7·3	
	105*					1·2		1·2	7·7	Phot. Pl. XVII, Fig. 2.
	106*					1·5		1·5	8·0	Phot. Pl. XVII, Fig. 3.
	107					2·0	0·6	1·4	14·0	Maximum resistance.

Date	Number of experiment	Water-layers		Starting force		Towing force	Accelerating force	Resistance under uniform motion		Remarks
		Spec. g. of salt water	Depth of fresh water	grammes	centimetres			Resistance	Velocity	
	108			2·5	30	1·5	0·2	1·3	15	Phot. Pl. XVII, Fig. 4.
	109			2·5	30	1·25	0·0	1·25	14	
	110			2·5	50	1·0	0·0	1·0	15·5	
Dec. 4 1900	111	1·030	9·0 cm.			0·15		0·15	4·6	Temp. of fresh water 11° C.
	112					0·25		0·25	6·1	
	113			2·5	12	0·5		0·50	7·3	
	114			2·5	5	0·75		0·75	7·9	
	115					1·0		1·0	8·5	
	116					1·1		1·1	8·8	
	117					1·2		1·2	9·0	
	118					1·5	0·2	1·3	12	Maximum resistance.
	119					1·7	0·6	1·1	12·6	Maximum resistance.
Dec. 5	120			2·5	50	1·1	0·0	1·1	16·6	The same water used as on Dec. 4.
Dec. 28 1900	121	1·030	13·0 cm.	2·5	5	0·25		0·25	6·6	Temp. of fresh water 9·5° C.
	122			2·5	8	0·5		0·50	9·0	
	123			2·5	15	0·75		0·75	9·7	
	124					1·0		1·0	< 11·7	
	125			2·5	15	1·25	0·15	1·1	14·0	Maximum resistance.
June 9 1901	126	1·180	3·0 cm.			0·25		0·25	4·2	
	127					0·5		0·5	5·3	
	128					3·0		3·0	10·6	
	129					4·5		4·5	11	
	130					5·9		5·9	13	
	131					8·0		8·0	< 20	
	132					8·5	0·6	7·9	21	Maximum resistance.
Dec. 7 1900	133	1·010	5·0 cm.	2·5	1	0·15		0·15	2·7	
	134*			2·5	1	0·25		0·25	3·2	
	135			2·5	1	0·5		0·50	< 4·6	
	136					0·75	0·0	0·75	6·9	Maximum resistance.
	137			2·5	20	0·4	0·0	0·4	9·2	
Dec. 7 1900	138	1·020	5·0 cm.	2·5	1	0·15		0·15	2·7	Temp. of fresh water 11° C.
	139			2·5	1	0·25		0·25	3·7	
	140					0·75		0·75	5·0	
	141*					1·0		1·0	5·5	

Date	Number of experiment	Water-layers		Starting force		Towing force	Accelerating force	Resistance under uniform motion		Remarks
		Spec. g. of salt water	Depth of fresh water	grammes	centimetres			Resistance	Velocity	
	142					1·3		1·3		Still in dead-water.
	143					1·4	0·2	1·2	9·4	Maximum resistance.
	144			2·5	20	1·0	0·0	1·0	11·0	
	145			2·5	30	0·82	0·0	0·82	11·5	
June 12 1901	146	1·070	5·0 cm.			0·5		0·5	6·1	
	147*					1·0		1·0	7·5	
	148*					2·0		2·0	8·5	
	149*					3·0		3·0	9·8	
	150*					4·0		4·0	10·7	
	151					4·5	0·3	4·2	16	Maximum resistance.
	152			4·5	30	3·5	0·1	3·4	18·2	
	153			4·5	50	3·0	0·2	2·8	21·5	
June 10 1901	154	1·160	5·0 cm.			0·25		0·25	4·0	
	155					0·5		0·50	6·5	
	156*					0·75		0·75	7·8	
	157					1·0		1·0	9·0	
	158					2·0		2·0	11·5	
	159					3·0		3·0	12·0	
	160*					5·0		5·0	13·6	
	161*					7·0		7·0	14·7	
	162*					9·0		9·0	15·6	Phot. Pl. XII, Fig. 1.
	163					9·5		9·5	< 22	
	164					10·0	0·7	9·3	25·4	Maximum resistance.
March 7 1901	165	Water-layers represented by diagr. 2, Fig. 2, Pl. XI				0·15		0·15	2·3	
	166					0·25		0·25	3·6	
	167					0·5		0·50	4·8	
	168					1·0		1·0	5·6	
	169					1·5		1·5	6·5	
	170					1·75		1·75	7·0	
	171					1·9		1·9	< 9·9	
	172					2·0	0·2	1·8	11·0	Maximum resistance.
	173			2·5	35	1·25	0·0	1·25	12·4	
	174			2·5	35	1·4	0·3	1·1	16·0	

Date	Number of experiment	Water-layers: Spec. g. of salt water	Water-layers: Depth of fresh water	Starting force: grammes	Starting force: centimetres	Towing force	Accelerating force	Resistance under uniform motion: Resistance	Resistance under uniform motion: Velocity	Remarks
March 5 1901	175	Water-layers represented by diagr. 1, Fig. 2, Pl. XI				0·15		0·15	2·5	Temp. of fresh water 6° C.
	176					0·25		0·25	3·0	
	177*					0·5		0·50	3·9	
	178*					0·75		0·75	4·1	
	179*					1·25		1·25	5·7	
	180					1·5	0·1	1·40	8·8	Maximum resistance.
	181			2·5	30	0·75	– 0·3	1·05	10·1	
	182			2·5	30	0·95	0·0	0·95	12·0	
	183			2·5	30	1·05	0·03	1·02	12·6	
	184			2·5	30	1·15	0·05	1·10	12·5	
Boat-model No. 2: "Fram", Scale 1 : 100, trimmed.										
Febr. 24 1901	185	1·030	5·0 cm.			0·15		0·15	3·3	
	186					0·25		0·25	3·9	
	187					0·5		0·50	4·9	
	188					0·75		0·75	5·6	
	189					1·0		1·0	6·2	
	190					1·25		1·25	6·6	
	191					1·5		1·5	6·8	
	192					2·0		2·0	6·9	
	193					2·15	0·2	1·95	10·6	Maximum resistance.
	194			2·5	30	1·5	0·5	1·0	16·8	
Boat-model No 3: Flat-bottomed.										
Nov. 25 1900	195	No salt water	37 cm.	2·5	1	0·15	0·02	0·13	4·4	
	196			2·5	7	0·25	0·02	0·23	6·3	
	197			2·5	20	0·5	0·00	0·50	9·6	
	198			2·5	35	0·75	– 0·04	0·79	12·0	
	199			2·5	40	0·9	0·00	0·90	13·4	
	200			2·5	60	1·5	0·00	1·50	17·2	
	201			2·5	70	1·8	– 0·14	1·94	18·8	
	202			2·5	75	2·1	– 0·10	2·20	20·8	
	203			2·5	80	2·4	0·07	2·33	22·4	

Date	Number of experiment	Water-layers		Starting force		Towing force	Accelerating force	Resistance under uniform motion		Remarks
		Spec. g. of salt water	Depth of fresh water	grammes	centimetres			Resistance	Velocity	
Dec. 21 1900	204	1·030	3·0 cm.	2·5	2	0·15		0·15	2·7	
	205			2·5	2	0·25		0·25	3·4	
	206			2·5	1	0·5		0·50	4·4	
	207					1·0		1·0	5·4	
	208					1·25		1·25	< 7·2	
	209					1·5		1·5	< 8·2	
	210					1·7		1·7	< 8·7	
	211					1·8		1·8	< 9·3	
	212					1·9		1·9	< 9·8	
	213					2·0	0·1	1·9	10·6	Maximum resistance.
	214			2·5	40	1·5	0·0	1·5	12·2	
Dec. 21 1900	215	1·030	5·0 cm.	2·5	1	0·25		0·25	3·8	
	216			2·5	1	0·5		0·50	4·8	
	217			2·5	1	0·75		0·75	5·5	
	218					1·0		1·0	5·6	
	219					1·5		1·5	6·5	
	220					2·0		2·0	7·5	
	221					2·5		2·5	6·3	
	222					2·75	0·25	2·5	11·0	Maximum resistance.
	223			2·5	50	2·0	0·0	2·0	14·6	
Dec. 28 1900	224	1·030	9·0 cm.	2·5	1	0·25		0·25	5·1	
	225			2·5	5	0·5		0·50	6·8	
	226			2·5	7	0·75		0·75	7·5	
	227					1·0		1·0	7·9	
	228					1·25		1·25	8·1	
	229					1·5		1·5	8·3	
	230					1·75		1·75	8·7	
	231					2·0		2·0	< 11·7	
	232					2·25	0·15	2·10	12·2	Maximum resistance.
	233			2·5	35	1·6	0·0	1·6	13·4	
Boat-model No. 4, Sharp-bottomed.										
Dec. 17 1900	234	(no	37 cm.	2·5	10	0·25	0·00	0·25	6·7	
	235	salt		2·5	15	0·5	0·06	0·44	9·4	
	236	water)		2·5	20	0·5	0·00	0·50	10·0	

Date	Number of experiment	Water-layers: Spec. g. of salt water	Water-layers: Depth of fresh water	Starting force: grammes	Starting force: centimetres	Towing force	Accelerating force	Resistance under uniform motion: Resistance	Resistance under uniform motion: Velocity	Remarks
	237			2·5	40	1·0	0·08	0·92	14·9	
	238			2·5	60	1·5	0·00	1·50	18·4	
Dec. 20 1900	239	1·030	3·0 cm.	2·5	1	0·15		0·15	3·1	
	240			2·5	3	0·25		0·25	4·1	
	241			2·5	10	0·5		0·50	4·9	
	242					0·75		0·75	5·4	
	243					1·3		1·3	7·3	
	244					1·5		1·5	< 9·9	
	245					2·0	0·4	1·6	11·0	Maximum resistance. (Time-pricks failed).
	246			2·5	40	1·25	0·00	1·25	12·6	
Dec. 21 1900	247	1·030	5·0 cm.	2·5	1	0·25		0·25	4·6	
	248			2·5	1	0·5		0·50	5·4	
	249			2·5	1	0·75		0·75	5·8	
	250					1·0		1·00	6·0	
	251					1·25		1·25	6·5	
	252					1·5		1·50	6·7	
	253					2·0		2·00	< 10·1	
	254					2·25	0·2	2·05	11·5	Maximum resistance.
	255			2·5	50	1·5	0·0	1·50	13·5	
Dec. 28 1900	256	1·030	9·0 cm.	2·5	3	0·25	0·03	0·22	5·0	
	257			2·5	5	0·5		0·50	6·9	
	258			2·5	7	0·75		0·75	7·5	
	259					1·0		1·00	7·9	
	260					1·25		1·25	8·5	
	261					1·5		1·50	9·0	
	262					1·75		1·75	9·0	
	263					1·9		1·90	< 11·5	
	264					2·0	0·03	1·97	12·1	Maximum resistance.
	265			2·5	35	1·5	0·00	1·50	14·0	
Boat-model No. 5: Thin board.										
Dec. 20 1900	266	(no salt water)	37 cm.			0·15		0·15	6·2	
	267			2·5	5	0·25		0·25	8·8	
	268			2·5	10	0·5	0·03	0·47	14·0	

Date	Number of experiment	Water-layers		Starting force		Towing force	Accelerating force	Resistance under uniform motion		Remarks
		Spec. g. of salt water	Depth of fresh water	grammes	centimetres			Resistance	Velocity	
	269			2·5	15	0·75	0·03	0·72	16·6	
	270			2·5	18	1·0	0·10	0·90	21·9	
	271			2·5	25	1·5	0·20	1·30	27·0	
Dec. 21 1900	272	1·030	2·0 cm.			0·15		0·15	3·8	
	273					0·25		0·25	4·8	
	274					0·5	0·02	0·48	8·5	Maximum resistance.
Dec. 20 1900	275	1·030	3·0 cm.			0·15		0·15	4·7	
	276					0·25		0·25	5·8	
	277					0·5	0·03	0·47	8·5	Maximum resistance.
Boat-model No. 6: "Fram", Scale 1 : 200										
Nov. 6 1900	278	(no salt water)	37 cm.	2·5	4	0·15	0·005	0·145	11·7	No steering-string used in the experiments 278-280[1].
	279			2·5	8	0·25	0·005	0·245	16·2	
	280			2·5	12	0·4	0·02	0·38	20·7	
June 11 1901	281	1·136	2·5 cm.			0·25		0·25	8·0	Phot. Pl. XIII, Fig. 1.
	282					0·5		0·50	9·4	Phot. Pl. XIII, Fig. 2.
	283					0·75		0·75		Phot. Pl. XIII, Fig. 3 (Registr. of velocity failed).
June 9 1901	284*	1·180	2·5 cm.			0·25		0·25	8·6	No steering-string used in the experiments 284-291.
	285*					0·75		0·75	11·4	
	286*					1·0		1·00	12·2	
	287*					1·25		1·25	(13·8)	Time-record unsuccessful.
	288*					1·5	0·15	1·35	18	Maximum resistance.
	—					—	0·5	1·0	25	
June 9 1901	289*	1·180	3·0 cm.			0·25		0·25	9·2	
	290*					0·5		0·5	11·0	
	291*					1·0		1·0	13·3	

[1] This was originally not recorded in the journal; the results however show that the steering-string cannot have been used.

D. EXPERIMENTAL ERRORS AND CORRECTIONS.

On account of the small scale on which the experiments had to be carried out, even a small error in the determination of resistance might be of importance in applying the results to the case of full-sized ships. Most of these errors have, however, practically the same value (at a given velocity of the boat) independently of whether the boat moves with a small resistance in homogenous water, or with a great resistance in "dead-water". They have consequently no essential influence upon the determination of the "dead-water resistance" (see p. 61); and this is the part of the resistance which is of the greatest interest to us. An approximate determination of the resistance in homogenous water is sufficient for our purpose.

With regard to the measurements *in homogenous water*, as well as in fresh layers above salt water, the following points may be noticed.

1) The measured resistances are somewhat too high, owing to *the friction against the steering string* (p. 59). To approximately determine it, some experiments in homogenous water, were made without, as well as with, steering string (see experiments 18—47, p. 78). The friction, found from these experiments, is represented graphically by the faint curve in Fig. 1, Pl. X. As it was difficult to make the boat go straight, without using the steering string, the determination of the friction is only approximate. It was eventually found most convenient not to make any correction for it, and the values of resistance given in the tables and on the diagrams, therefore include friction. Exception must be made in the case of most experiments with the *small Fram*-models, in which the steering string was not used. When steering string was not used, it is always mentioned in the tables.

2) *The friction in the wheels* of the towing-apparatus, was as already mentioned, allowed for when adjusting the towing-weights. This friction varying from 16 mgr. in the case of the smallest towing-forces, to 82 mgr. with a towing force of 10 gr., was by control-determinations found to be practically the same at the end of the experiments in June 1901 as in November 1900. It is consequently without influence upon the measurements.

3) The loss of towing-force owing to *the small inclination of the towing-string* from the horizontal line, was, when calculated, found to be quite insignificant.

4) *The dust* which was constantly settling on the surface, was a source of resistance. It gathered up ahead of the boat and formed a rigid disc, which was pushed before just like an ice-floe would be. The resistance which it caused, seemed to be independent of the velocity of the boat, but it increased with the quantity of dust on the surface. The "dust-floe" grew larger and thicker, as the boat moved from one end of the tank to the other; and the resistance increased correspondingly. If there was very much dust, it would even stop the boat in spite of a towing force of half a gramme or more. To get rid of this error, which was noticed in the very first experiments, the dust was skimmed off in the manner described on p. 57. This operation was made generally twice a day, sometimes more often; and in this way, the errors due to dust, were kept within 2 or 3 cgr., in any measurement of resistance inserted in the tables. In most cases, it has probably not been more than a centigramme.

5) If the surface be even very little contaminated, so that its *surface-tension* be reduced, it will cause an increased resistance, as was mentioned on p. 53. In the eddying wake

aft of the boat, the surface-film might have a tendency to be ruptured, and the surface-tension takes its maximum value or in any case becomes increased[1], whereas no such effect takes place ahead of the boat. Not much attention was paid to this circumstance, before the completion of the experiments, and it would in any case have been difficult to avoid the error caused by it.

The greatest possible variation of the surface-tension being from 0·0755 to 0·0407 or 0·0348 gr. per cm.[2], and the *Fram*-model being 11 cm. broad, the resistance caused by surface-tension could theoretically amount to $0{\cdot}0348 \times 11 = 0{\cdot}38$ gr. The surface-tension has not, however, its minimum value in its equilibrium state (see F. NANSEN l. c. pp. 38 and 43) but takes a mean value. In our case, it might rather have been nearer to the maximum value, because the surface was repeatedly cleaned by expansion. The effect of the blotting paper in reducing the surface-tension (F. NANSEN l. c. p. 63 seq.) cannot have been very considerable, because the area of it in contact with the cleaned surface was almost nil. Further, as the breadth of the boat-model decreases very evenly towards its stern, the maximum surface-tension could hardly act in any case upon more than a small part of the whole breadth of the boat. For these reasons it is probable that the resistance due to the surface-tension effects, has not been more than 0·1 gr. for the highest velocities, and much smaller for the smaller velocities at which "dead-water" occured. In a few cases, exactly similar experiments were made before and immediately after the surface was cleaned; the results agreed with the above supposition, that the surface-tension effects was of very little importance. They might possibly have been somewhat greater in the case of dead-water than in homogenous water, because in the former case the motion of the water aft of the boat, is more turbulent.

6) *Owing to the boat moving in a rather narrow channel*, the frictional resistance and the pressure-resultant caused by it, may have been too high, for the same reason given at the top of p. 47. The experiments made, do not allow the value of this error being determined with any certainty; this would require a greater accuracy in the determination of the resistance especially in the case of the *small* boat-model, than was actually obtained. A comparison between the curves (1) in Figs. 8 and 9, Pl. VI, shows however, that the resistance to the small *Fram*-model was practically the same in the small and in the large tank. (The model used in the former case, had a rougher surface than that used in the latter case, see p. 59). This error cannot therefore have been very great, and has probably been quite inappreciable; its influence on the determination of the "dead-water resistance" has certainly been of no importance whatever.

On the other hand, the narrowness of the tank may have influenced the dead-water resistance, by altering the wave-motion. This circumstance will be examined below in section *F* of this chapter.

7) When the boat had not time to acquire a steady speed, the towing force must be diminished by the force accelerating her, in order to give the resistance to uniform motion (see columns 7, 8, 9, of the tables). This correction which in the case of the actual measurements, was always comparatively small, is equal to the product of the boat's acceleration and her inertia. The inertia which comes into account is, however, somewhat more than the mass of the boat itself; for the surrounding water must be accelerated as well as the boat. It was therefore determined experimentally in the following way. In experiments 46—47 the towing force was 0·1 gr., and the velocity 4 cm. per second. According to the diagrams on Pl. VII, the acceleration was practically nil, and 0·1 gr. is consequently the resistance at 4 cm. per second. In experiment 35 the towing force was 0·8 gr.; and the diagram on Pl. VII shows that the velocity increased from 2·5 to 5·5, that is by 3 cm. per second, in 4 intervals of time. The mean velocity during the interval being 4 cm. per second, the resistance to uniform motion was as before, 0·1 gr.; and the accelerating

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[1] Compare the treatise "On hydrometers and the surface-tension of liquids", by FRIDTJOF NANSEN. No. 10 of this report, p. 21.

[2] L. c. p. 27. Lord RAYLEIGH, Phil. Mag., 5 Ser., v. XXX, p. 386.

force was consequently $0{\cdot}8-0{\cdot}1 = 0{\cdot}7$ gr. As each interval of time is 1·09 seconds, the acceleration was $3/4{\cdot}36 = 0{\cdot}688$ cm./sec.^{2} $= 0{\cdot}00070\ g$. The virtual inertia of the boat is consequently $0{\cdot}7/0{\cdot}0007 = 1000$ gr., which is 125 per cent of the boat's own weight. Several such estimations gave between 110 and 130 per cent. I have used the mean, and assumed the virtual inertia of each boat-model to be 120 per cent of its own weight.[1]

In the case of the large *Fram*-model (weight 800 gr.) an acceleration of 1 cm./sec. in one interval of time then corresponds to a force $= \frac{1{\cdot}20 \times 800}{1{\cdot}09 \times 981} = 0{\cdot}90$ gr. In this way, the values in the 8th column of the tables, are calculated. From the corrected resistances given in the 9th column, the resistance-curves in the case of homogeneous water (Figs. 1—3, Pl. X), are calculated according to the formula

$$\text{resistance} = C_1 + C_2\,[\text{velocity}]^2;$$

the constant term being necessary owing to the friction against the steering string. This formula is in good agreement with all measurements at velocities below 15 cm. per second. For higher velocities the differences between calculated and observed resistance, are greater (up to 0·2 gr.), but this is of less importance because the range of velocities at which dead-water resistance was measured, lies below 15 cm. per second. The curves for the small *Fram*-models (Figs. 8 and 9, Pl. VI) are not calculated, but are drawn evenly through the separate points determined experimentally.

When the boat was moving in dead-water, its velocity periodically increased and diminished (see pp. 67 seq.). In this case the only possible way of applying the results, was to take the mean about which the velocity seemed to oscillate, and assume the resistance at this velocity to be equal to the towing force. These *mean* velocities are given in the 10th column of the tables, and are represented on the diagrams Pl. VII—VIII, by a short horizontal line on the left hand side of each diagram (see for instance diagrams 77—81, Pl. VII). When the towing-force was not near the maximum resistance, the velocity-oscillations were quite regular, and the mean velocity could then be determined very accurately. When, however, the towing-force was a little below the maximum resistance only an uncertain determination, or no determination at all, could be made of the velocity (see for instance, diagrams 81 and 82, Pl. VII. The upper left hand corner of the resistance-curves, could therefore be drawn only approximately. See further the explanation of the tables, p. 76.

To avoid the inconvient velocity-oscillations, I attempted to start the boat with a *smaller* force and then, when it had reached its greatest velocity, to increase the towing-force approximately so as to keep this velocity uniform.

[1] This is the same ratio which is given by *Froude* for full-sized ships. See p. 489 in *White* l. c. p. 33, where this quantity — 120 per cent of a ship's weight — is called her "virtual weight".

The necessary arrangements were made very simply, and worked satisfactorily; nevertheless, the method was not successful. The oscillations were somewhat diminished, but they were on the other hand less regular than before, and the estimation of the mean velocity was therefore more difficult and less reliable (see diagrams 62—64 and 90—94, Pl. VII—VIII). This method was therefore soon given up and a constant towing-force used during the whole experiment. Only when the towing-force was very small, was a small extra starting-force used.

Special attention was paid to the determination of the *maximum resistance.* As was pointed out on p. 75, this may be defined in two different ways: either as *the greatest resistance which the boat would experience at any steady speed (below the maximum wave-velocity);* or as *the greatest constant towing-force not able to bring the boat beyond that velocity, when starting from rest.* Theoretically, the former definition would offer the greatest interest; but it would be very difficult to measure this maximum resistance. A towing apparatus arranged for different uniform velocities and combined with a sensitive dynamometer, would be required for that purpose; and further a much longer tank, than that I had, would be a necessity, because the resistance in the neighbourhood of the maximum, increases very slowly up to its final value (see p. 73). The maximum resistance defined in the latter way, is comparatively easy to measure; and it is also of more practical interest than the former. The maximum resistance according to the former definition, is equal to the propelling force which a vessel must have, so as to be never troubled by dead-water; according to the latter definition, on the other hand, it gives the propelling force which a vessel must have to get rid of the dead-water (by simply stopping for a while and then making head-way again), if she be accidentally caught by it. In either case, the maximum resistance is physically a just as strictly defined quantity, and the rules in Chap. II, *E*, are obviously applicable to it. For all these reasons, I have desisted from determining the maximum resistance at steady speed and have kept to the latter of the above mentioned definitions.

The maximum resistance was therefore determined by towing the boat repeatedly with gradually increased towing-forces. Take as an example the experiments 77—84 (spec. gr. of salt-water 1·032; depth of fresh-water 5 cm.). With a towing force of 2·2 gr. (exp. 82) the boat was held in dead-water,

since the velocity increased to a maximum and then diminished again. With a towing force of 2·3 gr. (exp. 83), the velocity is accelerated more and more slowly up to 10·5 cm. per second, and then the acceleration begins to increase; which shows that the resistance was greatest at a speed of 10·5 cm. per second and diminished again with further increasing velocity. The boat had consequently got rid of the dead-water, and it is concluded that the maximum-resistance is between 2·2 and 2·3 gr. For a more exact approximation, the acceleration of the boat was taken into account in the following way. In experiment 83 the velocity increased, when the acceleration was a minimum, with about 0·12 cm./second per time-interval, which corresponds to an accelerating force of 0·1 gr. The greatest resistance during the whole of this experiment was consequently $2·3 - 0·1 = 2·2$ gr. But it should obviously have grown larger if the towing force had had any value between 2·2 and 2·3 gr.; and it should have attained its greatest possible value, if the minimum acceleration had been *nil, i. e.* if the towing-force had been equal to the "maximum-resistance". The "maximum-resistance" is therefore assumed to be *the mean of the towing-force (2·3 gr.), and the greatest resistance during the experiment (2.2 gr.),* in this case 2.25 gr. This method, of approximation, has been used in drawing all the resistance-curves; and it seems to be confirmed when applied to experiments with towing-forces exceeding the maximum-resistance by *different* quantities.

If, at the beginning of an experiment, there were even slight waves in the boundary, they might have disturbed the results by increasing or decreasing the resistance at the critical moment (compare foot-note, p. 73). Although it was necessary to wait rather a long time in between two experiments, for the wave-motion to subside, it is possible that small errors in the determination of the maximum resistance are introduced on this account.

From experiment 82 we may on taking into account the retardation of the boat, conclude, that the difference between the maximum resistance at steady speed and the maximum resistance according to our definition, may be rather considerable. When the velocity had dropped to 5 or 6 cm./second, it was retarded by 0·75 cm./second in one time-interval, which indicates an excess of resistance above the towing-force, of $0·75 \times 0·90 = 0·67$ gr. (see p. 89); the resistance was consequently $2·2 + 0·67 = 2·87$ gr. By subtracting the resistance in homogeneous water (0·15 gr. at 5·5 cm./second, see Fig. 1, Pl. X) we

get a dead-water resistance of **2·72** gr.; while experiment 83 gave a maximum resistance of 2·25 gr. at 10·6 cm./second and a dead-water resistance of 2·25 — 0·43 = **1·82** gr. only. It is not probable that the maximum dead-water resistance at steady speed should be *smaller* than the greatest dead-water resistance in an experiment such as 82; and it might consequently be expected to be 50 per cent greater than the maximum dead-water resistance according to our definition, or even more. In open water, where the waves spread more, to the sides, the difference may however, be smaller (compare p. 73).

The above-mentioned attempts to give to the boat a more uniform motion (pp. 89—90), were then, for one more reason unsuitable. For if the towing force be increased gradually during the course of the experiment, the maximum resistance obtained, will have another, not definable, value *between* the maximum resistances according to the two before mentioned definitions.

To determine a few points on the resistance-curve between the maximum-resistance and minimum-resistance points a starting force greater than the maximum resistance was used, and an attempt made to remove it when the resistance was diminishing and just equal to the constant towing force (see for example experiment 84). If the velocity was retarded, or continued to be accelerated after the starting weight was lifted off, a corresponding correction was applied to the resistance. It is apparent that these measurements could not be but rough approximations, for the motion will be influenced by the waves just before generated at a lower speed. They establish however, the continuity of the resistance-curve and give an idea of its shape. With regard to the part of the resistance-curve beyond the minimum-resistance point, the measurements are still more unreliable. I have for this part of the curve supposed that the resistance is very nearly the same as in homogeneous water.

At the time of the experiments, I did not pay much attention to the *temperature* of the water; and in the following applications I have for the sake of simplity assumed the specific gravity $S\,\frac{t}{15^\circ}$ of the fresh-water to be 1. As the temperature in most cases, was below 15°, the differences of specific gravity between fresh-water and salt-water, were as a rule, somewhat *smaller* than here given, sometimes by more than 0·0005.

E. EXPLANATION OF THE VARIOUS EFFECTS OF THE DEAD-WATER PHENOMENON.

All the accounts of dead-water, received, agree that the phenomenon is necessarily connected with the existence of a more or less fresh water-layer on the top of the heavier sea-water. It is proved by theoretical and experimental investigation above, that a vessel moving in such a place, creates waves in the boundary between the two water-layers, and that on this account, very marked effects on the speed of the vessel will occur; and it will be shown below, that from the existence of such waves all essential effects and peculiarities of the dead-water phenomenon can be very simply explained. In the next section of this chapter it will, in addition, be shown that the resistance and speed-reduction due to the wave-generation, is of just the proper order of magnitude to explain the effects of the dead-water; so that the correctness of the explanation may be regarded as completely substantiated.

If the surface-layer be running as a current on the top of the heavier water below with a velocity of its own, this circumstance will be a contributary cause to the loss of steering-power, which may however, often be entirely due to the boundary-waves mentioned above, and quite independent of such a surface-current.

Several very different theories — more or less impossible or insufficient — have been put forward with the view of explaining dead-water; and it would be to no purpose to criticise them all. One of them may however be mentioned, as it has recently been published in a well-known journal (Annalen der Hydrographie und Maritimen Meteorologie, 1904, Heft. I; "Totwasser", von Kapt. H. MEYER). Kapt. MEYER mentions several cases of dead-water which, as they refer to places from whence I had before only very scanty information, are of great interest here, and I shall therefore permit myself to make an abstract of them in a supplement at the end of this treatise. Finally, Kapt. MEYER presents a detailed explanation of the phenomenon in its various aspects. He explains not only the loss of steering but also the speed-reduction, as due to currents of different direction or velocity: since a ship is resisted by a very great force when moving in an athwart direction, and moves easily, only in the direction of her keel-line, he concludes that she must on the whole be difficult to move, if she stick in two currents of different directions so that motion athwart through at least one of the water-layers, cannot be avoided. "Wenn man bedenkt, dass ein schwimmendes Schiff vermöge seiner Bauart nur in seiner Kiel- oder Längsrichtung mit verhältnismässig geringer Kraft durch das Wasser bewegt werden oder solches durchschneiden kann, dagegen in seiner Dwars- oder Querrichtung nur mit grosser Kraft in minimale Bewegung zu bringen ist, so wird es begreiflich, wie schwer es halten muss, ein Schiff vorwärts zu bewegen, das in zwei wagerecht getrennte Wasserschichten taucht, sofern beide nicht die gleiche Richtungsbewegung haben, es mithin keine derselben in seiner Längsrichtung durchschneiden kann."

This conclusion would be correct if the resistance against a vessel were directed along the same line as her velocity relative to the water. But just because the water offers a greater resistance to the athwart motion than to the headway motion of the vessel, the resistance will have a more athwart direction, if the vessel be moving obliquely through the water; that component of the resistance which acts in the direction of the keel-line, and opposes the vessel's head-way motion, will obviously be independent (or nearly independent) of her athwart-motion or of a current impelling the vessel sideways. If Kapt. MEYER's deduction were correct, it would also appear, that a sailing-vessel should be unable to make head-way in a strong side-wind because the water makes a great resistance against her leeway-drift. Likewise, the "otter-board" used for keeping a fishing-line with hooks strained sideways from a moving boat, would not be able, in spite of the strain in the line, to run up almost in a line with the vessel, as it actually does.

One more mistake in the same paper, may be pointed out, although it is not of the same definite importance in estimating the value of the explanation given. Kapt. MEYER distinguishes distinctly between two cases; *viz.* when the two water-layers move with different velocities but *in the same direction,* and when they move *in different directions.* In the former case a vessel should not be essentially influenced by the currents when moving in their common direction of velocity, but in the latter case the vessel must always be impelled from the side, by *one* of the currents at least, in whatever direction she might move. The latter is, however, not true, since there is always one direction in which both currents have the same velocity component; and if heading perpendicularly to this direction, the vessel will apparently be carried sideways with a velocity equal to that velocity-component, and will cut the water at all levels in the direction of her keel-line. That a vessel is able to cut both currents in the direction of her keel-line, is also clear, for the simple reason that the case considered may be reduced to the case of a current moving on the top of still water, by simply superposing on the whole system a velocity equal and opposite to the velocity of the under-current.

The loss of speed; the sudden appearance and disappearance of the dead-water; its apparent capriciousness.

The generation of boundary-waves and the consequent loss of speed proved by the experiments, has been repeatedly pointed out above, and it is therefore not necessary to further enter into this subject very closely. It is common knowledge that a vessel when moving, under ordinary circumstances at a good speed, creates appreciable waves in the surface, which contribute to the resistance; and when she attains a certain higher speed depending on her dimensions, these waves and the resistance caused by them, increase so forcibly as to practically restrict the vessel's speed to within this limit. In shallow water there will be a corresponding limit, which as shown by SCOTT RUSSELL (see pp. 38 seq.), is comparatively low, and is determined by the depth of the water. If the vessel's velocity is raised above this limit (which is the maximum wave-velocity) the waves practically disappear and the resistance correspondingly diminishes. The case of dead-water is quite analogous. But the boundary-waves causing it, arise at a much slower velocity than do the

surface-waves, and also disappear at a much slower velocity than these latter; and this is the reason why the velocity is so considerably reduced by the dead-water, whenever it is upon the whole, influenced by it.

The physical reason why the waves in the boundary are created, and the way in which they are able to exert resistance upon the vessel, have been explained above, from different points of view (see particularly pp. 66—67). Here it may be sufficient to keep to the two facts that the vessel generates waves in the boundary, and that they (owing to the energy required for their maintenance) cause resistance. As far as other details are concerned, the explanation may be based upon the resistance-curves and other experimental results.

As was mentioned on pp. 60—61, curve (4) in Fig. 8, Pl. VI, shows that the change of the boat's velocity from *below* to *above* the maximum wave-velocity (and *vice versâ*) must always take place *suddenly*. And as the necessary condition for the generation of large boundary-waves, and *ipse facto* for all the effects of the dead-water, is, that the vessel move at below the maximum wave-velocity, the same resistance-curve involves the explanation of the fact that the dead-water always appears and disappears suddenly. This matter may be more explicitly stated; and for that purpose it is convenient to alter the units of velocity and resistance in Fig. 8, Pl. VI, to refer to a case more similar to that of full-sized ships. If we *e. g.*, imagine the velocities to be given in *decimetres* per second and the resistances in *tons*, instead of in cm. per second and grammes respectively, it follows from FROUDE's rule (pp. 51 and 53) that the curves in Fig. 8, Pl. VI, refer to a vessel of one hundred times the size of the model used in the experiments, *i. e. a vessel of the same shape as the "Fram" but of half her size.* The depths of the water-layers must at the same time be imagined to be as many *metres* as they were centimetres in the actual experiments.

Suppose, for example, this vessel to sail — in open sea-water of ample depth — before a fair breeze giving her a speed of 14 dm. per second (2·8 knots). According to curve (1), the propelling force should then be 0·24 ton. Now suppose that the vessel reaches a place where the sea-water (of spec. gravity 1·03) is covered with a 2 m. thick, layer of fresh-water. Curve (4) referring to this case, shows that the speed of the vessel will be hardly appre-

ciably altered (from 14 to 13.8 dm. per second)[1], and the change of water has no effect on the vessel. If now the wind gradually slackens, the velocity of the vessel diminishes slightly more than it would have done under ordinary circumstances (because the vessel must to a greater and greater extent generate diverging boundary-waves), and is 12 dm. per second, when the propelling force has fallen to 0·21 ton. *But if the wind slacken a very little more, the velocity of the vessel drops right down to 4·3 dm. per second (0.86 knot);* the reason is that with decreasing velocity the resistance due to boundary-waves begins to increase more than the frictional resistance decreases, and particularly, when it has fallen below 7·3 dm. per second, large *transverse* boundary-waves are created and contribute towards the resistance — *the vessel has taken dead-water. If the wind now recovers its initial strength, the only effect is that the vessel has her velocity increased a little (to 4·7 dm. per second), but she still lies in dead-water, and consumes her energy of propulsion upon large boundary-waves. Only if the wind freshens still more, so that the propelling force gets the better of the maximum resistance 0·29 ton, is her velocity at once increased from 6 to 15·6 dm. per second (full 3 knots); and the large boundary-waves simultaneously disappear — the vessel has got free from the dead-water.*

A vessel may evidently get into dead-water or get rid of it, even if the wind is not altering. The vessel may for instance move in a brackish surface-layer on the top of salt water, at a speed at which she is still insensible to dead-water. If the vessel enters a more and more fresh, surface-layer, or if the thickness of the latter alters suitably, its effect upon the resistance is increased (as will be shown in the next section of this chapter), and particularly, the *maximum resistance* represented by the top point of the resistance-curve and the *minimum resistance* at a somewhat higher velocity, are increased. When the minimum resistance (which in the above example was 0·21 ton) is large enough to get the better of the force of propulsion, the velocity is at

[1] For the sake of simplicity, it is supposed for the present, that the curves in Fig. 8, Pl. VI, are exactly correct, although their determination (as well as the applicability of FROUDE's rule) was to a great extent only approximate. Especially is it assumed, that the resistance is always the same at the same velocity, although it actually depends also on the manner in which the vessel is set in motion (see p. 75). The errors which these suppositions involve, are of no importance, as far as our present qualitative discussion is concerned.

once retarded, and the vessel is caught by dead-water. Independently of whether it is *the water-layers or the propelling-force which is changed, it will have no peculiar effect on the speed of the vessel, until the propelling-force is equal to the minimum or the maximum resistance; but then the vessel with a sudden change of velocity, falls into dead-water or gets rid of it, as the case may be.* This obviously explains the fact that as a rule, only sailing vessels and towed ships are liable to dead-water; the propelling force of a steamer being in most cases sufficient to get over the maximum resistance. It also explains the experience of sailors, that they (at a given place and at a given season etc.) are safe from dead-water when going at above a certain speed; because the vessel's speed is a measure of her force of propulsion.

Keeping to our first example (Fig. 8, Pl. VI, curve 4), suppose the vessel to move — under sail or steam — at a speed of 14 dm. per second. If the vessel accidentally — for example by currents or during a manoeuvre — has her speed reduced to somewhat less than 9 dm. per second, only for a few moments, the resistance becomes *increased,* the velocity is further decreased to 4·6 dm. per second, and the vessel is then in dead-water. This explains why vessels, according to the experience of several sailors, are most liable to dead-water when making turns in their course or as a consequence of bad steering. It also explains the capriciousness with which the dead-water sometimes seems to occur. Indeed, it is often a matter of mere chance whether waves, manoeuvres, changes in the wind, etc., conspire together unfavourably or favourably — in other words so as to bring a vessel into dead-water or out of it. Currents in the surface-layer depending on boundary-waves[1] may especially be one of the causes affecting the speed (see foot-note, p. 73). One vessel may therefore fall into dead-water, while another quite similar, or a worse vessel, goes past her a short distance away without experiencing any trouble at all.

The appearance of the sea-surface.

The various descriptions in chapter I, of the appearance of the sea-surface, do not agree with oneanother, in all their details. A close agreement was

[1] These may have been created by other vessels in dead-water, or by various other causes, as for example by the motion of one water-layer above another just as a wind affects the water-surface, or by accidental or periodic motions at sea reaching the mouth of a fjord covered with a layer of light surface-water.

indeed hardly to be expected, and all the more, because the observations were made casually, and, in most cases, without any intention of afterwards giving an accurate description of them. Nevertheless, as far as their main features are concerned, they may all be explained from the boundary-waves which have been studied above, theoretically and experimentally. Wind, currents, and other circumstances vary in many ways, and may, as will be explained below, also account for variations in the appearance of the phenomenon.

Dr. NANSEN observed, when he was steering in dead-water off Taimur, *very low waves following on the sea-surface, to the sides of the vessel and in her wake* (see Fig. 1, Pl. IV). These waves are evidently the low surface-waves, (see pp. 42 and 65—66), which are a secondary effect of the waves in the boundary between salt and fresh water. Their crest-lines should then follow the lines of the boundary-waves; and indeed, it is not difficult to recognize the shape of these on the sketch, Fig. 1, Pl. IV. (See further below). The height of the waves is also not in contradiction to this explanation. The photographs, Figs. 2—3, Pl. XV and XVI — illustrating cases, which must have been similar to that off Taimur — show boundary-waves of 2 or 3 millimetres' height; or if allowance be made for the exaggeration of the wave-height on account of the oblique reflection of the waves against the walls of the tank, 1·5 or 2 mm. at least. As the vessel on these photographs, is on the scale 1 : 1000, it is very probable, that the *Fram* may have been followed by boundary-waves of 1·5 or 2 metres' height from hollow to crest. The difference of specific gravity between the two water-layers was supposed to have been about 0·02, and the height of the surface-waves would then, according to formula (2) p. 42, be nearly 0·02 of the boundary-waves, that is 3 or 4 cm. I have observed that waves of such heights and of 10 metres' length or even more, may be perceived without any difficulty, in smooth water.

It is not very extraordinary, that the above mentioned long and low waves have not been observed on any other occasion. Indeed, they will probably escape one's attention if the sea is even moderately rippled by the wind; and in a very slight wind, the dead-water waves created will, as far as sailing-vessels are concerned, be too low for them to be observed in the water-surface.

Several narrators have seen two or more *stripes on the sea-surface, stretching obliquely abaft of the vessel or across her wake.* They are described in different ways — as streaks of hopping wavelets (accounts Nos. 3

and 5) or as "rips" or paths in the water, resembling the boundary lines between currents of different velocities or directions. To explain these observations, it is sufficient to remember that the boundary-waves already mentioned, essentially consist in veritable currents, running alternately here in the one and there in the other direction in the shallow surface-layer. Along the slopes of the waves, there will consequently be boundary-lines, where the currents encounter and part from each other: at the former places the wavelets in the surface become short and steep (hopping); on the other hand, wherever the surface-currents issue in opposite directions, the surface, owing to the rising water, is smooth and unrippled or else has a whirling appearance.

According to this explanation, *the shape of the stripes* should be the same as that of the crest-lines of the boundary-waves. To judge from the experiments, the boundary-waves are very high only in the case when the vessel generating them, has a speed somewhat below their maximum velocity (2/3 of it, or more); and in this case their crest-lines are more extended to the sides [as *e. g.* in Fig. 4, Pl. V, see p. 48], than are ordinary ship-waves. It might therefore be expected that the shape of the dead-water stripes should, whenever they have been very noticeable, have been something like this figure. For reasons which have been already mentioned, it cannot however be expected that the descriptions of the dead-water stripes should completely agree with one another, or that they should be in all their details reliable. If we therefore make allowance for some discrepancies, we recognize on the whole, a remarkable similarity between the sketches of the stripes (Pl. IV), and of the lines in Fig. 4, Pl. V. This is particularly true in the case of Figs. 1 and 6, Pl. IV and even of Fig. 5, Pl. IV; the stripes in Fig. 8, Pl. IV apparently follow the transverse waves, while in Figs. 9—15, Pl. IV they seem to indicate a couple of diverging waves[1].

It seems remarkable that in some cases the transverse waves (stripes) are seen, and in other cases the diverging one's. The reason probably is that the propelling force of the vessel — and consequently her velocity — has been comparatively greater in the former cases than in the latter. It is proved by the experiments, as well as by calculations of which a summary will be

[1] It may be added that most of the narrators had no information on the explanation here given; and in the case of all the sketches and descriptions it is impossible, that they could have been influenced by a knowledge of this explanation.

given in an appendix below, that the transverse waves, very small at lower velocities, increase forcibly by accumulation, when the vessel's velocity approaches the maximum wave-velocity. The reason of this is that much the greater part of their energy is then transmitted with the same speed as the waves and the vessel herself. The diverging waves will not be exaggerated to a similar degree, at velocities approaching the above-mentioned critical velocity, because even at this velocity, a fraction only of their energy, is transmitted in the direction of the vessel's motion (see p. 37). The transverse stripes, generated by the transverse waves, may therefore at these comparatively high velocities be more distinctly seen than the diverging stripes. At lower velocities the diverging waves are more comparable in height with the transverse waves, and it is obvious that in this case particularly the headmost couple of diverging stripes should appear more distinctly than the other stripes; because the water inside it, is owing to the boundary-waves, a mass of oscillating currents which prevent the wind-ripples forming, and the headmost stripes form the boundary between this unrippled, whirling area, and the area outside it, which is rippled by the breeze (compare the description in the last paragraph but one, on p. 20, as well as Fig. 1, Pl. V). Quite in accordance with this explanation, is the observation of Mr. Eriksen (Account No. 8) that a couple of diverging stripes are seen when the wind is very slight, but transverse stripes when the wind freshens a little. Some details in account No. 7 (at the bottom of p. 16) probably point to the same facts as these observations of Mr. Eriksen.

That the dead-water stripes really indicate waves in the boundary between salt-water and the lighter surface-water, is directly proved by an observation of Mr. G. A. Larsen (p. 14): when a vessel in dead-water has passed a moored vessel, the latter is pushed backwards and forwards, as she is reached by the stripes, one after the other. This shows that the water is really oscillating, and the wave-length would, in consequence of statements given on p. 14, be equal to twice[1] the distance between two stripes. It cannot be a case of ordinary surface-waves, because with this wave-length, they would have an

[1] If the wave-length were equal to *twice* the distance between two stripes, it might be expected that each second stripe would have a different appearance to the others. Mr. Larsen says nothing about this, and I am not able to decide in what respect the description might possibly be incorrect, or its explanation incomplete. It is not improbable that the apparent disagreement depends on the phenomenon being different

incomparably greater velocity than a vessel in dead-water; and the only possible explanation is therefore, that the oscillations depend upon boundary-waves.

There is another observation (see account No. 8), which is in remarkable accordance with the above explanation of the dead-water stripes: they draw nearer to the vessel or farther off, according as her speed is slowered or increased. The reason is that the wave-length must be increased at the same moment as the vessel's speed (compare pp. 35—36), *i. e.* the distances from the vessel to the wave-crests, grow longer. It is further mentioned, that the stripes remain on the water, when the vessel has got rid of the dead-water and is sailing from them; it is obvious that it should be so, if the stripes depend upon waves which continue to move at their own velocity even after the force generating them has ceased to operate.

Several observations, although described in different ways, obviously refer to the above mentioned dead-water stripes. For example: "a system of sharply defined, but small waves generated on each side of the vessel" (account No. 5; Fig. 11 Pl. IV); "a number of small whirl-pools (eddies) are seen in the boundary between the 'ice-floe' and the water outside it" (account No. 7); "the ship was followed by *two* sharply defined *wakes*, the one slightly curved to the port side and the other more strongly bent to starboard" (account No. 13). Perhaps the following descriptions also indicate dead-water stripes: "A line of distinction can often be observed between the dead-water round the ship and the 'living water' outside" (account No. 9); "the water round the ship was somewhat stripey" (account No. 11); "it was as if a floating spar were lying fair across the vessel's stem" (account No. 37); "I saw a long stripe stretching from the bow far over the water on each side, dividing the water into two parts etc." (account No. 38). In the last-mentioned case it is said that the vessel (30 m. long) required 5 or 6 minutes to pass by the stripe, which would hardly be possible if the stripe had been fixed in space; on the other hand it might have been a couple of dead-water stripes which gradually dropped astern, when the vessel got rid of the dead-water.

It often happens that *the dead-water stripes are asymmetrically arranged* or that one stripe only, is seen to the one or to the other side

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under different circumstances, such as different depth of the surface-layer, and so on. It would however, lead far beyond reasonable limits, if I were to try to thoroughly clear up this and several similar details.

of the vessel (see Figs. 12, 14, 9, 13, Pl. IV). Such irregularities are, however, very easily explained, and the explanation is, in most cases, already given in the accounts. The stripe to the windward side, might possibly be invisible owing to the effect of rather strong wind-ripples (compare account No. 8), although under different circumstances, if for instance the boundary-waves are larger, the ripples may only have the effect of making the stripes more apparent owing to the contrast between the smooth area inside the stripe and the ripples outside it. If the vessel is sailing in a shallow fresh-water surface-current and reaches right down into the still salt-water below, the surface-water will run past the vessel with a certain velocity. (That such shallow surface-currents exist in the Kattegat, was directly proved in the case reported by Count HAMILTON, account No. 14). The motion of the stripes (boundary-waves) relative to the vessel, is then a resultant of their own velocity through the surface-water, and the velocity of the latter past the vessel; and in consequence, the stripes will be bent in the direction of the surface-current. This was apparently the case in the accident described by Mr. SIDNER (account No. 13). It might perhaps be questioned whether this accident was not an effect of the surface-current only, but the two stripes ("wakes") issuing from the stern, and indicating the diverging waves, are so characteristic of dead-water that there is really little doubt about it. Neither should the current alone have had the effect of retarding the *speed* of the vessel. When there is a surface-layer of lighter water on the top of the heavier sea-water, the former is evidently very often acted upon by other forces than the latter; it will in consequence run as a distinct surface-current on the top of the sea-water, and it must therefore be expected that the dead-water phenomenon will often be modified by surface-currents. When a vessel is sailing with the wind from the side, her leeway-drift might sometimes have a similar effect, so as to give to the stripes a direction more to windward (compare account No. 6 and Fig. 12 Pl. IV).

The motion of the water around the vessel.

It is commonly observed that the water surrounding the stern of a vessel in dead-water seems to *follow* the vessel; and this is apparently confirmed by the fact, that if a small boat is in tow astern, it is sucked up to the vessel and cannot be kept clear. These observations may, however, be explained as merely an effect of the above-mentioned boundary-waves; and the

belief that the vessel drags along a bulk of surface-water, is certainly an easily explained illusion.

As the boundary-waves follow the vessel, their wave-crests and wave-hollows remain in an invariable position relative to the vessel. If the wave-motion gives to the water at a particular spot a velocity *with* the vessel, it would appear as though a bulk of water were being dragged along with her, *although it is really always a new mass of water which follows the vessel for a short distance.* It is exactly analogous to the case of a boat sailing before the wind with just the same speed as a breaking wave at her side. In the case of dead-water, on the other hand, the illusion will be more complete, because the vessel moves at a slow velocity, and the waves causing the motion of the water, are themselves not visible.

It only remains to be seen in what *direction* the surface-water around the vessel will be moved, as a result of the wave-motion. From the photographs Pl. XV—XVII there appears to be a wave-hollow (elevation of the boundary) right around the stern, and if this were the case, the surface-water itself, would have a sternward motion. The photographs Figs. 2 and 3 Pl. XIII — and still better, Fig. 2 Pl. XII — however, show that there is really a wave-crest immediately around the stern, but not very far to the sides of it. *The surface-water nearest to the stern would consequently move in the same direction as the vessel.* Its velocity would not, according to the theory of the boundary-waves, be so great as the velocity of the waves themselves *i. e.* of the vessel (see p. 45); but it will be increased by the suction of the vessel's stern, and the water might therefore follow the vessel. The situation of the boundary-waves relative to the vessel, also agrees very well with the statement (p. 13) that the water is pushed before her stem and is running astern along her sides, at an increased speed. The fact that the wash of a tug does not encounter the bow of a towed vessel, but is pushed aside (p. 14), is obviously a consequence of the forward motion of the water before the bow.

A few simple experiments were made in verification of the above conclusions as to the motion around the stern. A small davit was fitted in such a way to the stern of the boat (the large *Fram*-model; scale 1 : 100) that it could be fixed in different positions. A small wooden bar was suspended from the davit like a pendulum, and reached into the water; its inclination then showed the velocity of the water relative to the vessel. In homogeneous

water, the pendulum was always inclined aftwards, even when hanging as near to the vessel as possible, and this shows that the water had a sternward velocity or else had a forward motion slower than that of the vessel. When the salt-water was covered with a layer of fresh-water and the boat was moving in dead-water, the pendulum if suspended some distance out from the vessel, was inclined aftwards; but if it hung just astern (within 2 or 3 cm. aft of the boat and within about 1 cm. of her median plane), the pendulum was drawn in towards the boat, and was only occasionally repelled. Observation of small particles of dust on the surface, gave the same result. On both sides of the vessel the particles were carried aft past the vessel, but immediately astern they followed her. A grain of dust was once noticed which was carried aft past the vessel, and, when about 5 or 6 cm. astern, was sucked in again towards, and continued to follow the vessel. The phenomenon seemed to be analogous to that of the wake following just astern of a vessel even under ordinary circumstances; but in the case of dead-water this forward-moving wake is much more developed.

According to the above described experiments, the statements that the water appears to be dragged with the vessel, may be supposed to be quite correct; but the area within which the water really follows the vessel, is probably not very wide. If the vessel has a stern broad and bluff, so as to produce a heavy wake, we may expect this area to be comparatively wider than in the case of the *Fram*-model, with which the experiments were made. In every case it might, according to the experiments, be sufficiently wide, for a small boat towed astern, to be drawn up.

Those cases in which it is said that the vessel was partly or completely surrounded by a wide expanse of water dragged along with her, I am inclined to explain as due to an illusion. The accident described by Mr. KROEPELIEN (p. 20; Fig. 1 Pl. V) exemplifies this very well. Behind the two "rips" stretching from the bows, the water is smooth and unrippled; which is easily explained by the dead-water waves steadily bringing up new quantities of water to the surface. On looking at this large, smooth area steadily following the vessel, and at the agitated boundary-lines between it and the water outside, one might well believe that the whole bulk of water was moving through the sea with the vessel. Such a mistake is so much the easier to make, since the speed of the vesse through water, is very small; further, because

the water around the vessel's stern actually follows her, and because the water in front of the "bulk" — just behind the two rips, which evidently indicate wave-crests — has actually a forward-motion against the sea-water outside, and thereby causes a roar. When it is said that the bulk of water, which follows the vessel, comes nearer to or draws farther off from the vessel, according as the wind slackens or increases, the real cause of this observation is obviously to be found in the corresponding changes of position of the dead-water stripes.

When treating of the motion of the water behind the vessel, the oscillatory movements observed by Mr. LARSEN (see p. 14) should also be taken into account. As has already been mentioned on p. 100, they are, however, a so obvious consequence of the "dead-water waves", that no further explanation of them is needed. Mr. LARSEN's statement, that the moving effects of the dead-water waves upon another vessel at her moorings, are greatest in the case of a ship being towed *down* the river, can also be explained. The moving effect will as a rule be greatest at the wave-*crests*, where the fresh-water is thickest, because at the wave-hollows, a vessel of not too small a draught dips into the salt-water below, which has a motion against that of the fresh-water and consequently diminishes its effect. And the velocity of the fresh-water in a *wave-crest* will be increased if the waves are travelling down river, and *vice versâ* if they are travelling up river.

The loss of steering.

The very common fact, that a vessel, when in dead-water, loses her steering-power, finds its simple explanation in the above described motion of the water around the stern. The fresh-water just around the rudder, moves forward with the same velocity as the vessel, or even faster; the salt-water just below, has perhaps a sternward velocity, but since the thickness of the fresh-water is increased just around the rudder (*i. e.* it is a wave-crest there), only a comparatively small part of the rudder is in the salt-water. *A large part of the rudder is consequently working in forward-moving water,* and that thoroughly explains why the power of the helm is lost, or at any rate largely reduced.

The degree and the manner, in which the steering is affected in dead-water, vary infinitely; it is hardly necessary, and it would indeed require too

much space, to try to explain the particular effect upon the steering, in each separate case reported. On the other hand it appears worth while to try to show how the dead-water might affect the steering in the different ways related in the accounts received, and consequently to show that these are not in disagreement with the explanation of the phenomenon here given.

Screw-steamers do not lose their steering at all, because the screw throws a jet of water astern, against which the rudder is able to work.

The steering of sailing-vessels may be affected in different degrees; in some cases the vessel becomes quite unmanageable (see accounts Nos. 5, 6, 7, 8, 10), in others she can still be manoeuvred, but only with difficulty (accounts Nos. 8, 9, 12, 38), again she may even have her steering-power uneffected, while her speed is almost lost (account No. 11). These differences may for instance, be explained as due to different thicknesses of the surface-layer; the thinner the forward-moving surface-layer, the larger will be that part of the rudder which reaches into the salt-water, and has its power unaffected. The shape of the stern will very likely be of importance too, since the forward-motion of the water will be a minimum if the lines of the stern are sharp and fine. Finally, the dead-water may have a very marked effect upon the steering, whenever the wind or current tends to turn the vessel round from her course with great force; while it may be of no influence, if the vessel be set to have no inclination to turn from her course into one or the other direction. In this respect the difference of draught fore and aft, may often be of great importance, as was pointed out by Mr. Colin Archer (p. 15).

Dead-water often causes a vessel to turn in one particular direction, as for instance, when she runs up into the wind and lies with her sails shivering. This is obviously only a consequence of the powerlessness of the helm; for a vessel is usually, owing to the arrangement of her sails, liable to run up into the wind, and she will succeed in doing so, when the power of the helm from any cause becomes reduced.

In other cases a surface-current may cause similar effects, especially if at the same time the steering-power is reduced by dead-water. If the fresh-water is running on the top of the sea-water, as a surface-current somewhat shallower than the draught of the vessel, this will be carried away in the direction of the current, and the sea-water below, having another velocity, will take hold of her keel. It might then take better hold of one end of the

vessel than of the other — especially if she lies deeper aft than fore — and would consequently endeavour to turn her in a particular direction. The ship obviously ought to turn her stem in the direction of the surface-current, just as Mr. ERIKSEN mentions (p. 18). In this way it is also possible to explain, why a vessel in a particular case, could be manoeuvred as long as her course was within the angle between two particular directions, while it was impossible to bring her into a course beyond this angle (accounts Nos. 13 and 14); if the surface-current, under-current, and wind combined, try to keep the vessel in a certain direction of equilibrium, the power of the helm might be sufficient to turn her only to within a certain angle to each side of this direction of equilibrium.

If the surface-current is strong enough, it may, of course, have a similar effect upon the steering, even if the power of the helm be not reduced by dead-water. When the steering is influenced, although the ship keeps her ordinary speed — as Mr. ERIKSEN mentions (p. 18) is sometimes the case in the Kattegat — the cause is certainly a surface-current and not dead-water. It might therefore be possible, that the accident in account No. 14, depended on a surface-current only — especially as no effect upon the vessel, except on the steering, is mentioned. The only circumstance, in this case, which is very characteristic for dead-water and which makes it probable that this has been the cause of the phenomenon, is that the steering was lost *suddenly* when the sail-area was diminished, *and was not regained when the sails were set again.*

The sudden effect produced by the stopping of the engine.

It was noticed that the *Fram,* when in dead-water, stopped dead when the engine was stopped — in one or two or even in only half a ship's-length (see pp. 10, 11, 12), and she even seemed to be sucked back again. Mr. G. A. LARSEN has noticed the same thing when towing lighters at the mouths of the *Glommen* River. According to him, the lighter stops short, and then moves alternately backwards and forwards several times as the "dead-water stripes" pass by, when the engine of the tug is stopped (see p. 14).

Although the explanation of these phenomena is, in reality, very simple, I nevertheless made a few experiments in verification. The larger of the

two *Fram*-models, was dragged through the tank by means of a constant towing weight. When it had gone half the length of the tank or more, I simply took the towing-string with my fingers, and held it steadily slack. The results of these experiments were, on the whole, in accordance with the above mentioned observations; *in homogeneous water* the boat stopped gradually, moving some boat-lengths, after the towing-string had been slackened, while *in "dead-water"* it moved only a few centimetres. In the latter case the motion was furthermore not constantly the same; sometimes the boat stopped short, sometimes it lost its speed more gradually, and then began to sway several times, first forwards and then backwards, as the waves passed it; sometimes the motion was modified in other ways.

The explanation of the phenomenon is quite simple with the help of a concrete case. Imagine the *Fram* moving, first under ordinary circumstances and in another case in dead-water. As will be mentioned in the next section of this chapter, the propelling force of her engine is about 1000 kgr. (or more correctly 1000 g kgr. $\times$ cm. $\times$ second $^{-2}$, g being the acceleration due to gravity); this is therefore, in both cases, the resistance experienced by the vessel when moving at full pressure. Her inertia is about 1 000 000 kgr., when the influence of the surrounding mass of water is taken into account (see the next section of this chapter). When the engine is stopped, the resistance is the only force influencing the vessel; she will therefore have a retardation of $1000\,g / 1\,000\,000 = 1$ cm. $\times$ second $^{-2}$. *I. e.*: in each second, her velocity will be diminished by 1 cm. / second. *Under ordinary circumstances* (in homogeneous water) the *Fram* had a speed of at least 4 knots, or 200 cm. / second. If her resistance were invariably 1 000 kgr., this speed would then be completely lost in 200 seconds, and the space covered by the vessel in this time would be 20 000 cm. or 200 m. The resistance in reality decreases very nearly as the square of the velocity; and supposing this law to be exact, it would be found that 200 seconds after the engine had stopped, the vessel would still have half her original velocity and would have in the meantime moved 277 m. *In dead-water*, her speed was about 1·2 knots, or only 60 cm. / second. The retarding force is, however, the same as in the former case; the ship would therefore lose her speed in 60 seconds, and in this time she would have moved only 1800 cm. or half a ship's-length. It must be remarked, that in this case, in opposition to the former case, *the*

resistance does not decrease correspondingly with the velocity of the vessel since it is chiefly due to wave-making and only to a very small extent to friction. The wave-making resistance depends on the size of the boundary-waves and their position relative to the vessel (see p. 74); and as the vessel stops very quickly, the boundary-waves, owing to their great length and slow speed, do not alter their position relative to the vessel, very much during this time: The resistance might consequently, in a particular case, persist even after the velocity is completely lost, and this explains the observation made on the *Fram*, that *the ship was sucked back again.* This result, namely that the *Fram* when in dead-water, should lose her speed after going a distance of about 18 m., agrees remarkably well with the observations at *Taimur* and at *Bergen.*

If the vessel moves in dead-water in a narrow channel, she will, as shown by the experiments, be followed by a long train of transverse boundary-waves. When she has stopped, she will be overtaken by these waves and will swing to and fro under their action, just as was observed by Mr. G. A. LARSEN. It is here of interest to notice, that Mr. LARSEN's experience is derived principally from working on rivers, where the conditions are more favourable for the formation of transverse waves. In open water, the transverse waves, by spreading to the sides, diminish more rapidly in size at a distance from the vessel; and this is probably the reason why similar swingings were not observed in the case of the *Fram.*

The varying ways in which the boat moved in the different experiments, obviously depend upon the velocity-oscillations, described on pp. 67 seq. The velocity of the boat and the height and the position of the waves relative to the vessel, continually change from one instance to another. As the experiments were not all made in exactly the same way, the values of these quantities — and consequently of the resistance — at the instant at which the towing force was annulled, may therefore have been different in different cases. It is obvious that this accounts for the differences mentioned. I have given reasons for supposing that the velocity-oscillations are on the open sea, always very small; the effect of the dead-water on a vessel, when her engine, is stopped, should therefore be less variable than in the experiments.

According to Mr. G. A. LARSEN, the dead-water stripes may, in particular cases, repeatedly disappear and recur again. This may possibly depend on velocity-oscillations, and as a matter of fact, Mr. LARSEN derives his experience chiefly from towing on a river, where owing to the narrowness of the waterway, these oscillations would be greater than on the open sea.

How to get free of the dead-water.

The principal hindrance caused by the dead-water is, as far as steamers and towed ships are concerned, the loss of speed; while in the case of sailing vessels, the loss of steering is a still greater trouble.

To get a steamer or a towed ship free from dead-water, the point to aim at then, is to regain her speed. Practical experience seems to have discovered just the best possible means of accomplishing this (see the bottom of p. 7), although they have not all been so successfully explained. The simplest way, which also has the advantage that it is practicable for steamers as well as for towed vessels, is just *to stop for a while and then after the boundary-waves have disappeared, to suddenly go full speed ahead again.* It is shown (pp. 75 and 91—92) that a considerably smaller force may be sufficient to get a vessel free from dead-water when she is starting from rest, than when she is moving at a gradually increasing speed; and there is therefore a good chance of succeeding in this way. If necessary, the manoeuvre may be repeated, at suitably chosen intervals. It is said that vessels towed in dead-water have sometimes got free after having run aground (p. 13). Very possibly the simple stopping is the explanation of this, with perhaps the addition of the fact that the steam was at top pressure when the vessel at length got off. But the desired result might have had another important origin, which will be mentioned below.

Several means tried with the object of getting free from the dead-water seem calculated to *mix the fresh surface-layer* with the salt water below. Tug-masters for example sometimes take their tug to and fro along the sides of the towed ship and then make full speed again; or they will shorten the tow-rope so that the screw can stir up the water around the vessel in tow. It will however be shown in the next section of this chapter, that the reduction of the dead-water resistance caused by a moderate mixing of the water-layers, is very small; and to sufficiently affect the water layers in this matter of mixing,

must certainly be a long and difficult work. I am therefore inclined to explain the effect of these methods as *very largely* to be ascribed simply to the fact that *the tug stopped* for a while, when making the manoeuvre. The mixing may in addition have been of some little use, and an even greater effect in reducing the resistance might be ascribed to interference between the systems of waves created by the tug and by the vessel towed, when the tow-rope is shortened. The effect of any of the expedients given in the list on p. 7 in reducing the resistance may certainly be regarded as *nil*, as far as the effectiveness depends on the mixing of the water-layers.

It is said that a vessel sometimes gets loose from dead-water when passed by a steamer (see accounts Nos. 8 and 13). So far as I can see, the only possible explanation of this fact, is that the steamer has produced boundary-waves which, on reaching the vessel, have diminished her resistance at a favourable moment. It is however difficult to understand why a vessel not in dead-water, should generate considerable boundary-waves; and altogether these facts must be accounted among those not thoroughly explained, unless we suppose they depended merely upon chance of some kind, or else that the maximum resistance was very little more than the vessel's propelling force, so that an exceedingly small start was sufficient to quite free her from the dead-water.

There is another very interesting method of getting rid of the dead-water, which may often be very useful, and which is also confirmed by experience (see Kapt. MEYER's account from the *Congo*, in the supplement). It is this: *simply go as near as possible to the shore, where the vessel has only very little water under her keel.* If the fresh-water layer is nearly as deep as the vessel, or deeper, the salt-water layer will then have a very small thickness, (or none); according to equation (3) p. 43 the maximum wave-velocity will then in any case be very small, and the vessel will in consequence easily get rid of the dead-water and may steer from the shore again, if there is no risk of falling into it once more. Even if the fresh-water layer is only about half as deep as the vessel, the resistance will be reduced in the same way; partly owing to the diminished maximum wave-velocity, and partly because the vessel's body when reaching from the surface above to the bottom below, will give to the water particles a nearly horizontal motion and

so will not disturb the boundary[1]. It is clear that a vessel getting free from dead-water after having run aground (see p. 13) might owe a good deal to the simple fact that she was there in very shallow water.

It is obviously more difficult for a sailing vessel to get rid of the dead-water. To stop and then to set all sail again suddenly, will hardly be possible especially as the steering-power is lost. *But by diminishing the effect of the wind upon the sails to a minimum and then after a while increasing it again as quickly as possible,* it might nevertheless often be possible to get free. If the wind-direction, the possibility of manoeuvring, and other circumstances allow, it would certainly be advantageous *to steer in-shore into as shallow water as possible* (see above).

There may be cases in which neither of these methods proves successful or is available, and in which the vessel cannot be freed from dead-water. It will in this case be a great advantage to be able to manoeuvre the vessel, even if her speed be reduced. If there are no strong currents in the water, this would as a rule be possible simply by using a rudder so situated that the water surrounding it has a sternward motion or at least, does not move *with* the vessel. It is shown, that *aft of the stern, to the side of the vessel's midline,* the water is running sternwards at a considerable rate; *and the chances therefore appear very favourable that with an auxiliary rudder or simply with an oar, in this region, the vessel can be steered even in dead-water.*

The occurrence of dead-water.

It follows from the experiments and their discussion in the next section of this chapter, that the resistance due to the generation of boundary-waves, may be considerable if the difference of specific gravity between the surface-water and the sea-water below, be not too small, and if the former have a thickness comparable to the draught of the vessel (between half and twice as great, say). It is not essential that the transition from salt to fresh water

[1] This is obvious, because the motion will then be *symmetrical* with regard to the boundary; at any rate if the vessel has sides vertical from the water-line right to the bottom. It would have been very interesting to have an experimental confirmation of these points, but I did not think of this method of getting rid of the dead-water, before the experiments were finished.

should be very sharp, nor is the vessel's shape of any material importance. It would therefore be expected, that dead-water occurs at about all places where a considerable quantity of fresh-water flows out over the sea.

The answers obtained in reply to an appeal published in foreign newspapers (see p. 8) do not seem to agree with this conclusion. On the contrary, it seems to follow from the replies that dead-water is very little known except in Scandinavia; and only in Norway is it of common occurrence. Only a few cases of dead-water in other parts of the world *e. g.* in the *Mediterranean,* the *Congo* River (see the Supplement), and off the *Fraser* and *Orinocco* Rivers — have come to my notice. In addition, dead-water is, by Norwegian seamen, said to occur on the *Gulf of Mexico* and off the *St. Lawrence* and the great rivers of *South America;* and on the French rivers *Loire* and *Garonne,* phenomena are known which possibly are due to dead-water[1]. (See further Chapter I.)

To completely clear up this apparent contradiction, I neither have a sufficiently complete and reliable knowledge of the actual distribution of the phenomenon, nor am I well enough acquainted with the hydrographic conditions of the coasts, and especially of the river-mouths, in different parts of the world. I will therefore, only try to show here, how the above mentioned facts may probably be explained.

There are three circumstances which must be considered, namely: the conditions of formation of the water-layers, the kind of vessels navigating the place concerned, and the seamen's attention to the phenomenon.

The formation of considerable fresh-water layers by rain-fall, may be regarded as exceptional, and the melting of ice does not come into account in this connection, outside the polar regions; the occurrence of dead-water, must consequently be as a rule, restricted to places where *rivers* fall into the

[1] It may be pointed out that in the case of Accounts No. 32 and 39—42, it cannot be decided whether dead-water has been the cause of the accident, or not. They have been inserted each on its own merits, because no other accounts from the particular neighbourhoods concerned, were available. It is hardly necessary to point out that the fact mentioned in No. 45 p. 32, might for instance just as well have been caused by sludgy ice or some other circumstance resisting the vessels, as by reason of a case of dead-water. On the other hand, it seems very likely that the cases under No. 43 and Account Nr. 44 refer to cases of dead-water — in the last two of these cases the vessel had lost her headway almost entirely, and after a while (when after having stopped rowing, they began again?) she regained her speed.

sea. According to PENCK[1] estuaries are essentially of three kinds, which he calls *bar estuaries* ("Barrenmündungen") *funnel estuaries* ("Mündungstrichter"), and *deltas*. The funnel estuaries are formed by strong tidal currents which wash away the ground as far up the river as they reach. The quantities of sea-water flowing up and down, at flood and ebb, in these estuaries, are many times greater than the river-water carried by the river itself. In very large and full rivers, the river-current is not stopped by the rising tide, and the flood-current runs in as an under-current; if the river carries less water, the direction of the current in the estuary, is determined by the tide. In either case, the river-water and sea-water mix and give brackish water in the estuary (PENCK p. 501). Such places are obviously not favourable for the formation of dead-water. The tides have not had the same erosive effect upon the two other kinds of river-mouths, which are instead formed by the erosive or sedimentary effects of the river-water itself. The channel must consequently be entirely filled up by the river-water; and the particles carried with it, either settle down directly on the bottom or are carried in again from the sea by an under-current of salt-water, to form a bar which silts up the mouth. The river consequently falls directly into deep water. If the mouth is on a straight coast or at a cape, there may often be no conditions favourable for the formation of dead-water, for in such a case the river-water may be either carried off by currents, or the water-layers may be mixed as an effect of ocean waves. Only when the river falls into a deeper estuary or cove, will its water have an opportunity of spreading out over the sea as a distinct layer of gradually decreasing depth; and at such a place dead-water is most likely to occur. [This is the case for example with *Congo* River and *Göta* River.] Even in such places, however, little or no dead-water will be felt, if they are navigated chiefly by steamers, or by vessels of a draught much greater or much smaller than the depth of the surface layer.

In conclusion there are several circumstances necessary, to make the conditions for dead-water really good; and at a large number of river-mouths these conditions are obviously not fulfilled.

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[1] DR. ALBRECHT PENCK. Morphologie der Erdoberfläche. Stuttgart 1894. Zweiter Teil. P. 497 seq.

The long and deep Norwegian fjords with no strong tidal currents, obviously offer excellent opportunities for the river-water to spread without mixing as a layer of gradually diminishing thickness, on the top of the salt-water. Furthermore, they are to a large extent navigated by sailing-vessels (and some of them by towed lighters), which as already mentioned, are particularly susceptible to the influence of dead-water. Norwegian seamen therefore become well acquainted with the phenomenon and easily recognize it when meeting it in foreign seas, as well as on their own waterways. On the other hand, a man who meets it once or twice and has not heard of it before, will not be likely immediately to think that he has to do with a quite new and obscure phenomenon; but he will if possible, put it down to the effect of simple and easily explainable causes, such as *e. g.* currents, whirlpools, muddy water etc. This is so much the more plausible because in many cases, someone or other of these cases has actually contributed its share to the dead-water effect.

It is therefore probable from the seamen's accounts and the experimental investigation conjointly, that dead-water occurs strongly and particularly often in the Kattegat, the Norwegian fjords and in the arctic regions, as well as at some river-mouths in different parts of the world, and that under favourable circumstances it may occur everywhere where fresh-water flows out over the sea. On looking through the collection of accounts of dead-water beyond Scandinavia (in Chap. 1 and in the Supplement), it seems highly improbable that they should supply a complete and correct representation of the geographical distribution of the phenomenon; on the contrary, the places mentioned in this collection more probably represent just the few cases which have by chance come under notice. It is to be expected that when the phenomenon has been more generally noticed, we shall soon get a fuller knowledge of its occurrence in different parts of the world.

F. EMPIRICAL LAWS OF RESISTANCE IN DEAD-WATER.

Influence of the difference of spec. gravity between the water-layers; the different causes of resistance.

Fig. 1 Pl. XI shows different curves of resistance for the larger *Fram*-model (1 : 100). The lowest curve refers to homogeneous water. The other

full-drawn curves apply to the case of a fresh-water layer af 5 cm. depth; the specific gravity of the salt-water below, in each case (1·010—1·160), is given over each curve. The velocities of the boat-model, are plotted horizontally in cm. per second, and the resistance to the boat, vertically in grammes. The vertical distance between the lowest curve and one of the others, gives, for the different velocities, the "dead-water resistance" in the case concerned.

As might be expected, *the effect of the fresh-water layer is, upon the whole greater, the greater the specific gravity of the salt-water below.* The exact relationship is very simple. In Chap. II (p. 52) it was shown theoretically that *if the difference of specific gravity between the upper and the lower water-layer be increased γ times, the wave-making resistance against a vessel in dead-water, will increase in the same ratio, if at the same time her velocity be $\sqrt{\gamma}$ times increased.* It was also shown that the same rule holds approximately true for the *frictional* resistance. It will be shown below, that the "dead-water resistance" depends chiefly on wave-making, and only to a smaller extent on friction; and the above rule should therefore hold very nearly true for the entire dead-water resistance. The experiments are in complete agreement with this rule; as is best seen if it be applied to different series of experiments in reducing them all to the case of a difference of spec. gravity equal to 0·030, say. For instance, the series of experiments Nos. 133—137, 138—145, 85½—97, 77—85, 146—153, 154—164 (see the tables pp. 80—82), were all made in a 5 cm. deep fresh-water layer, but the spec. gravity of the salt-water below was 1·010, 1·020, 1·030, 1·032, 1·070, 1·160 respectively. If then, in the 1st series the velocities be multiplied by $\sqrt{3}$ and the corresponding dead-water resistances by 3, in the 2nd series, by $\sqrt{3/2}$ and 3/2 respectively, in the 4th series by $\sqrt{3/3{\cdot}2}$ and 3/3·2, and so on, and if resistance curves be drawn for each series of reduced values, it will be found that they all coincide very nearly with oneanother, and the divergences themselves are chiefly of an experimental character and not systematic. The maximum resistance does not differ in any of the reduced curves by more than 0·2 gr., from that found by direct experiments with salt-water of specific gravity 1·030. The agreement is just as good when the series 58—71 and 126—132 are compared (fresh-water layer 3 cm.; spec. gravity of the salt-water 1·030 and 1·180). Considering that the specific gravity in these experiments, varied by as much as from 1·01 to 1·16 and

from 1·03 to 1·18, it is clear that for variations actually occurring on the sea, the rule will hold almost exactly.

The curves in Fig. 1 Pl. XI, show the remarkable fact, that within a certain range of velocities, *the resistance may be increased by diminishing the difference of spec. gravity between the two water-layers,* and *vice versâ.* For instance, at a velocity of 7 cm. per second, a difference of spec. g. of 0·030 gives a resistance of 1·8 gr., and a difference of spec. g. of 0·160, 0·6 gr. only. The reason is evidently that in the latter case, the difference of spec. g. was too great for the vessel to give rise to any appreciable waves at that velocity; indeed in this case it was not possible from the side of the tank, to detect the least disturbance in the boundary[1], while at the same velocity, and with a difference of spec. g. of 0·032, the waves were very considerable (see Pl. XVI Figs. 2 and 3)[2]. If the bottom-layer were mercury instead of salt-water, the boat would generate practically no waves within the range of velocities used in the experiments, and would move with just the same resistance as if the boundary were replaced by a rigid bottom (the depth of the surface-layer being more than the vessel's draught). When the spec. g. of the salt-water is sufficiently increased, the resistance at a given velocity will diminish towards this limit. The resistance-curve in 5 cm. of homogeneous water should consequently be something like the dotted curve in Fig. 1 Pl. XI — or perhaps it should approximate somewhat more nearly, to the curve for salt-water 1·160 — *i. e.* the resistance should be somewhat more than twice the resistance in deep homogeneous water.

In conclusion, the resistance in "dead-water" may be subdivided in various ways. The difference between the resistance in "dead-water" and in homogeneous deep water, is the above mentioned "dead-water resistance".

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[1] In experiment 155, only very slight diverging waves could be seen through the clear fresh-water from above. In experiment 157 they were very beautiful, and very low transverse waves could also be detected although with difficulty. Only when the velocity was at least 12 cm. per second (experiment 159), were the waves large enough to be seen from the side of the tank.

[2] The wave-making resistance might of course, increase, with the difference of spec. gravity although the wave-height decreases; because the wave-energy is proportional to the difference of spec. gravity. The latter circumstance is however, insufficient to balance the effect of the largely decreased wave-height. It follows from the rule p. 52 that the wave-making resistance (or the dead-water resistance) will increase with decreasing difference of spec. gravity, if it increases according to some higher power than the square, of the velocity; the curves in Fig. 1 Pl. XI, show an actual increase even equal to the *sixth* power of the velocity.

It is due to the *wave-making* as well as to the *effect of shallowness.* The latter part of the resistance depends on the water's diminished passage past the vessel; it is at the lowest velocities, approximately represented by the vertical distance between the dotted curve and the lower continuous curve, in Fig. 1 Pl. XI. At higher velocities, it is smaller than this, and at velocities greater than the maximum wave-velocity, it practically disappears altogether. That it does so is obvious, because the entire dead-water resistance vanishes; the physical reasons for it are indicated in Chap. II, p. 47. The resistance represented by the dotted curve, may be called "shallow homogeneous-water resistance". At below the critical velocity the resistance may be divided into two parts, of which the one is approximately equal to the "shallow homogeneous-water resistance", and the other is the "wave-making resistance" due to boundary-waves. According to the arguments brought forward in Chap. II (p. 47), the resistance in homogeneous deep water, as well as the resistance due to shallowness, depends partly on friction and partly on pressure, while the wave-making resistance depends essentially only on the latter.

The relationship between the different curves in Fig. 1, Pl. XI, shows clearly and conclusively that the greatest part of the dead-water resistance, is due to wave-making. From the measurements of surface-disturbance (see p. 66 and the diagrams Pl. IX) we are able to approximately calculate the pressure at different points. The pressure-resultants against the boat-model, calculated in this way are in complete agreement with the above estimation of the different parts of the resistance and their causes; and although this calculation cannot be more than a first approximation, it may therefore be of interest to cite it here.

The table on the next page contains the chief results of the measurements of surface-disturbance. The experiments were made with the larger *Fram*-model. The numbers in the *1st column*, refer to the corresponding numbers of the diagrams in Fig. 3 Pl. IX. The *2nd—6th columns* have the same signification as the 3rd—7th columns in the tables p. 77. The *7th column* gives the final velocity, or if there were velocity-oscillations, the mean towards which it approximated. The *8th column* gives the greatest difference of level of the water-surface, measured on the original drawings, which are copied in Pl. IX.

If the waves were moving freely, solely on account of gravity and the inertia of the water, it should be possible to calculate the ratio between the wave-heights at the surface and in the boundary, according to equation (1) p. 42; in this case the moving forces are concentrated in the neighbourhood of the salt-water fresh-water boundary. Actually however, the waves nearest to the vessel, are *forced*; and the moving forces are to a certain extent distributed over the vessel's surface, *i. e.* in the middle of the surface-layer. In this case it is obvious that the corresponding equation for long waves (2, p. 42) gives a better approximation; the *9th column* gives the greatest difference of level in the boundary,

calculated in this way. Considering that the surface-disturbances must be measured somewhere in between the walls of the tank and the course of the vessel, these values agree as well as could be expected, with the wave-heights shown by the photographs Pl. XV—XVI.

Number of the experiment.	Water-layers		Starting force		Towing force, grammes	Velocity, cm./sec.	Greatest surface-disturbance in mm.	Height of boundary-waves, calculated in mm.	Difference of surface-level		Pressure resultant	
	spec. g. of salt water	Depth of fresh water	grammes	centimetres					stem to stern (h_1-h_4)	10 cm. from stem to 10 cm. from stern (h_2-h_3)	Observed	Calculated
1	—	37 cm.	2·5	40	1·0	17·0	0·15		0·15	0·11	0·45	
2		"	2·5	90	2·5	27·0	0·40		0·4	0·25	1·1	
3	1·070	5·0 cm.			1·0	7·3	0·32	4·9	0·2	0·1	0·5	0·63
4	"	"			1·5	8·2	0·52	8·0	0·5	0·25	1·25	1·17
5	"	"			2·0	8·6	0·77	11·8	0·65	0·3	1·6	1·64
6 [1]	"	"			3·0	9·5	1·2(?)	18(?)	0·9(?)	0·45(?)	2·25	2·56
7 [2]	"	"	4·5	70	3·0	27	0·49	7·5	(about nul)			
8	1·030	3·0 cm.			1·0	5·2	0·60	21	0·4	0·17	0·75	0·81
9	"	"			1·5	7·6	0·94	32	0·6	0·3	1·2	1·12

The pressure-resultant against the boat-model, is calculated for its middle-body and for its ends, separately. The calculation is made in the following way; the first line in the table on the next page, gives the area of the midship-section of the boat model (below the water-line), as well as those parts of it which lie *above* and *below* a level 3 cm. below the water-surface, and the two lines following, give the means for the corresponding areas at equal distances from the stem and the stern. From these areas, mean cross-sections are calculated for the middle-body reckoned to 10 cm. from the ends, and for the rest of the boat. The former numbers (A) and the latter (B), as well as the differences (A—B), are given in the same table.

If the water-pressure in one horizontal plane, be p_1 at the stem, p_2 10 cm. aft of the stem, p_3 10 cm. before the stern, and p_4 at the stern, then the sternward pressure-resultant R will be given approximately by

$$\begin{aligned} R &= B(p_1 - p_2 + p_3 - p_4) + A(p_2 - p_3) \\ &= B(p_1 - p_4) + (A - B)(p_2 - p_3). \end{aligned} \qquad \text{(a)}$$

The values of p_1, p_2, p_3, p_4, have to be calculated from the surface-disturbances. As was pointed out above, the moving forces are not concentrated only in the salt-water fresh-water boundary, but are also distributed in the middle of the surface-layer; and the formulae for the case of "long waves" (5 and 6, p. 44) therefore give better approximations

[1] The disturbance of surface-level was greater than could be measured with the level-gauge. The dotted part of diagram 6, is therefore hypothetical.

[2] The diagram could not, in this experiment, be traced accurately enough, to permit of a calculation of the pressure-resultant.

than the exact formulae for free waves. As far as the pressure in the salt-water is concerned, only a rough approximation is necessary, because the corresponding terms in the expression for the resistance, become very small. Equation (5) p. 44 is used, as well for homogeneous water as for a surface-layer. In all the formulae, g (the acceleration due to gravity) may be thrown out, if *gramme* be used in its frequent sense of *force*.

	Areas of cross-sections		
	entire	0—3 cm. below surface	3—5 cm. below surface
Midship-section	36 cm.²	28 cm.²	8 cm.²
Mean of sections 10 cm. from stems	32 —	26 —	6 —
" " " 5 " " "	17 —	15 —	2 —
Mean section of middle-body (A)	34 —	27 —	7 —
" " " the ends (B)	16 —	14 —	2 —
(A—B)	18 —	13 —	5 —

1) *Homogeneous water.*

The values of B and (A – B) in (a) are to be taken from the first column in the table above. If h_1, h_2, h_3, h_4, are the heights of the surface-level (in *millimetres*) at the sections to which p_1, p_2, p_3, p_4 refer, $(h_2 - h_3)/10$ and $(h_1 - h_4)/10$ may be substituted for $(p_2 - p_3)$ and $(p_1 - p_4)$, and (a) becomes

$$R = 1{\cdot}6\,(h_1 - h_4) + 1{\cdot}8\,(h_2 - h_3) \text{ grammes.} \quad \text{(b).}$$

2) *Fresh-water layer 5 cm. deep.*

The boat moves entirely in the fresh water. We have just as in the former case,

$$R = 1{\cdot}6\,(h_1 - h_4) + 1{\cdot}8\,(h_2 - h_3) \quad \text{(c).}$$

3) *Fresh-water layer 3 cm. deep.*

The pressure of the fresh-water and of the salt-water, must be calculated separately, according to formulae (5) and (6) p. 44. As the wave-length l is about 90 cm., $2\pi d/l$ may be put equal to 0·2. The values of B and (A – B) in the 2nd and 3rd columns of the table above, are to be used. We then obtain

$$\begin{aligned} R &= (1{\cdot}4 - 0{\cdot}2 \times 0{\cdot}2)\,(h_1 - h_4) + (1{\cdot}3 - 0{\cdot}2 \times 0{\cdot}5)\,(h_2 - h_3) \\ &= 1{\cdot}36\,(h_1 - h_4) + 1{\cdot}2\,(h_2 - h_3) \text{ grammes.} \quad \text{(d).} \end{aligned}$$

The differences of level $(h_1 - h_4)$ and $(h_2 - h_3)$ are found in the *10th* and *11th columns* of the table p. 119; the pressure resultant calculated according to one of the formulae (b), (c), or (d), is given in the *12th column.*

The acceleration of the boat-model in the two first experiments, was inappreciable, and the entire resistance was consequently equal to the towing-force. The numbers in the 6th and 12th columns of the table p. 119, then show, that in homogeneous water, about 45 per cent of the resistance was due to pressure, and only the other 55 per cent to friction. Indeed a certain pressure is necessary to overcome the friction against the water moving past the vessel astern; another part of the pressure-resultant is identical with the eddy-making resistance.

It is therefore assumed that even in dead-water 45 per cent of the resistance not due to wave-making, depends on the pressure alone and that the rest is due to frictional resistance. *The pressure resultant would then be equal to the wave-making resistance plus*

45 per cent of the rest, or approximately, to the whole resistance less 55 per cent of the shallow homogeneous-water resistance. The whole resistance may be assumed equal to the towing force found in the 6th column of the table p. 119. The "shallow homogeneous-water resistance" is, in experiments 3—6, easily found by means of the velocity given in the table, and the dotted curve in Fig. 1 Pl. XI. For experiments 8 and 9 it may be found in the same way, if a curve corresponding to the dotted curve Fig. 1 Pl. XI be drawn by using the experiment 126—132. In that way, the pressure-resultants found in the *13th column* of the table p. 119 were calculated. They agree, as well as could be expected, with the values in the 12th column, found from the observations of surface-disturbances. (It may be pointed out here, that if in the calculation of the former values the entire resistance be not assumed equal to the towing force but be determined according to the resistance-curves in Fig. 1 Pl. XI and Fig. 1 Pl. X, from the velocity, the agreement with the values in the 12th column will be found almost exact). In this agreement we have a sort of control upon the correctness of the explanation of the dead-water resistance.

The influence of the depth of the fresh-water layer.

Owing to the simple connection between the difference of spec. gravity of the water-layers and the dead-water resistance, it was not necessary in what follows to vary the specific gravity of the salt-water. This was therefore the same in most of the experiments, namely 1·030.

Fig. 1 Pl. X shows the resistance-curves for the larger *Fram*-model in a fresh-water layer of 1, 3, 5, 7, 9, and 13 centimetres' depth, as well as in homogeneous water. The velocities are plotted horizontally in cm./second, and the resistances vertically in grammes. The influence of the fresh-water layer upon the resistance, decreases, as might *a priori* be expected, when its depth becomes either very small or very great. It is (at most velocities) greatest, when the fresh-water is 4 or 5 cm. deep[1], *i. e.* about the same as the draught of the boat; a smaller as well as a deeper fresh-water layer causes a smaller resistance. It is easy to find a plausible explanation of this latter fact. On the one hand, the wave-making power of the boat, is smaller, the farther the latter is from the boundary where the waves are generated, and on the other hand, its wave-making power will be diminished, if it reaches into the salt-water, since that part which is *below* the boundary and that *above* it, counteract oneanother. Furthermore, if the depth of the surface-layer is small, it will put a limit to the height of the waves which can be generated, and consequently to the wave-making resistance.

[1] To keep the figure clear, the curve for fresh-water 4·0 cm., is omitted. It may easily be drawn from the numbers in the table (p. 79). The maximum resistance is found to be 2·2 grammes.

It is often believed that the great resistance to a vessel in dead-water, is felt only when the vessel reaches into the salt-water. This opinion, which is not in accordance with the explanation of the phenomenon as an effect of wave-generation, is not confirmed by the experiments. On the contrary, they show that when the depth of the surface-layer is even twice as great as the draught of the vessel, its effect upon the resistance is still very considerable, the resistance being at certain velocities, four times as great as in homogeneous water, and the maximum resistance being only about 40 per cent smaller than when the depth of the surface-layer is the same as the vessel's draught. In the latter case its effect upon *the resistance* has its greatest value, as has already been mentioned. On the other hand, the curves show, that when a vessel pulled by a given force, is in dead-water, *her velocity* is, as a rule, slowed down more, the thinner be the fresh-water layer.

It would be useless to try to express the above mentioned experimental results as a mathematical formula, it is better to use them, as they are given in the diagrams.

The resistance-curves in Fig. 1 Pl. X clearly show their connection with the maximum velocity of the boundary-waves. The latter quantity is represented at the top of the figure, by a small circle containing a number indicating the depth of the fresh-water layer in centimetres, and by a small pointer below indicating the maximum wave-velocity. For instance, it is 5·35 cm./second in the case of a 1 cm. fresh-water layer, 9·00 cm./second in the case of a 3 cm. layer, and so on[1]. The velocity at which the dead-water resistance is a maximum, cannot of course, be determined very accurately, although the resistance-curves are drawn from the experiments as exactly as possible. According to the resistance curves, it is in most cases somewhat smaller than the maximum wave-velocity; the ratio increases with decreasing depth of the surface-layer, being 0·7 or 0·75 if this is 9 or 13 cm., 0·9 if it is 3 cm., and even above unity if it is only 1 cm. When the depth of the surface-layer

[1] The numbers are calculated according te equation (3) p. 43, and the finite depth of the salt-water, is consequently taken into account. If the latter had been very deep, the maximum wave-velocities would have been somewhat greater. The difference would have been only 1 per cent for a 1 cm. surface-layer, but 7 per cent for a 5 cm. surface-layer and 20 per cent for a 13 cm. surface-layer. The relationship given in Fig. 1 Pl. X, between the resistance and the depth of the surface-layer, may there fore be regarded as chiefly qualitative, as far as the greatest depths are concerned.

was small, the boat however, generated a so called solitary wave, which on account of its considerable height, may have had higher velocity than the critical velocity (3) p. 43. The velocity of solitary waves in shallow water is given by the same formula as that of long waves of small amplitude, if the depth of the water be only reckoned to the top of the wave (see foot note 1, p. 40). Rough experiments indicated, as might be *a priori* expected, that solitary boundary-waves follow the same law. If the maximum wave-velocity be calculated in this way by measuring the depth of the surface-layer at the crest of the solitary wave, it will be found that the velocity corresponding to the maximum dead-water resistance, has a nearly constant ratio to it (about 0·73). That this ratio remains unchanged when the difference of spec. gravity between the water layers is varied, is a consequence of the rule on p. 52 and equation (3) p. 43, combined.

The influence of the sharpness of the boundary.

The actual conditions are in reality not so simple as they were arranged in the experiments; the transition from heavy salt-water into light fresh-water is in general not quite sharp, but is extended over a considerable region in which the salinity decreases upwards. Experiments 165—184 pp. 82—83, are intended to show the influence of this circumstance; and the results are represented in Fig. 2 Pl. XI.

The vertical distribution of spec. gravity in these experiments, is represented by accessory, step-shaped diagrams in the same figure. In Case (2), for instance, the spec. gravity is 1·000 down to 4 cm. below the surface, 1·010 from 4 to 5 cm., 1·020 from 5 to 6 cm., and 1·030 at 6 cm. below the surface and down to the bottom. In both cases the water-layers may be imagined as formed by the partial mixing of an originally fresh, water layer and a bottom-water of spec. gravity 1·030. These imaginary, sharply defined water-layers are represented on the diagrams by dotted curves (the spaces between the dots are unfortunately too small, so that they look almost like full-drawn curves). In Case (1) the mixing is very thorough, in Case (2) it is less complete; in both cases, the mixing proceeded during the experiments, so that the broken lines on the diagrams, passed into smooth curves. The water-layers seemed on the whole to be more apt to mix in these experi-

ments than in the case of *two* sharply separated layers, apparently because of the small difference of spec. gravity between each two adjacent water-layers.

The resistance in the "mixed" water-layers is represented by the heavy curves 1 and 2. For the sake of comparison, faint curves representing the resistance in the corresponding unmixed water-layers (fresh-water layers, 3 and 5 cm. deep respectively; saltwater of specific gravity 1·030) are also given. The velocities seem to be upon the whole, a little more reduced in the case of the mixed water-layers than in the case of the corresponding pure fresh-water layer; and the reason is obvious, because in the former case a smaller velocity is sufficient to disturb the equilibrium of the water-layers, and consequently to cause wave-motion. But this difference is of no practical importance. On the other hand, *the maximum resistance is somewhat reduced by the mixing.* The reduction is however, not very considerable; the maximum dead-water resistance being only diminished by 11 per cent — and the entire maximum resistance by 14 per cent — in Case (1), in which the mixing extended uniformly right up to the surface.

The fact that in the sea, the lighter surface-layer is in general not sharply defined from the bottom-water, will therefore not invalidate the applicability of the experimental results. In any case it will be possible to make a suitable correction by help of the curves Fig. 2 Pl. XI. Fresh-water layers as sharply defined from the salt-water below, as in diagram (2) — or even sharper — are not unrare near the coasts.

Application of the experimental results to the case of full-sized ships.

FROUDE's rule (p. 51) was proved to hold exactly for wave-making resistance in frictionless water, and reasons were given (p. 53) that it should hold approximately for the frictional resistance. It may therefore be expected to hold very nearly true for the entire dead-water resistance, since the greatest part of this is caused by wave-making. In other words: *if the dimensions of the vessel, the depth of the water-layers, and other linear dimensions are increased in the proportion* λ, *and if the vessel's velocity is increased in the proportion* $\sqrt{\lambda}$, *the dead-water resistance will increase as* λ^3.

For a rough verification of this rule, we may compare two series of experiments, one with the small *Fram*-model, and one with the large one

(experiments 11—17 and 72—76). The small model was drawn in the small tank, and the large one in the large tank, in fresh-water layers of 2 and 4 cm. depth respectively. The dimensions of the boat-models are then in the same ratio as the depths of the surface-layers; but the width of the tank was, by comparison, a little greater in the latter case. The spec. gravity of the salt-water was in both cases, 1·030. The results of the experiments with the small model are represented graphically by Curve (4) in Fig. 8 Pl. VI. From the results of the experiments with the large model we shall by FROUDE's rule calculate the resistance against the small model and afterwards compare with the directly observed values. The calculation is inserted here as an example.

Experiment 72 shows a resistance to the large model, of 1·5 gr., at a velocity of 6·5 cm./second. These figures are found in the first and second columns in the table below (first line).

The *Fram*, scale 1:100 Spec. g. of salt w. 1·030; surface-layer 4 cm. (observed values)			The *Fram*, scale 1:200 Spec. g. of salt w. 1·030; surface-layer 2 cm. (calculated values)		
Total resistance	Velocity	Dead-water resistance	Dead-water resistance	Velocity	Total resistance
1·5	6·5	1·32	0·165	4·6	0·20
2·0	7·5	1·76	0·220	5·3	0·26
2·15	10·5	1·72	0·215	7·4	0·29
2·05	11·0	1·58	0·198	7·8	0·28
1·5	13·3	0.83	0·104	9·4	0·22

The resistance in homogeneous water at the same velocity, is according to Fig. 1 Pl. X, 0·18 gr.; the difference 1·5—0·18 = 1·32 is the dead-water resistance and is given in the third column.

Now $$\frac{1{\cdot}32}{2^3} = 0{\cdot}165; \qquad \frac{6{\cdot}5}{\sqrt{2}} = 4{\cdot}6.$$

The dead-water resistance experienced by the *small* model at a velocity of 4·6 cm./second, should then according to FROUDE's rule, be 0·165 gr. These figures are given in the 4th and 5th columns. According to Curve (1) in Fig. 8 Pl. VI the resistance in homogeneous water, at the last-mentioned velocity, is 0·032 gr. This added to 0·165 gives the total resistance against the small model, and the rounded result 0·20 gr., is found in the 6th column. The other figures in the table are calculated in the same way.

The values of velocity and resistance calculated in this way, are represented in Fig. 8 Pl. VI by the faint curve (5). The curves (4) and (5) should consequently coincide if the measurements, and the method of comparison were exact. The agreement may be regarded as very good, and as far as the maximum resistance is concerned, the difference lies within the limits of experimental errors. The difference between the two curves at velocities below the critical one, will be explained below, as due to the different width of channel. The application of the experimental results to the case of vessels on a different scale, may consequently be made according to the scheme used in the Table p. 125.

The agreement between Curves (4) and (5) in the case of the maximum-resistance, does not exclude the possibility of appreciable errors, if the measurements be applied to the case of full-sized ships. For the larger *Fram*-model must have its dimensions 6·6 times doubled in order to attain the size of the *Fram* herself (the power 6·6 of 2, is 100). Supposing that the agreement between Curves (4) and (5) for the maximum dead-water resistance, is trustworthy to within 5 per cent of the measured quantity, the experimental result when applied to the case of the *Fram* herself, might then include an error of as much as 38 per cent (the power 6·6 of 1·05 being 1·38). In any case the above comparison, proves that FROUDE's rule gives results which are of the right order of magnitude; and there is apparently no reason for supposing that it should not be practically exact.

We may conclude also from experiments with salt-water of different spec. gravities, that the friction had not so much influence in the experiments as to destroy their applicability on a larger scale. For by using the artifice on p. 52 it follows from Equations (*b*) and (*c*) p. 50, that the influence of the friction upon the motion, will be diminished just as much when increasing the linear dimensions in the ratio r, as when increasing the difference of spec. gravity Δq in the ratio r^3. As the agreement between experiments with differences of spec. gravity = 0·01, 0·02, 0·03, 0·07, and 0·16, is very good (see p. 116), we may therefore expect the same agreement between for example, experiments on the scales 1 : 100 and 1 : 50 with a difference of sp. gravity 0·02; and when the scale is further increased, the agreement is likely to be so much the more certain. The same conclusion could be drawn from the fact that the amplitude of the velocity-oscillations, was just as great when the difference of spec. gravity was small, as when it was great; because the velocity oscillations are more stifled by friction, the smaller the difference of spec. gravity.

One more circumstance must be examined before applying the results of the experiments. *They were performed in a rather narrow channel,* while in cases actually interesting us, the vessel is moving on a more or less open and extensive water-surface. The effect of this circumstance, upon the frictional resistance and upon the sternward-pressure caused by it, has been mentioned in section *D* of this chapter; and it was then pointed out that in any case it would not appreciably influence the "*dead-water* resistance". On the other hand, the wave-making resistance might be influenced, the waves being to a greater extent *transverse* waves, when the vessel is moving in a narrow channel.

To get an idea of the degree to which the experimental results must be corrected on account of the narrowness of the channel, we may compare two series of experiments made in the large tank, one series with the small *Fram*-model and one with the large model. The former series (experiments 284—288) was made in a 2·5 cm. thick fresh-water layer on the top of salt-water of sp. gravity 1·180, and the results are represented by Curve (2) ("wide channel") in Fig. 9 Pl. VI; Curve (1) in the same figure, gives the resistance in homogeneous water according to experiments 278—280. The second series (experiments 154—164) was made in a 5 cm. thick fresh-water layer on the top of salt-water of specific gravity 1·160. We can in the way indicated on p. 116, construct from the resistances in this case, the resistance-curve for the case of salt-water of specific gravity 1·180. And further, we may from this curve according to the scheme on p. 125 construct the resistance-curve for the case when all linear dimensions are diminished in the proportion 1 to 1/2. For this purpose, the dead-water resistances are multiplied first by 18/16 and then by $(1/2)^3$, that is by 9/64; and the velocities are multiplied by $\sqrt{18/16}$ and by $1/\sqrt{2}$, *i. e.* by 3/4. The calculation is given in the table below, just as in the table on p. 125.

The *Fram*, scale 1 : 100 Spec. g. of salt-water 1·16; fresh-water layer 5·0 cm. Breadth of the channel 40 cm. (Observed values)			The *Fram*, scale 1 : 200 Spec. g. of salt-water 1·18; fresh-water layer 2·5 cm. Breadth of the channel 20 cm. (Calculated values)		
Total resistance	Velocity	Dead-water resistance	Dead-water resistance	Velocity	Total resistance
0·25	4·0	0·15	0·02	3·0	0·04
0·50	6·5	0·31	0·04	4·9	0·08
0·75	7·8	0·50	0·07	5·9	0·12
1·0	9·0	0·68	0·10	6·7	0·17
2·0	11·5	1·49	0·21	8·6	0·30
3·0	12·0	2·45	0·34	9·0	0·44
5·0	13·6	4·30	0·61	10·2	0·73
7·0	14·7	6·19	0·87	11·0	1·00
9·0	15·6	8·10	1·14	11·7	1·28
9·5	<22	7·75	1·09	<16·5	1·35
9·3	25·4	6·98	0·98	19·0	1·31

The values in the last column are obtained by adding to the numbers in the 4th column the resistances in homogeneous water, found from Curve 1

in Fig. 9 Pl. VI. By means of these values and the corresponding velocities in the 5th column, Curve (3) in the same figure, is constructed. This curve then, represents the same case as Curve (2), only with the difference, that *in the latter case, the boat is moving in a 40 cm. wide channel, while the former case refers to a 20 cm. wide channel.* [The numbers in the 6th column should really be calculated by adding to the numbers in the 4th column the resistances in homogeneous water in a *20* cm. wide channel. But as our object is to compare the *dead-water* resistances, it was more suitable to reckon them from a common curve, *e. g.* from Curve (1). In any case the difference is very small.]

A comparison between the two curves seems to prove that the narrowness of the channel has no remarkable influence on the maximum resistance as it has been defined on p. 90. The velocities corresponding to given resistances are, however, somewhat smaller in the narrow channel than in the wide one[1]. As shown by Fig. 1 Pl. I, the large tank is rather spacious for the small boat-model; and the photographs on Pl. XIII seem to show that the waves have spread just as in open water. It is therefore probable that the resistance in open water would follow a curve, much nearer to Curve (2) than the latter is to Curve (3). As the influence of the narrowness of the channel is then, not very great and, especially as it does not effect the maximum resistance appreciably, it is not necessary to make any allowance for it in applying the results.

Unfortunately, hardly sufficient experiments were made with the small boat-model in the large tank, to answer the above questions so completely and positively as had been desirable. At the time, it was not noticed that these particular experiments would be of such especial interest, and it was afterwards too late to have them repeated.

The influence of the shape of the boat-model.

According to some of the narrators in Chapter I, vessels of different shapes should be to different extents, liable to dead-water. Their statements

[1] It seems therefore probable that the difference between the resistance-curves (4) and (5) in Fig. 8 Pl. VI (see p. 126) depends on the different width of the channels. For, as shown by Fig. 1 Pl. I, the small tank is narrower compared with the small *Fram*-model, than is the large tank compared with the large *Fram*-model.

in this respect are, however, not very reliable and definite, and a few experiments were therefore made to determine with certainty whether vessels of different shapes are influenced in markedly different degrees. The boat-models used in these experiments, had about the same size as the larger *Fram*-model; their length at the water-line was the same, namely 36 or 36·5 cm. Other principal dimensions etc. are given in the table below.

Boat-models.

Number of the model	Description	Weight in grammes	Wetted Surface cm.^{2}	Draught			"Drag" or Trim
				For	Aft	Middle	
1	(The *Fram*). Round-bottomed	800	450	4·2	4·7	4·4	0·5
2	The same, trimmed	800	450	3·2	5·7	4·4	2·5
3	Flat-bottomed model	1050	527	4·4	4·4	4·4	0
4	Sharp-bottomed model	926	574	7·0	7·0	7·0	0
5	Thin rectangular board . . .	230	565	0·5	0·5	0·5	0

Model 1 was the before mentioned model of the *Fram*; its lines are given in Fig. 8 Pl. I, and its athwart-section in Fig. 1 Pl. X. Model 2 is the same as No. 1, only that the loading weight was moved from midships to the stern so as to make the trim greater. Models 3 and 4 have different midship-sections, as may be seen in Pl. X. In other respects, the models 1, 3, and 4, are made as nearly as possible similar to one another, so far as is consistent with the different midship-sections; Model 3 had a stem somewhat more bluff below the water-line, than had the others. Model 5 was only a thin rectangular board, 36 cm. long and 14·7 cm. wide and dipping 4 or 5 mm. into the water; at its ends it was thinned out to nothing[1].

Experiments with Model 2 were made only in a surface-layer of 5 cm. thickness (Experiments 185—194 p. 83). If the resistance-curve is drawn according to the results obtained, it will be found almost identical with the corresponding curve for Model 1. The resistance in homogeneous water, may be assumed to be nearly the same for both models. As the trim of Model

[1] It should have been mentioned on p. 59, that all boat-models except the one used in the small tank, were made at "*Liljeholmen's* boat-building yard".

2 is very considerable, it may therefore be concluded that *the trim of a vessel has practically no influence on the dead-water resistance.* In what follows, only the *mean draught* is therefore taken into account.

The experiments with Models *3* and *4* were to show the influence of the athwart-section of the vessel. They represent two extreme types of vessels which are both used, and which may be characterized by a nearly *rectangular section* (flat-bottomed vessels) and a nearly *triangular section* (sharp-bottomed vessels). Between these extremes is Model 1 with a round (circular or elliptical) athwart-section. The resistance-curves Pl. X seem to show that there is a marked difference of resistance to the 3 models. I shall here only compare the maximum dead-water resistance, which is of special interest to us; the corresponding velocities of the different boat-models are practically equal to one another. The following table gives the maximum dead-water resistance to each model, in water-layers of different thickness; the spec. gravity of the salt-water being in all cases 1·030.

Depth of fresh-water	Maximum dead-water resistance		
	Model 1	Model 3	Model 4
1 cm.	0·30 gr.		
3 —	1·35 -	1·53 gr.	1·37 gr.
4 —	1·84 -		
5 —	1·75 -	2·15 -	1·70 -
7 —	1·30 -		
9 —	1·00 -	1·56 -	1·42 -
13 —	0·60 -		

As, however, the models 1, 3, and 4, have not the same weight and draught, it is not possible by directly comparing these numbers to find the influence of the different athwart-sections; it is necessary for that purpose to reduce them to refer to vessels of the same weight and the same mean draught, but with different cross-sections. To directly apply the results to the case of full-sized ships, I have chosen the weight 800 tons (800000 kgr.) and 4·4 m., which is the mean draught of the *Fram* at that displacement. First imagine all linear dimensions (particularly the depth of the surface-layer and the dimensions of the boat-model) increased in the same ratio, so that its draught

becomes 4·4 m. According to FROUDE's rule the maximum dead-water resistance will then increase in the same ratio as the vessel's displacement. If further, by a moderate alteration of its breadth, or its breadth and length, the displacement be made equal to 800 tons, we may assume that the dead-water resistance will continually alter, approximately as the vessel's displacement, as long as its shape or "type", its draught and speed, and the water-layers are unaltered. The whole reduction is, in consequence, made by increasing the maximum dead-water resistance in the same ratio as the weight of the boat, and the depth of the fresh-water layer in the same ratio as its mean draught. The table below gives the numbers of the preceding table, reduced in this way.

Depth of fresh-water (spec. gravity of salt-water 1·030)	Maximum dead-water resistance of vessels of 800 tons weight and 4·4 m. mean draught		
	Type 1 Round-bottomed	Type 3 Flat-bottomed	Type 4 Sharp-bottomed
1 m.	0·30 tons		
1·9 —			1·18 tons
3 —	1·35 —	1·16 tons	
3·15 —			1·47 —
4 —	1·84 —		
5 —	1·75 —	1·63 —	
5·7 —			1·23 —
7 —	1·30		
9 —	1·00 —	1·19 —	
13 —	0·60 —		

The results contained in this table, are represented by the heavy curves (1), (3) and (4) in Fig. 3 Pl. XI, which give the maximum dead-water resistance[1] as functions of the depth of the fresh-water layer. The faint curve (2) shows approximately the velocity at which, according to the experiments, the dead-water resistance would be a maximum (when the total depth of the water is 37 m.); the velocities are plotted vertically in m. per second and the depths of the fresh-water layer horizontally, on the same scale as in the case of the other curves. [If the depth of the salt-water were infinite, and if the waves created were very low, Curve (2) should theoretically have been a parabola].

[1] In the figure stands "wave-resistance", which is not quite correct.

The curves (1), (3) and (4) show that there is a marked, although not very essential difference in the dead-water resistance, between vessels of such different types. The sharp-bottomed boat (with *triangular cross-section*) experiences comparatively *greater resistance* than the round-bottomed one, *if the surface-layer be thin, and smaller resistance if the surface-layer be thick.* In the case of the flat-bottomed boat (with *rectangular cross-section*), it is *vice versâ.* The reason is apparent: if the vessel reaches below the fresh-water layer, that part of its body which moves in salt-water does not contribute to the wave-making and to the dead-water resistance, but would rather have the contrary effect. Other circumstances being equal, that vessel which has comparatively the greatest part of its body in the fresh-water layer, will therefore experience the greatest resistance. On the other hand, as long as the vessel moves entirely in the fresh-water layer, it will generate more waves, and consequently experience more resistance, the nearer its whole body is to the fresh-water salt-water boundary where the waves are generated.

The influence of the ratio between length, breadth and draught of the vessel, was not examined, as it seemed likely that it would have brought the investigation beyond reasonable limits. This omission is however of no importance, as long as the dimensions of the vessel considered, are in approximately the same ratio as in one of the models 1—4.

The above results do not confirm the experience of seamen that a sharp-built fast-sailing vessel should be worse in dead-water than one of bluff design (Accounts Nos. 3, 7, and 9, Chap. I). But as a matter of fact, the contrary is stated in Account No. 8. The more general experience may perhaps be explained simply by the circumstance that the finely constructed vessel had a relatively smaller propelling force compared to its displacement.

The force necessary to free a vessel from dead-water under given circumstances, may, according to the theory given above, be approximately calculated as follows.

1) In accordance with the particular shape of the midship-section of the vessel, select one of the resistance-curves (1), (3), (4) Fig. 3 Pl. XI, or if necessary, draw a new curve by interpolation.

2) If the mean draught of the vessel is 4·4 m., the depth of the fresh (or brackish) surface-layer may be sought out along the horizontal axis in Fig. 3 Pl. XI. If her mean draught is D metres, the depth of the surface-

layer is first to be divided by D and multiplied by 4·4. The corresponding velocity given by curve (2), and the resistance given by the selected resistance-curve, are noted.

3) The last-mentioned quantity is the maximum dead-water resistance, if the vessel has a weight of 800 tons, and if the difference of spec. gravity between the surface-layer and the sea-water below, is 0·030. If these quantities are w tons and d respectively, the resistance given by the selected curve must be multiplied by $w/800$ and by $d/0{\cdot}03$, *i. e.* by $wd/24$.

4) In the case of a *gradual* transition from fresh surface-water into salt sea-water, the diagrams in Fig. 2 Pl. XI give an indication of the extent to which the resistance found in (3), must be diminished on account of this circumstance.

5) Multiply the velocity found in (2), by $\sqrt{D/4{\cdot}4}$ and by $\sqrt{d/0{\cdot}03}$; the result is approximately the velocity corresponding to the above (1—4) calculated maximum dead-water resistance. Add to the latter the resistance to the vessel in *homogeneous* water at this velocity; the sum then gives approximately the maximum resistance in the given water-layers. If the propelling force of the vessel is greater, she will not under ordinary circumstances fall into dead-water; and if she accidentally does so, she will be able to get free again by stopping completely for a while, and by then going full speed ahead again. If once in dead-water, and if this manoeuvre be not carried out, the vessel might, however (as was mentioned on p. 110) be kept in the dead-water in spite of a considerably greater propelling force.

Application of the experimental results to the case of the "Fram" at Taimur.

The only case of dead-water from which we have definite and reliable statements about the water-layers as well as of the vessel's speed and force of propulsion, is that which happened to the *Fram* west off Taimur (see Chap. I pp. 9—11). It is therefore of interest to see whether the experimental results are in accordance with these statements or not.

The force of propulsion of the ship, may be roughly calculated from the indicated H. P. of her engine. From the "Akers mekaniske Verksted", where the ship was built, I was informed that the engine with a speed of 6 knots (3·1 m. per second) developed 210 I. H. P. Of this work only a part

is available for propelling the ship, the ratio between the useful work of the propeller and the indicated work of the engine being generally called the "propulsive coefficient". For a single-screw ship with thick wood stern- and rudder-posts, the propulsive coefficient is estimated at about 40 per cent[1]. The useful work should then be 84 H. P. = 6 300 kgr. m. per second, when the ship had a speed of 3·1 m. per second; which gives a force of propulsion = 6 300/3·1 = 2 032 kilogrammes. This value however, when compared with the results of FROUDE's famous and excellent towing-experiments with H. M. S. *Greyhound*[2], seems rather too high. The dimensions of the *Greyhound* compared with those of the *Fram* are:

	Greyhound	*Fram*	Ratio
Length.	48·8 m.	36·25 m.	1·35
Extreme breadth . . .	10·1 m.	11·0 m.	0·92
Mean draught	4·2 m.	4·5 m.	0·93
Displacement.	1180 m.^{3}	800 m.^{3}	1·48
Wetted surface	700 m.^{2}	450 m.^{2}	1·55

The resistance of the *Greyhound*, when towed at a rate of 6 knots, was 1·4 english tons or 1·42 meter-tons. Supposing that the resistances of the two ships are to each other as the areas of their wetted surfaces, the resistance of the *Fram* at the same speed should be *0·92 ton*, only. This supposition is certainly approximately true; for at such slow speeds, by far the greatest part of the resistance is due to friction against the ship's hull (see White's "manual"). It is true that the *Fram* was proportionately shorter than the *Greyhound*, and as FROUDE has proved, this circumstance slightly increases the frictional resistance per unit of wetted area. But on the other hand, the dimensions of the boats, and their displacements, show that the *Greyhound* cannot have had such fine lines as the *Fram*. Finally, the *Fram's* resistance *i. e.* her force of propulsion at the speed considered, has probably not been more than *1 ton* at most. This corresponds to a propulsive coefficient of 20 per cent, which is not improbable, as the *Fram's* screw was made extraordinarily thick to stand the shocks of the ice, and its effect was hindered by very

[1] See WHITE, l. c. p. 33, p. 543.
[2] WHITE, l. c., p. 475–476.

big stern-posts. The screw is able to develop a somewhat greater force, when the vessel has her speed diminished by a hindrance, but I cannot from the information which I have been able to get, say to what extent.

The next question concerns *the depth and spec. gravity of the water-layers.* Exact measurements of the water-layers were not made. But it is recorded that the water at the surface was almost fresh, whereas through the bottom-cock of the engine-room perfectly salt water was obtained. The specific gravity of the surface-water was measured. Its greatest values while the *Fram* was in dead-water on Aug. 29th was nearly 1·008, and it was 1·005 on the two other days[1]. To judge from the measurements on other occasions, the water at 5—10 metres' depth has probably had a specific gravity of 1·023 or 1·024. The difference of spec. gravity between the surface-layer and the sea-water below should then have been as much as 0·016 during the whole time on Aug. 29th, and 0·019 on the other days. Prof. NANSEN suggests that the water-layers might have been analogous to those measured on July 11th 1894 (l. c. pp. 249—250), and that the light surface-layer might have had a thickness of about 2·5 m. The densities *in situ* were according to the measurements taken on the day: at 0·05 m. 1·001, at 0·12 m. 1·001, at 1 m. 1·001, at 2 m. 1·002, at 3 m. 1·023, at 5 m. 1·024, and at 10 m. 1·024. These measurements made in an analogous case, prove that the boundary between the two water-layers might well have been rather sharp.

There was only one direct observation on the thickness of the surface-layer: *the salt-water was on a level with the bottom-cock.* This was 3·5 m. below the constructed water-line, and as the ship lay at the time perhaps half a metre deeper, it would seem that the fresher surface-layer in any case cannot have been *more* than 4 m. thick. It must however be remembered that *the water was taken through the bottom-cock, while the ship was in motion, and consequently there were waves in the boundary between the salt and fresh water.* The bottom-cock is situated 5 or 6 m. from the stern; and the photographs on Pl. XVI—XVII show that the salt-water owing to wave-motion may have been there raised probably one metre or more above its mean level. *The surface-layer might consequently have been as much as 5 m. thick or even a little more.*

[1] See FRIDTJOF NANSEN, On the Oceanography of the North Polar Basin (Vol. III of these reports) pp. 158—159.

To calculate the greatest resistance by which the *Fram* could have been held fast in the dead-water, let us assume first, that the fresh surface-layer was 2·5 m. thick. The *Fram's* draught at that time, was 5 m. or more, say 5·1 or 5·2 m. aft. Her mean draught was then probably 4·9 m. According to the rules given on pp. 132—133, we have therefore in Fig. 3 Pl. XI to take $2{\cdot}5\,\frac{4{\cdot}4}{4{\cdot}9} = 2{\cdot}25$ on the horizontal scale. The corresponding resistance given by curve (1), is 0·87 ton, and curve (2) gives a velocity of 0·8 m. per second. The difference of specific gravity between the water-layers is assumed to be 0·016. The displacement of the *Fram* with 4·9 m. mean draught is about 950 tons. The maximum dead-water resistance would consequently be

$$0{\cdot}87 \times \frac{950 \times 0{\cdot}016}{24} = 0{\cdot}55 \text{ ton,}$$

and the corresponding velocity

$$0{\cdot}8 \sqrt{\frac{16}{30}\,\frac{4{\cdot}9}{4{\cdot}4}} = 0{\cdot}6 \text{ m. per second}$$

or 1·2 knots. As the *Fram's* speed, when not in dead-water, was at that time 4·5 knots with the full force of the propeller (1 ton), the resistance at 1·2 knots in homogeneous water may have been $(1{\cdot}2/4{\cdot}5)^2 = 0{\cdot}07$ ton. The whole resistance should then be $0{\cdot}55 + 0{\cdot}07 =$ *0·62 ton* only, and the ship would consequently not be held in dead-water.

As mentioned above, the depth of the surface-layer *might*, however, have been as much as 5 m. or even a little more. Suppose it was 4·5 m.; which would give nearly the greatest possible resistance. In Fig. 3, Pl. XI, we then have to take $4{\cdot}5\,\frac{4{\cdot}4}{4{\cdot}9} = 4{\cdot}04$, on the horizontal scale; the corresponding resistance given by curve (1), is 1·85 ton, and the velocity given by curve (2), is 0·85 m. per second. Multiplying by the same factors as above, we obtain a maximum dead-water resistance of 1·17 ton at a velocity of 1·3 knots. The resistance in homogeneous water is at that speed 0·08 ton, and the whole maximum resistance would consequently be about *1·25 ton*, which is more than sufficient to hold the *Fram* in dead-water. It may be remembered, that this resistance 1·25 ton, denotes the propelling force necessary to free the vessel from the dead-water, if the engine be stopped and after a while given full speed again; while a still greater force would be necessary, if this manoeuvre be not carried out.

While in dead-water on Aug. 30th and Sept. 2nd—3rd, it is possible that the difference of spec. gravity between the water-layers never fell below 0·019, and the maximum-resistance might then have been as much as $\frac{19}{16} \times 1{\cdot}25 = 1{\cdot}5$ ton. On Aug. 29th and 30th, the boiler required cleaning, and consequently the force of propulsion was somewhat reduced. These circumstances might perhaps be counter-balanced by the fact that the force of the propeller is greater at the low speeds considered, than at full speed; although I am not able to say *how much* influence the speed may have had on the force of propulsion.

The velocity, at which the resistance was a maximum, should according to the above calculation have been 1·2 or 1·3 knots, and this should consequently be an upper limit for the actual velocity. This calculation is based upon experiments in the comparatively narrow tank, while in open water the velocities become somewhat greater (see Fig. 9, Pl. VI). It is, however, highly improbable, that the vessel had the velocity corresponding to the maximum resistance, because then she might at a favourable moment have got rid of the dead-water. From a comparison of Curves 2 and 3 in Fig. 9 Pl. VI, it follows, that *the Fram's speed when in dead-water, was very probably between 1·2 and 1·5 knots; which agrees remarkably well with the speeds alleged by Prof. Nansen.*

The above calculations prove that under suppositions consistent with the actual measurements of the water-layers, the observed effects of dead-water may be explained from the experimental results, even as far as absolute quantities are concerned. But they also show another point of interest. The excess of the maximum-resistance above the force of propulsion, is rather moderate, even assuming the former to have been the greatest possible allowed by the observations made at the time; probably this excess has been very small. When the *Fram* was steadily struggling and steaming ahead, the power of the dead-water may have grown considerable; but it is very possible that if she had stopped and after a while, had suddenly made full speed, she would after a few such spurts, have succeeded in getting entirely free (see p. 110). The *Fram* had a triple-engine, which could be shifted into compound; and then for a short time, it was possible with great waste of

steam, to raise her speed by two or three knots. *By wasting steam in this way for only a few minutes, the Fram would almost certainly have got rid of the dead-water; and this accomplished, the engine might then have been shifted into triple again. In this way she would with considerable saving of coal have reached her harbour in a small fraction of the time which actually was spent.*

APPENDIX.

NOTE ON THE MATHEMATICAL TREATMENT OF THE WAVES AND THE WAVE-MAKING RESISTANCE.

Keeping to suitably simplified cases, it is possible to calculate mathematically the wave-making resistance at different velocities, in shallow water as well as in "dead-water". The results of these calculations as far as worked out, agree remarkably well with the experimental measurements; which is not always the case with exact solutions of hydrodynamical problems. Such agreement seems indeed to be rather usual in the case of wave- and other oscillatory motions, presumably because these motions are *stable*. Below, some of the results will be given and compared with the experimental results, but the method of calculation will be only indicated; a fuller account of the method and its application to some other problems, will be given on a subsequent occasion.

In No. III of a series of papers: "On stationary waves in flowing water"[1], Lord KELVIN has solved a problem which in a way represents the case of a vessel moving in a shallow and narrow channel. The channel is supposed to be of rectangular section, and the vessel is replaced by a small ridge on the bottom, right across the channel. The ridge is supposed to be very low, and with smooth and even slopes.

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[1] (Sir William Thomson) Phil. Mag. Ser. 5 Vol. 22–23 (1886–87). No. III is found in Vol. 22 p. 517. A few errors which influence, although not essentially, the results of this paper, may perhaps be noted. The factor 2 on the right hand side of (26) p. 523, should be omitted, and the two last members in (30) p. 524, as well as the right hand side of (31), (34), and (40), should in consequence be doubled. In (47) and (48) p. 528, the denominators $1 + D/b$ should be replaced by $1 - D/b$.

Under these circumstances, and if KELVIN's notation be slightly altered, the height h of the water-surface above its mean level, is expressed by the integral

$$h = \frac{A}{\pi D} \int_0^\infty \frac{\cos\left(\frac{x}{D}\sigma\right)}{\varphi(\sigma)} d\sigma , \tag{1}$$

where

$$\varphi(\sigma) = \frac{e^\sigma + e^{-\sigma}}{2} - \frac{1}{\sigma s^2} \frac{e^\sigma - e^{-\sigma}}{2} \tag{2}$$

and where

A = the cross-section of the ridge producing the waves,

D = the depth of the channel;

x = the horizontal distance from the ridge to the point in the water-surface under consideration, counted positively in the direction opposite to the motion of the ridge relative to the water; and

s = the ratio between the velocity of the ridge through the water (or of the water relative to the immovable ridge) and the maximum velocity $\sqrt{gD}$ of waves in the channel.

To calculate the integral (1), Lord KELVIN breaks up the function $\frac{1}{\varphi(\sigma)}$ into partial fractions which put its singularities in evidence, and h is then obtained as a series of exponential functions. When $s > 1$ all the singularities are imaginary values of σ, and in this case the expression of the level-disturbance (1) is infinitesimal at some distance from the ridge, and represents a constant quantity of energy. There is then *no* wave-making resistance. When $s < 1$, $\frac{1}{\varphi(\sigma)}$ is infinite for *one* positive value of σ, and to this value corresponds a term in (1) representing a series of harmonic waves, and which may be calculated by help of an indeterminate definite integral evaluated by CAUCHY, namely

$$\int_0^\infty \frac{\cos px}{q^2 - x^2} dx \begin{cases} = +\frac{\pi}{2q} \sin pq & \text{for } p > 0 \\ = -\frac{\pi}{2q} \sin pq & \text{for } p < 0 \end{cases} \tag{3}$$

When the sole object is to determine the resistance, the calculation may be made much simpler, since we can leave out of account any term in the expression for h of the form

$$\int_0^\infty \psi(\sigma) \cos\left(\frac{x}{D}\sigma\right) d\sigma,$$

in which $\psi(\sigma)$ is a uniform analytic function of σ, regular for all values of $\sigma \geqq 0$, and when

$$\int_0^\infty |\psi'(\sigma)| \, d\sigma$$

is finite; $|\psi'(\sigma)|$ being the modulus or "*absoluter Betrag*" of $\psi'(\sigma)$. By partial integration it is easily seen that such a term decreases infinitely with increasing x, and that it in consequence represents a local disturbance in the neighbourhood of the ridge, which causes no resistance.

The right hand member of (2) is a holomorphic function for all finite values of σ; it has no zero for positive values of σ if $s > 1$, and when $s < 1$ a simple zero for *one* positive value σ_1, only. It is then easily seen that in the former case $\frac{1}{\varphi(\sigma)}$ satisfies the above conditions for $\psi(\sigma)$, so that (1) gives no wave-making resistance. When $s < 1$ we may write (1) in the form

$$h = \frac{A}{\pi D} \int_0^\infty \frac{2\sigma_1 \cos\left(\frac{x}{D}\sigma\right)}{(\sigma^2 - \sigma_1^2)\varphi'(\sigma_1)} d\sigma + \frac{A}{\pi D} \int_0^\infty \psi(\sigma) \cos\left(\frac{x}{D}\sigma\right) d\sigma \tag{4}$$

$$\psi(\sigma) = \frac{1}{\varphi(\sigma)} - \frac{2\sigma_1}{(\sigma^2 - \sigma_1^2)\varphi'(\sigma_1)},$$

where $\varphi'(\sigma)$ is the first derivative of $\varphi(\sigma)$.

The function $\psi(\sigma)$ obviously satisfies the above conditions for the second integral in (4) to make no contribution to the resistance, and this integral is therefore left out. As a result of (3), the first integral in (4) gives

$$h = -\frac{A}{D\varphi'(\sigma_1)} \sin\left(\frac{x}{D}\sigma_1\right) \qquad \text{for } x > 0$$

$$h = +\frac{A}{D\varphi'(\sigma_1)} \sin\left(\frac{x}{D}\sigma_1\right) \qquad \text{for } x < 0.$$

When an endless series of waves which annul the waves for $x < 0$ are superposed, so that there are no waves ahead of the ridge, those *behind* the ridge become doubled, and we obtain for x positive,

$$h = -\frac{2A}{D\varphi'(\sigma_1)} \sin\left(\sigma_1 \frac{x}{D}\right).$$

From this expression, which is identical with the final result in Lord KELVIN's paper, the wave-making resistance R is easily found in the way indicated on p. 37. The wave-energy per unit area of the water-surface is

$$E = \tfrac{1}{2} g q H^2,$$

q being the density of the water and H the amplitude of the waves. The wave-making resistance is

$$R = B(1-r)E,$$

where B is the breadth of the ridge, *i. e.* of the channel, and r is the ratio of transmission of wave-energy (see p. 36), found by means of a well-known formula of Lord RAYLEIGH (see for instance Lamb, Hydrodynamics art. 221). From this, it follows

$$R = \frac{gqBA^2}{D^2} \frac{1 - \dfrac{4\sigma_1}{e^{2\sigma_1} - e^{-2\sigma_1}}}{\left(\dfrac{e^{\sigma_1} - e^{-\sigma_1}}{2}\right)^2 \left(1 + \dfrac{1}{\sigma_1^2 s^2} - \dfrac{1}{\sigma_1^2 s^4}\right)^2}. \tag{5}$$

The broken curve in Fig. 2 Pl. VI represents the resistance calculated in this way, as a function of s, that is of v/v_m. The unit of resistance is chosen arbitrarily. It is seen that the resistance is inappreciable as long as v is less than $\tfrac{1}{2} v_m$; it increases with v and is a maximum when $v = v_m$, at higher velocities it is, as before mentioned, *nul.*

In a similar way the waves and the resistance in the case of dead-water, may be calculated. The boat may be imagined to be replaced by a small ridge stretching in the upper surface, right across the channel and moving along with a velocity v. The disturbances of the salt-water fresh-water boundary (reckoned *downwards* from its equilibrium level) are then given by the same formula (1), where however the function $\varphi(\sigma)$ is somewhat different. If the two water-layers are equally deep, φ is found to be given by (2) as before, s being the ratio of the velocity of the ridge to the maximum velocity of the boundary-waves (3, p. 43)[1]. If the salt-water is by comparison infi-

[1] The fact that the laws are the same in homogeneous shallow water and in the boundary between two different water-layers of equal depths, gives an interesting and convenient method of studying wave-motion on a small scale, since the boundary-waves can be made to move at a conveniently slow speed and — even when of small dimensions — without the disturbing effect of capillarity.

nitely deep (and if the difference of density Δq be regarded as infinitesimal) φ is, with the same notation as before,

$$\varphi(\sigma) = e^{\sigma} - \frac{1}{\sigma s^2} \frac{e^{\sigma} - e^{-\sigma}}{2}. \tag{6}$$

The function φ is a holomorphic function for all finite values of σ, with no zero for positive values of σ if $s > 1$ and with only one simple zero $\sigma = \sigma_1$ if $s < 1$. Just as in the case of the former problem we then obtain *no* waves if $s > 1$; and

$$h = -\frac{2A}{D\varphi'(\sigma_1)} \sin\left(\sigma_1 \frac{x}{D}\right) \quad \text{for positive values of } x,$$

if $s < 1$, and if there is supposed to be no wave-motion ahead of the ridge. The resistance R is then calculated in exactly the same way as before, and

$$R = \frac{g\Delta q B A^2}{D^2} \cdot \frac{\sigma_1^2}{e^{2\sigma_1} - \frac{1}{s^2}}.$$

This quantity is represented by the broken curve in Fig. 3 Pl. VI in the same way as (5) was represented in Fig. 2 Pl. VI. In the present case it will be seen that the resistance is a maximum at *below* the maximum wave-velocity (for $v/v_m = 0{\cdot}77$ about). When the vessel's velocity is further increased to v_m, the resistance decreases to nul.

In Figs. 2 and 3, Pl. VI the theoretical curves are drawn for comparison alongside experimental ones, the latter representing as nearly as possible the resistance due to wave-making. As the waves are assumed in the theoretical calculations, to be created by a ridge stretching across the channel, while a boat-model was used in the experiments, there can be no question of comparing the absolute value of the resistance according to experiments as well as calculation. The scale of resistance is therefore simply chosen so that the highest points of the experimental and of the theoretical curves shall be at the same level. The scale of velocities is the same for both curves.

The heavy full-drawn curve in Fig. 2 Pl. VI represents the wave-making resistance in a shallow canal, according to SCOTT RUSSELL. For this purpose the resistance given by Curve E in Fig. 4 p. 38 is diminished by a quantity increasing as the square of the velocity, and which is at velocities above 8 miles an hour, approximately equal to the total resistance given by E; the rest is assumed to be the wave-making resistance and is represented as above mentioned, in

Fig. 2 Pl. VI. The maximum wave-velocity v_m is, according to the data in SCOTT RUSSELL's paper, assumed to have been 8 miles an hour. The faint curve in the same figure represents on the same scale the quantity subtracted.

The two full-drawn curves in Fig. 3 Pl. VI represent the dead-water resistance according to Fig. 1 Pl. X — the heavy curve in the case of a 5 cm. fresh-water layer, and the faint curve in the case of a 9 cm. surface-layer. Strictly speaking, the wave-making resistance and not the dead-water resistance, should be compared with the calculated wave-making resistance. The difference between these two quantities could, however, be determined only for very small velocities and not for velocities at which the wave-motion is considerable. In any case, the difference is not *very* great.

It is obvious that the agreement between the calculated and the experimental resistance-curves *could* not be complete: because in the experiments the boat also created *diverging* waves, which persisted and caused resistance, even at velocities higher than the maximum wave-velocity; and part of the dead-water resistance was also due to friction. Finally, owing to the oscillations in the velocity, (see pp. 67 seq.) the resistance measured, was not the resistance at steady speed. The agreement between the experimental and theoretical curves in Figs. 2 and 3 Pl. VI may under these circumstances be regarded as surprisingly good. The peculiar shapes of the theoretical resistance-curves in homogeneous shallow water and in "dead-water" are, indeed, easily recognized on the experimental curves, without any explanation. It is worthy of special notice that the ratio between the velocity at which the dead-water resistance was a maximum, and the maximum wave-velocity, should according to theory be about 0·77 while the experiments gave practically the same value namely 0·73 (see p. 123).

The case in which the vessel moves in open water instead of in a narrow channel has also been investigated in the same way and the waves affecting the resistance evaluated in finite functions. In this calculation the vessel has been replaced by a small reversed cupola put in the water-surface; the wave-height was found to decrease inversely as the square root of the distance from the vessel. As the numerical calculations particularly of the *diverging* waves, have at present not been completely worked out, I must here confine myself, to one point of interest concerning the *transverse* boundary-waves. An approximate calculation of the resistance due to these latter was made, and the result-

ing resistance-curve was found to be very similar to the broken curve in Fig. 3, Pl. VI, except that the resistance is a maximum at a slightly higher velocity than in that case. This result agrees well, as far as the transverse waves are concerned, with the experimental results. Even the absolute height of the transverse waves indicated by the photographs on Pl. XVI, seem to agree in order of magnitude, with theory.

SUPPLEMENT.

Kapt. H. MEYER mentions in "Annalen der Hydrographie und Maritimen Meteorologie", Heft I, 1904, a case, of which the chief points are briefly cited below.

"In August 1874, in a fresh sea-breeze, we entered the *Congo* River with a brig drawing 3·8 m., speed about 5 knots. At the mouth, in the neighbourhood of Shark Point, there was a distinctly seen "rip" forming the boundary between the sea-water and the river-water; after having passed this rip, the ship suddenly ceased to answer her helm, and after oscillating for a time, about her course, laid herself right across the current. Manoeuvres with the sails were of no avail, although the wind was fresh. The river-water flowed past the vessel as if she were grounded. Gradually she drifted right across to the shore on the north side of the river and here we anchored. The ship then laid herself along the current; a double wake was observed, the two directions of which formed an angle of about two points. We endeavoured once more to sail with the fresh and favourable wind, but the ship neither made head-way nor answered her helm, and we therefore anchored again.

"The next day the current seemed to be somewhat weaker, the other circumstances the same as before. Again we tried to sail with the sea-breeze but with the same result as before; finally we were obliged to lie to.

"On the third day, a dutch pilot, who was acquinted with local conditions, helped us to reach *Banana* with the sea-breeze. He let the ship drift so near in shore that she must according to our opinion have had very little water under her. The current was there considerably weaker; we steered along the shore always in about the same depth of water, and continually with the usual double wake aft. At first, she did not answer her helm, but gradually she recovered her steering-power and finally steered quite well.

"A barque which lay in *Banana* when we came there, had a like experience.

"A month after, we left Banana and the ship again lost her steering in the neighbourhood of Shark Point; just as before, we drifted over to the other shore and were obliged to anchor. After having again tried without success to sail away from the coast with the sea-breeze, we tried at night with the land-breeze, and then succeeded."

The double wake mentioned by Kapt. MEYER, seems to prove that dead-water has been, in any case a contributive cause of the accident. Kapt. MEYER cites some other accounts of vessels which have lost their steering on the mouth of the *Congo* under similar conditions; although the double wake is not mentioned in any of them. One of these cases, when a vessel

in spite of a speed of 4 knots, did not answer her helm, proves that the surface- and under-currents alone, may sometimes be sufficient to deprive a vessel of her steering-power.

Kapt. J. FRÜCHTENICHT of the ship "Wilhelm" mentions in a report in "Ann. d. Hydr. etc.", 1881, p. 28, the following case:

"The water from the *Fraser River* sometimes spreads over the Georgia Strait and gives rise to so called dead-water. On June 20th 1880, the ship (draught 7 m.) was towed by a steamer and at this place she was held fast in spite of the full power of the tug. The depth was according to a sounding taken, more than 100 m. After an hour an attempt was made to set more sail and to fall farther from the wind, and we then gradually got up speed again. No different currents could be observed."

Mr. HAHN, Navigation Instructor in Leer, mentions in the same journal (Heft IV, 1904) that dead-water is often met with on the *Murman coast* where it is believed to depend on under-currents; and in *Baffins-Bay*, in the neighbourhood of *Labrador*.

EXPLANATION OF THE PLATES.

Pl. I. The apparatus (see pp. 54 seq.).

Fig. 1. Cross-section of the large tank. The water-level and the cross-sections of larger and the smaller *Fram*-models are indicated. The dotted lines denote the cross-section of the small tank.

Fig. 2. The large tank seen from the side, Scale 1 : 20.

Fig. 3. Shows the way in which the glass panes are fitted in the tank.

Fig. 4. The towing apparatus.

Fig. 5. Apparatus for registering velocity.

Fig. 6. Arrangement for producing suitable water-layers.

Fig. 7. Arrangement for cleaning the water-surface.

Fig. 8. The lines of the large *Fram*-model.

Fig. 9. The arrangement for observing the waves from the side. When taking photographs (Fig. 1, Pl. XII, and Pl. XIV—XVII) the oil-lamp *l* was replaced by a flash-lamp.

Fig. 10. The arrangement for taking the "relief-photographs" (Fig. 2, Pl. XII, and Figs. 1—3, Pl. XIII).

Pl. II.

Map of Scandinavia, illustrating the occurrence of dead-water according to seamen's accounts (p. 8).

Pl. III.

Maps illustrating certain accounts in Chap. I.

Pl. IV and Fig. 1 Pl. V.

Sketches illustrating the appearance of the dead-water.

Fig. 1. Sketch by Prof. NANSEN, showing the crest-lines of the waves which followed the *Fram* in dead-water (p. 11).

Figs. 2—5. Sketches by Mr. AANONSEN (p. 19).

Figs. 6—7. Sketches by Mr. G. A. LARSEN (p. 13).

Figs. 8—9. Sketches by Mr. ERIKSEN (p. 17). (The three "rips" aft of the vessel in Fig. 8, must obviously be imagined to stretch *in the water-surface* perpendicularly to the vessel's wake, although they are drawn vertical, in the plane of the paper).

Fig. 10. Sketch by Kommandörkaptein KROEPELIEN (p. 20).

Fig. 11. Sketch by Mr. COLIN ARCHER (p. 15).

Fig. 12. Sketch by Admiral SPARRE (p. 16).

Fig. 13. Sketch by Kaptein SCOTT-HANSEN (p. 11).

Fig. 14. Sketch made by the author, according to statements from Kommendörkapten SIDNER, and revised by the latter (p. 22).

Fig. 15. Sketch by Lieutenant WALLANDER (p. 23).

Fig. 1, Pl. V. Sketch by Kommandörkaptein KROEPELIEN, illustrating the same case of dead-water as Fig. 10, Pl. IV (p. 20).

Pl. V, Figs. 2—5.

Curves showing the shape of the crest-lines of dead-water waves according to calculation (p. 48). (The depth of the salt-water is in the calculation, assumed to be infinite compared to that of the fresh-water layer).

Pl. VI.

Fig. 1. (See pp. 43—44).

Figs. 2—3. Comparison between theory and experiment (pp. 143—144).

Fig. 4. Graphic representation of boundary-waves; the waves in the upper surface exaggerated (pp. 42—44).

Fig. 5. A solitary boundary-wave (p. 69).

Figs. 6—7. (To p. 46).

Fig. 8. Experimental resistance-curves with the small *Fram*-model (pp. 60—61).

Fig. 9. (To p. 127).

Pl. VII—VIII.

Curves representing the variation of velocity (plotted vertically in cm./sec.) with time (plotted horizontally), in some selected experiments (see pp. 67 seq.).

A short horizontal line at the left of a diagram represents the mean velocity as given in the tables. A short vertical line across the curve (in diagrams 54, 77, 80, 81, 84, 103, 105, 106) indicates the moment when one of the photographs in Pl. XV—XVII, was taken.

Pl. IX.

Figs. 1—2. Show the different shapes of the water-surface around a vessel; Fig. 1: at a low velocity, in dead-water; Fig. 2: at a higher velocity, without dead-water. The disturbance of the water-level exaggerated. (Pp. 66—67).

Fig. 3. Curves showing the disturbances of water-level, highly exaggerated (pp. 66 and 118 seq.).

Pl. X.

Fig. 1. Curves of resistance experienced by the larger *Fram*-model (1 : 100) at different velocities and in fresh-water layers of different depths on the top of salt water. The small circles at the top, indicate the theoretical maximum-velocity of waves in the respective water-layers. (Pp. 121 seq.).

Figs. 2—3. Similar curves for different boat-models (pp. 128 seq.).

Pl. XI.

Fig. 1. Resistance-curves showing the influence of the difference of spec. gravity between the water-layers (pp. 115 seq.).

Fig. 2. Resistance-curves in the case of several superposed strata of different densities (pp. 123—124).

Fig. 3. Curves for calculating the maximum dead-water resistance experienced by vessels of different shapes and in different kinds of water-layers (pp. 131—133).

Pl. XII.

Fig. 1. Photograph from the side, of the larger *Fram*-model in dead-water; Experiment 162, p. 82. The photograph is completed by a sketch of the upper part of the *Fram* and her tackle. (Pp. 64—65).

Fig. 2. "Relief-photograph" (taken as indicated by Fig. 10, Pl. I) of the waves in a case analogous to Fig. 1, and intended to make the latter clearer (pp. 64—65).

Figs. 3—4, Photographs illustrating the train of waves following a vessel under ordinary circumstances.

Pl. XIII.

Photographs of dead-water waves taken obliquely from below (pp. 62—63).

Fig. 1. Experiment 281, p. 86.

Fig. 2. Experiment 282.

Fig. 3. Experiment 283.

Pl. XIV.

Photographs from the side, of the small *Fram*-model in the small tank (pp. 62, and 69).

Figs. 1—6: in a 1·5 cm. thick (clear) fresh-water layer on the top of (black) salt-water of spec. gravity 1·020 and 1·028 respectively; towing forces 0·1—0·3 gr.

Figs. 7—8: in three water-layers, black and clear allernately, with very small differences of spec. gravity (p. 65).

Pl. XV.

Photographs from the side, of the larger *Fram*-model in a 3 cm. thick fresh-water layer; Experiments 53—56, p. 79. Salt-water black; fresh-water

lighter. The boat dragged by a force, in each case greater than in the preceeding. In Fig. 4 very little is wanting to free it from the dead-water. (Pp. 62 seq.).

Pl. XVI.

Photographs from the side, of the larger *Fram*-model in a 5 cm. thick fresh-water layer; Experiments 77, 80, 81, 84, p. 80. Salt-water black; fresh-water lighter. (Pp. 62 seq.).

Figs. 1—3: in dead-water, with a towing-force, increased in successive experiments.

Fig. 4: at a high speed, without dead-water.

Pl. XVII.

Photographs from the side, of the larger *Fram*-model in a 7 cm. thick fresh-water layer; Experiments 103, 105, 106, 108, pp. 80—81. Salt-water black; fresh-water lighter. (Pp. 62 seq.).

Figs. 1—3: in dead-water.

Fig. 4: at a high speed, without dead-water.

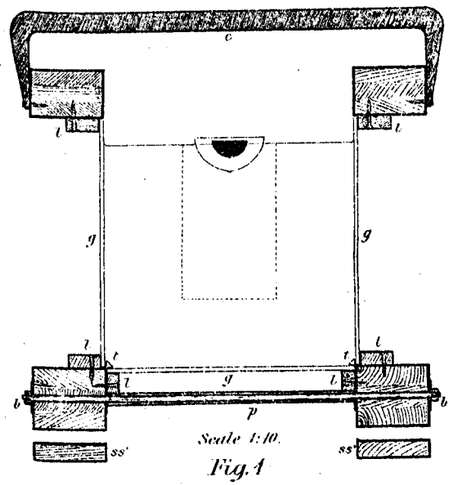

Fig. 1

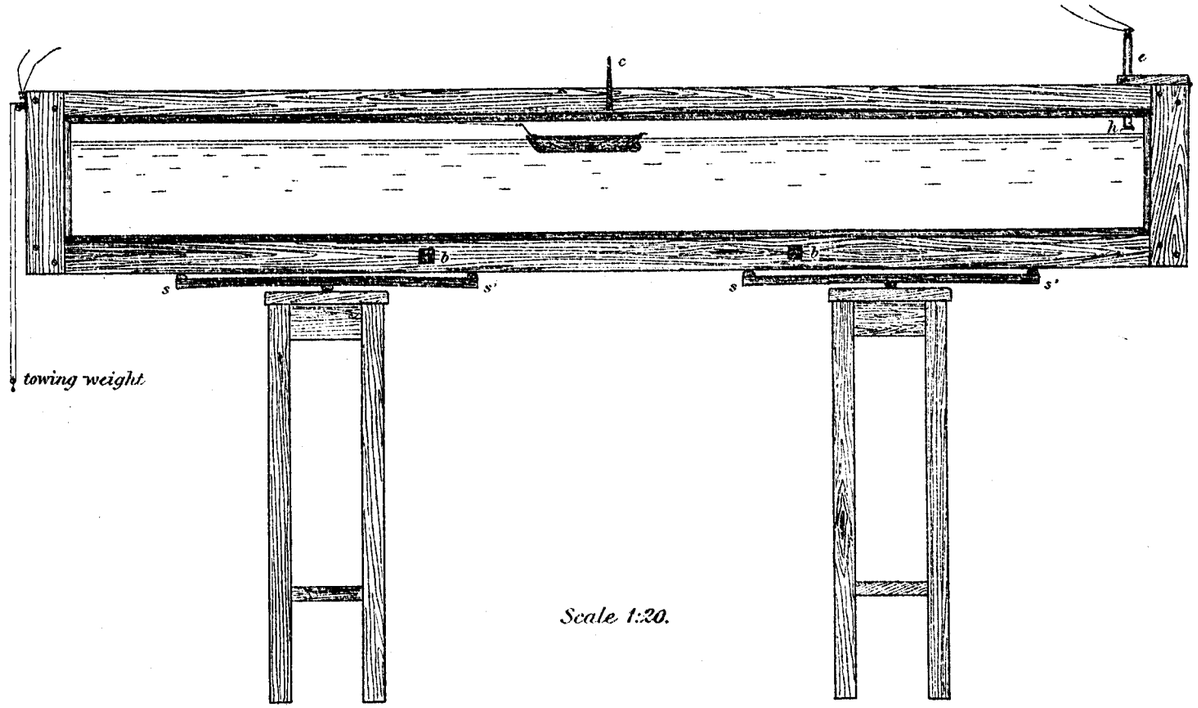

Fig. 2

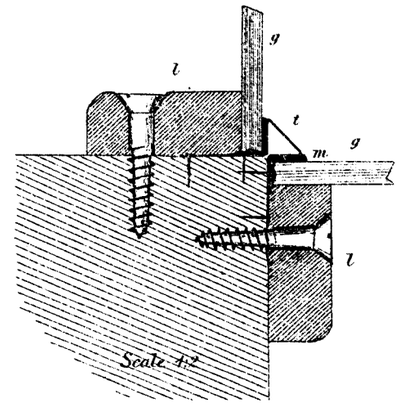

Fig. 3

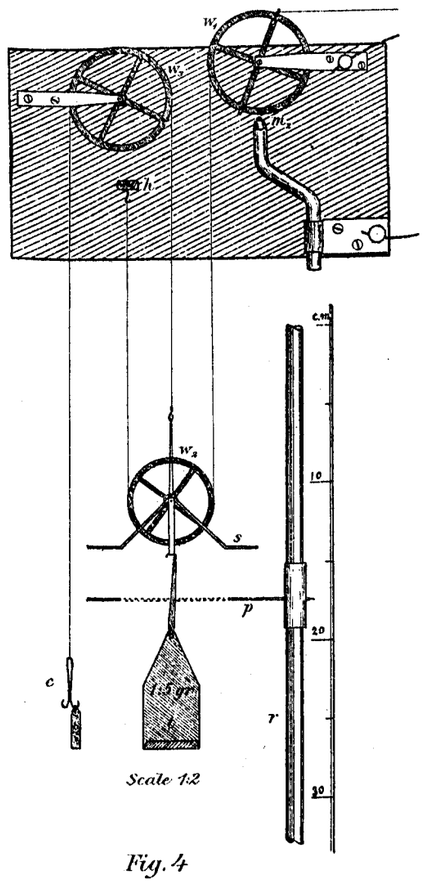

Fig. 4

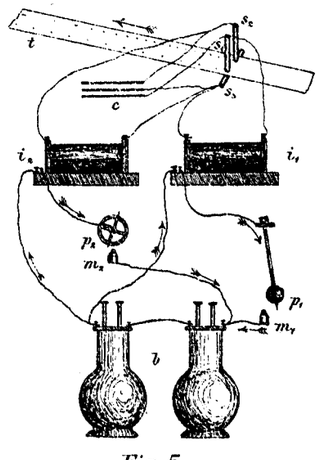

Fig. 5

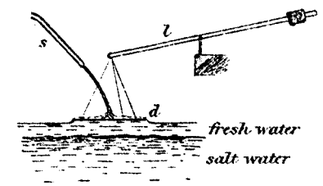

Fig. 6

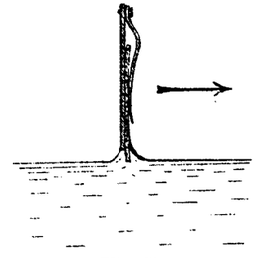

Fig. 7

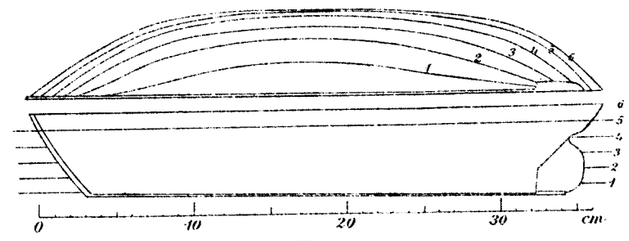

Fig. 8

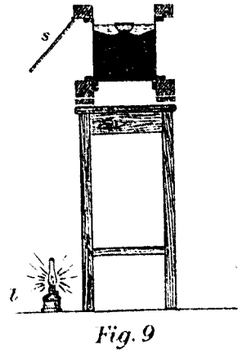

Fig. 9

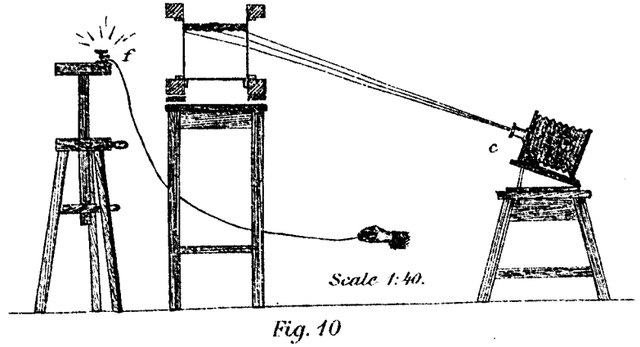

Fig. 10

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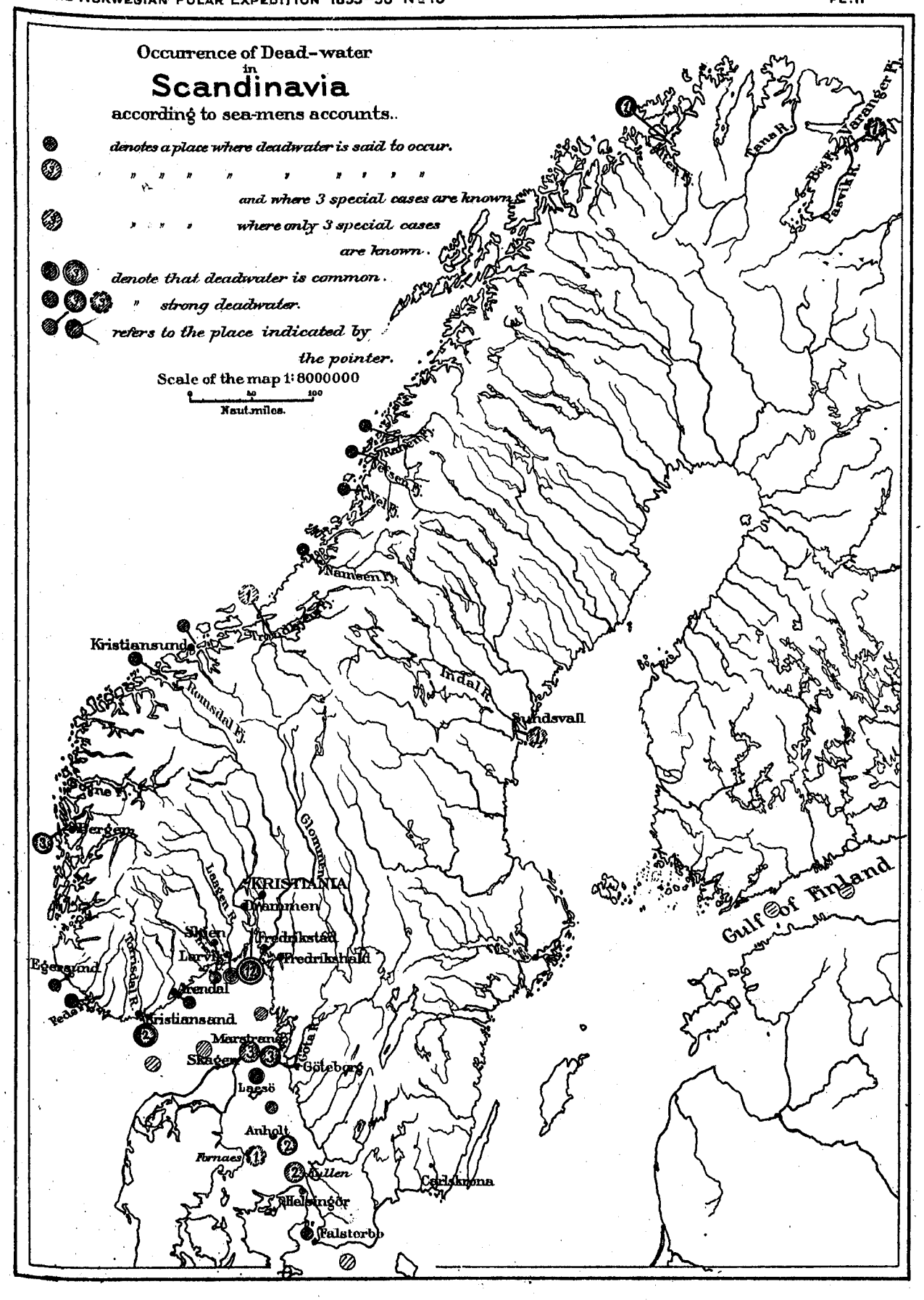

Occurrence of Dead-water
in
Scandinavia
according to sea-mens accounts.
denotes a place where deadwater is said to occur.
" " " " " " " " "
and where 3 special cases are known.
" " " where only 3 special cases are known.
denote that deadwater is common.
" strong deadwater.
refers to the place indicated by the pointer.
Scale of the map 1:8000000
0 50 100
Naut.miles.
Tana R.
Varanger Fj.
Pasvik R.
Rauen Fj.
Namsen Fj.
Kristiansund
Romsdal Fj.
Indal R.
Sundsvall
Bergen
Glommen
Laagen R.
KRISTIANIA
Drammen
Skien
Fredrikstad
Larvik
Fredrikshald
Egersund
Arendal
Kristiansand
Mandal R.
Marstrand
Skagen
Göta R.
Göteborg
Læsö
Anholt
Fornaes
Kullen
Helsingör
Falsterbo
Gulf of Finland

BERGEN
1 naut. mile.
KRISTIANSAND FJORD
1 naut. mile.
BÖG FJORD
1 naut. mile.
KRISTIANIA FJORD
Naut. miles.

Fig. 1

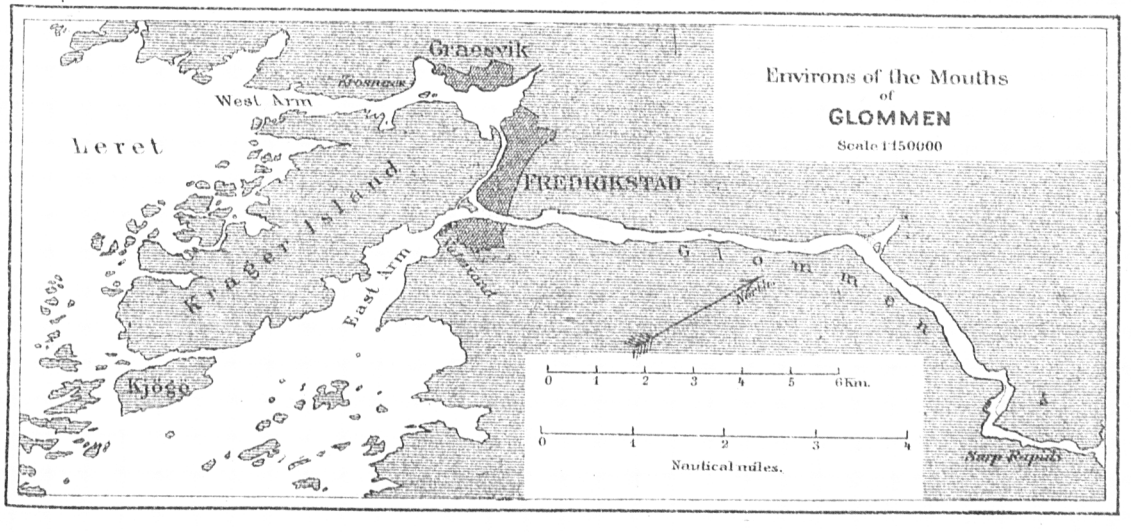


Fig. 2

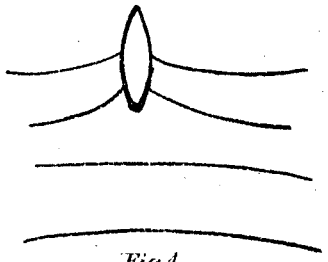


Fig. 1

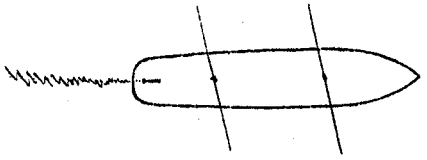


Fig. 2

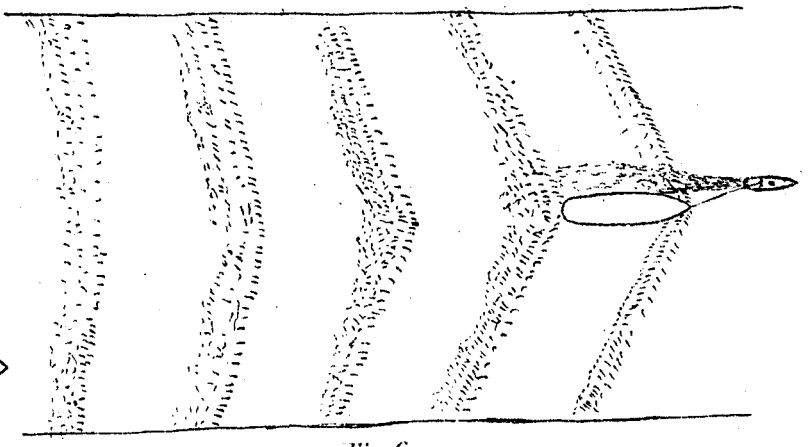


Fig. 6

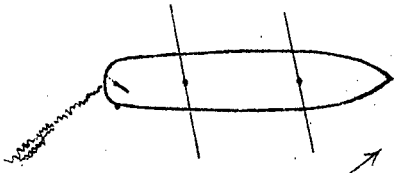


Fig. 3

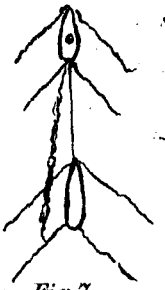


Fig. 7

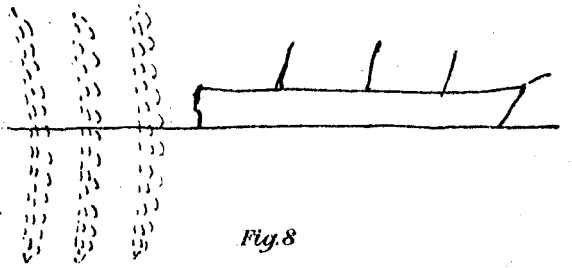


Fig. 8

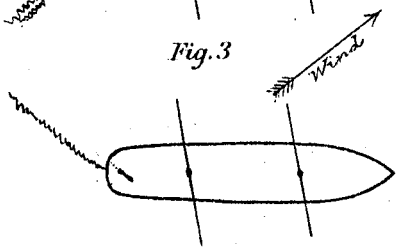


Fig. 4

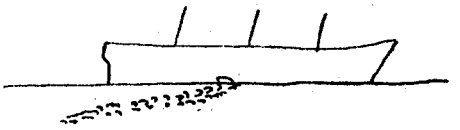


Fig. 9

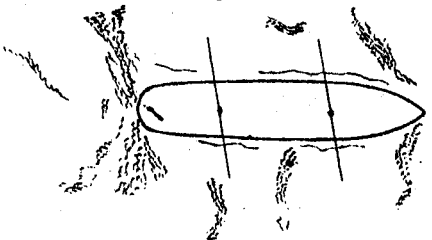


Fig. 5

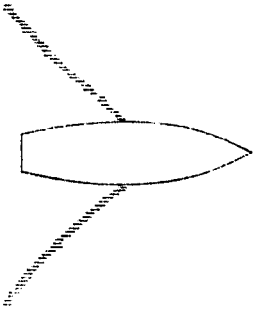


Fig. 11

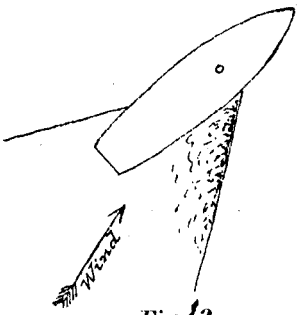


Fig. 12

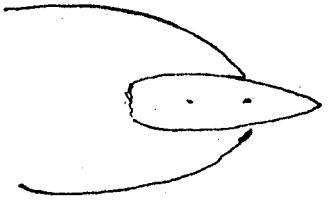
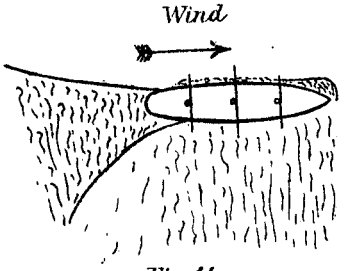


Fig. 10



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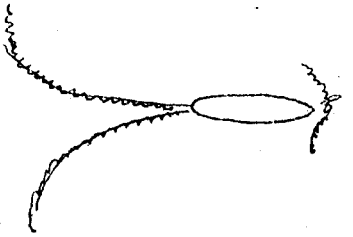

Fig. 15

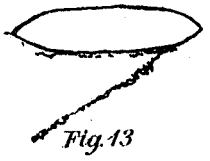

Fig. 13

Fig. 1

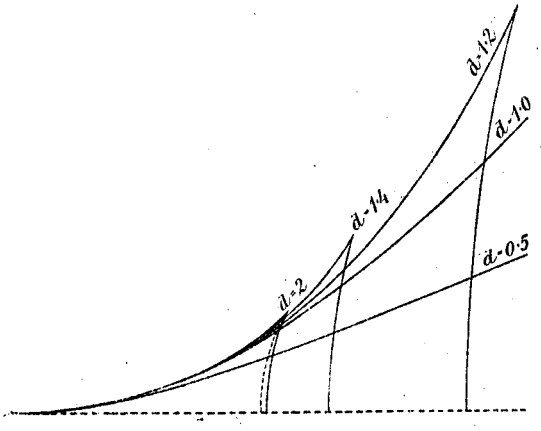

Fig. 2

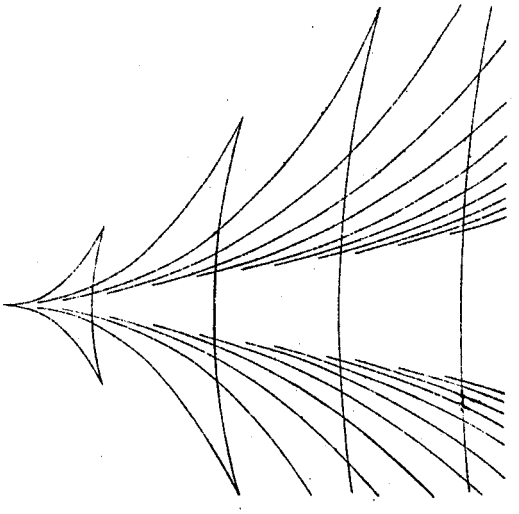

Fig. 4

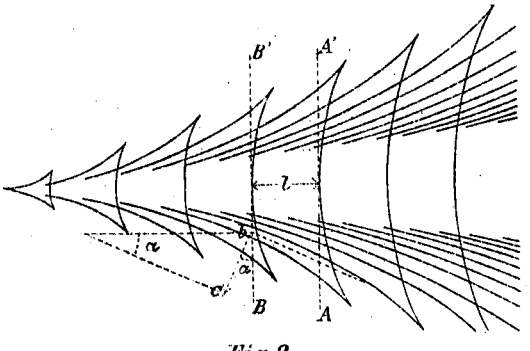

Fig. 3

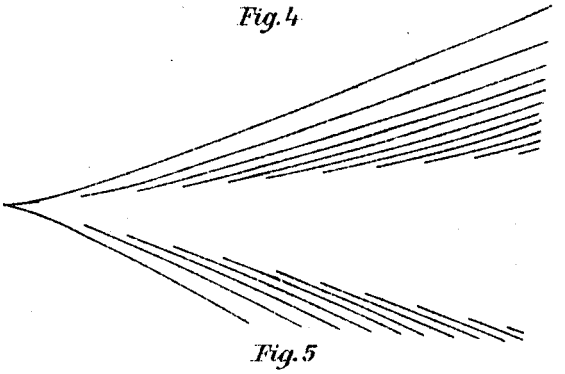

Fig. 5

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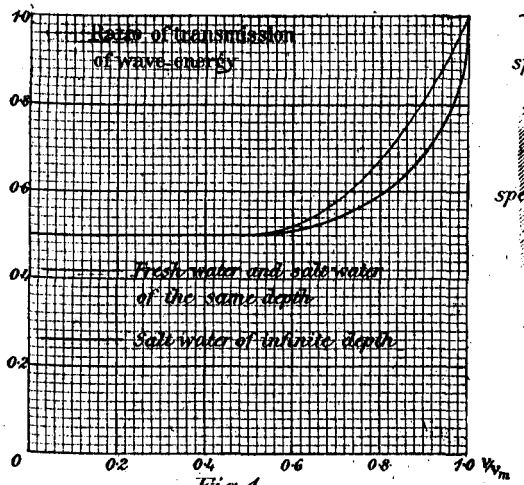


Fig. 1

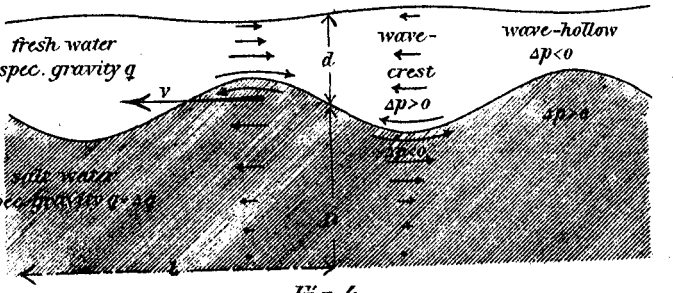


Fig. 4

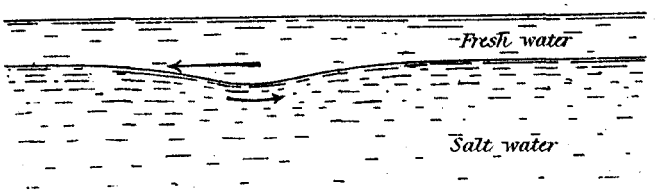


Fig. 5

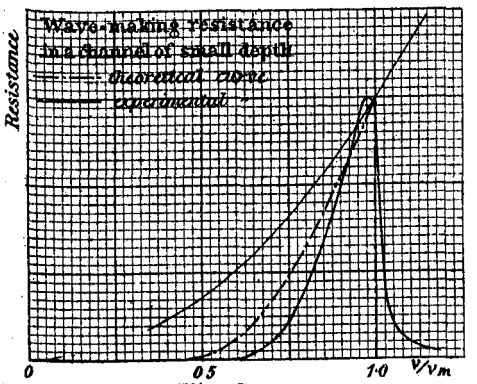


Fig. 2

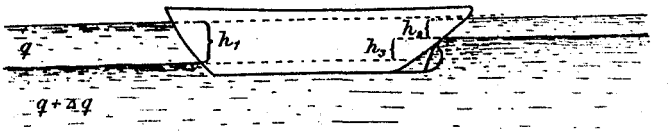


Fig. 6

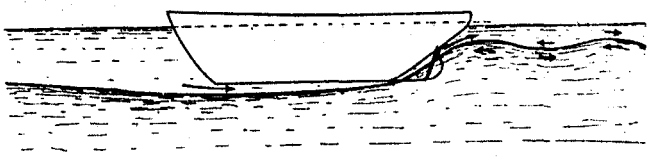


Fig. 7

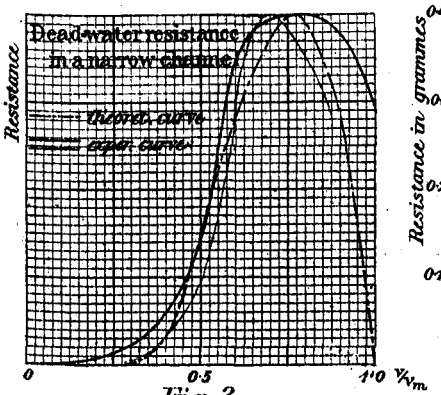


Fig. 3

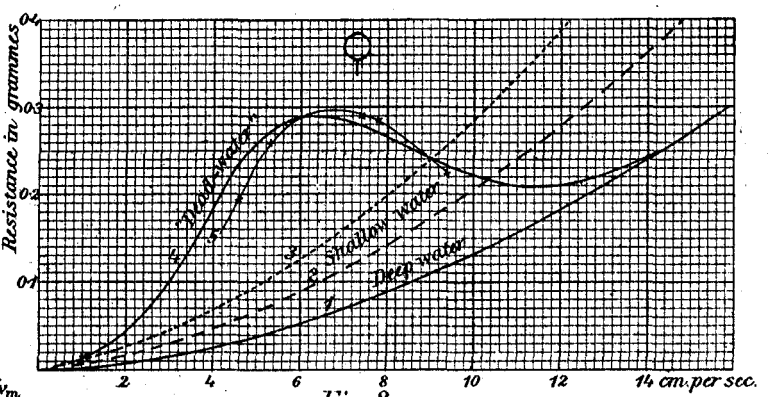


Fig. 8

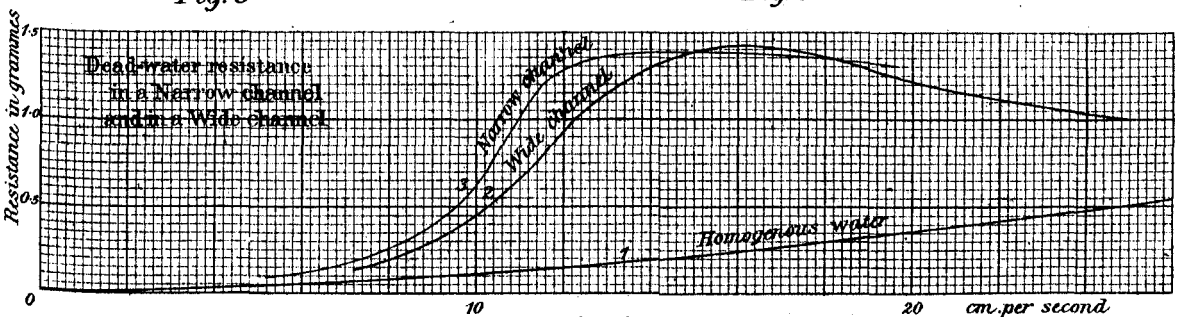
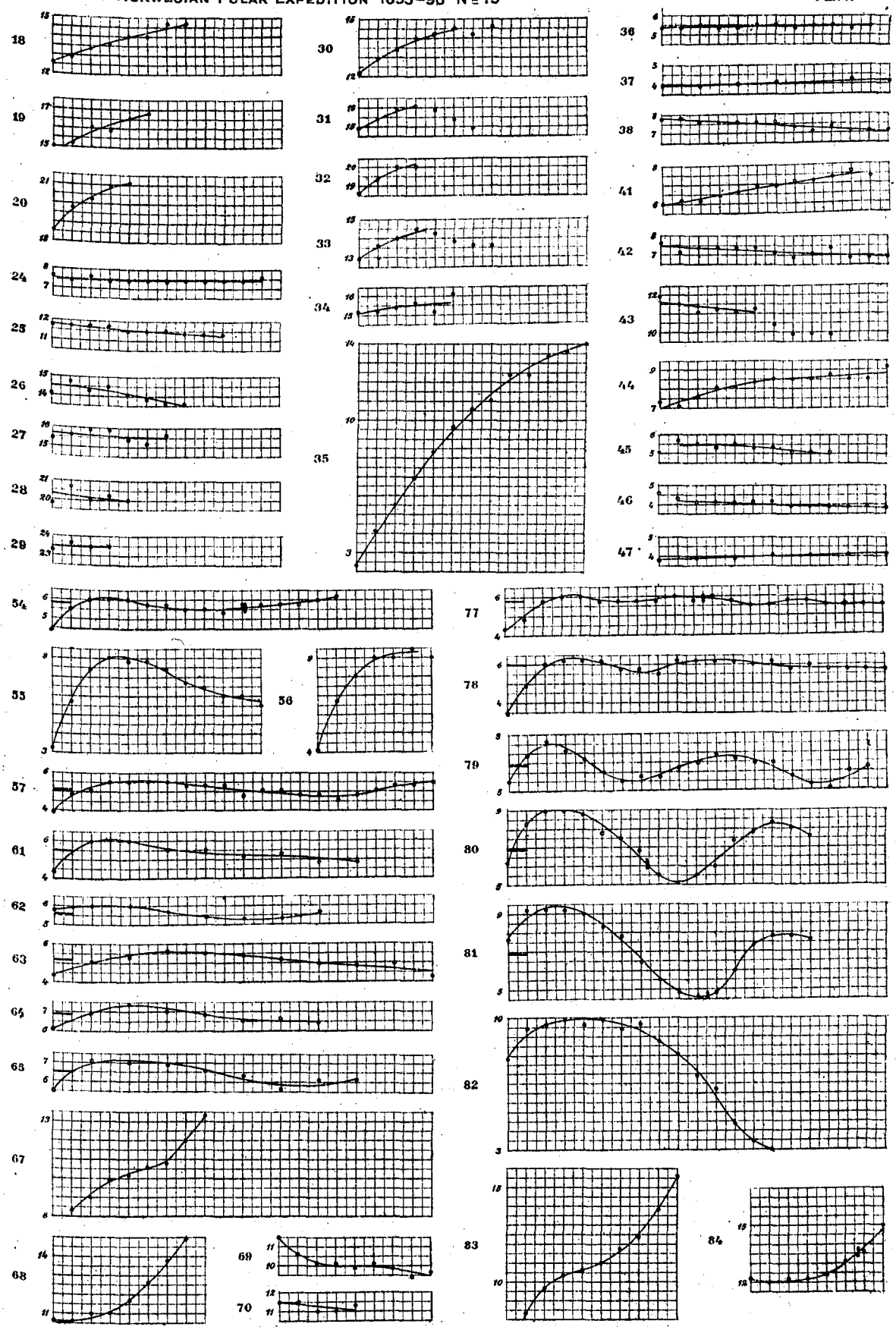


Fig. 9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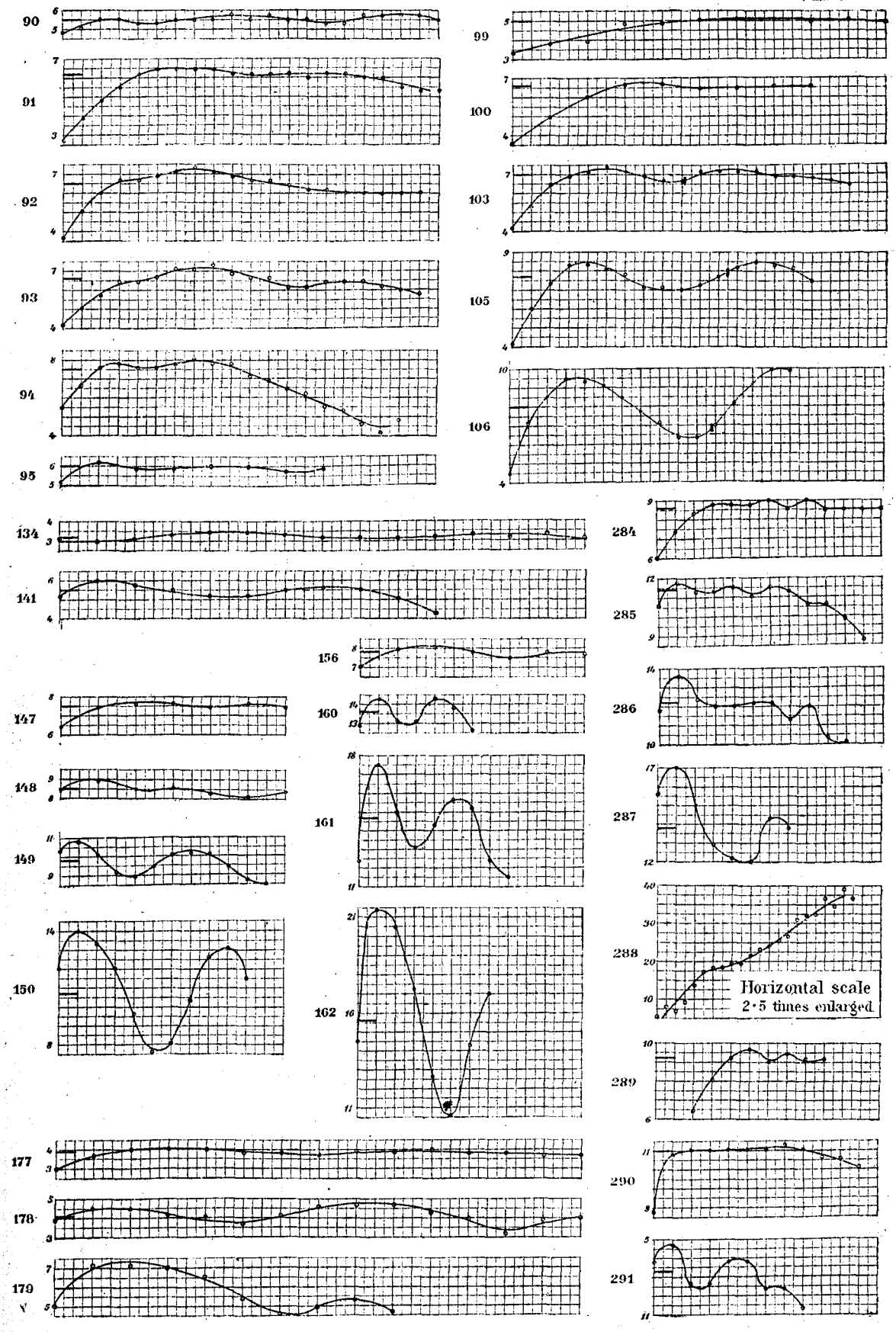

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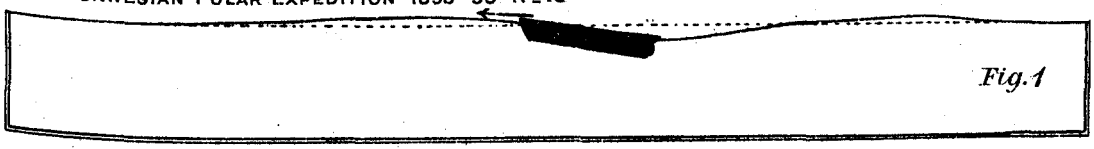

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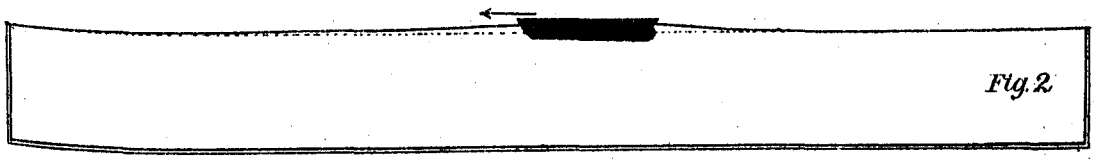

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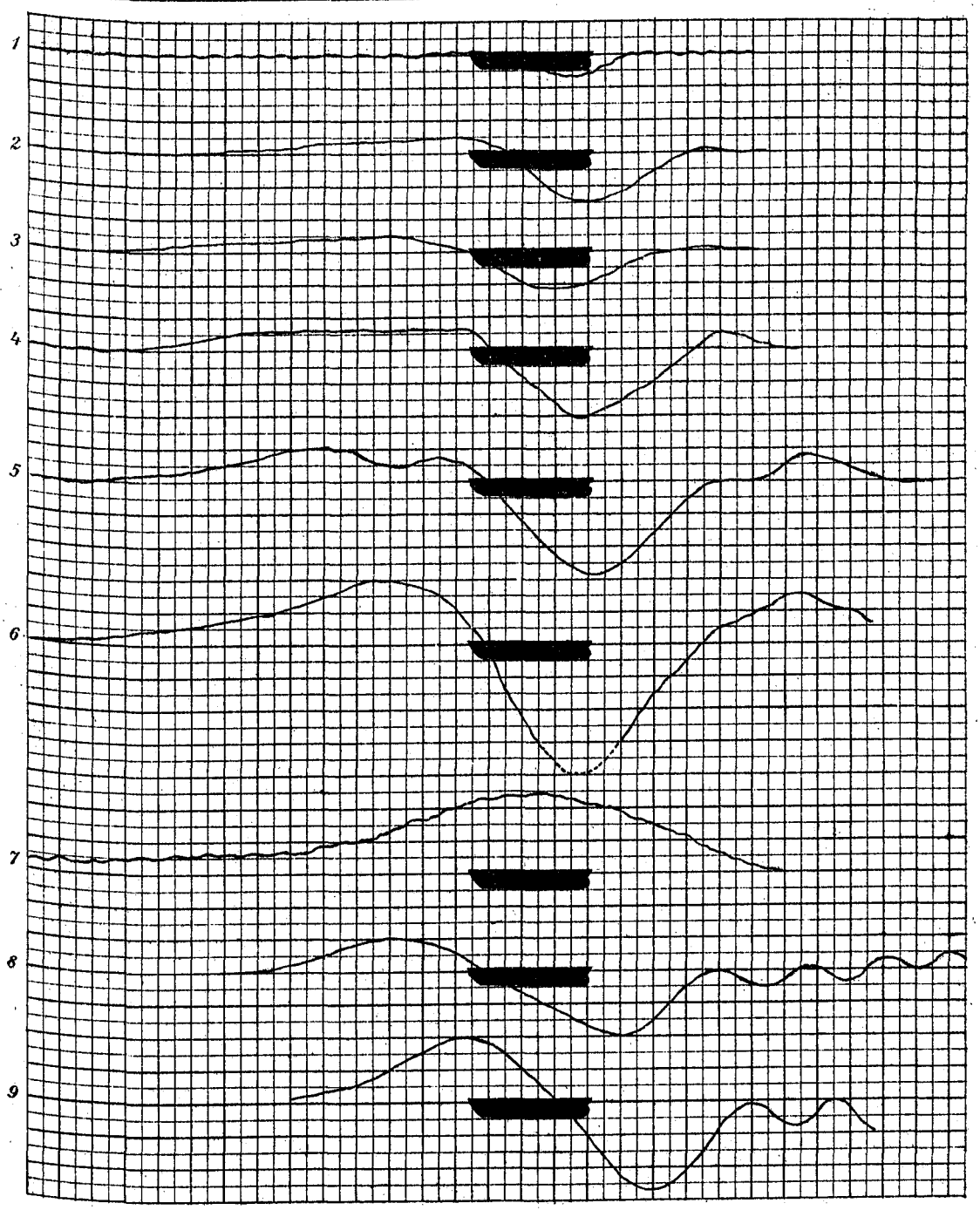

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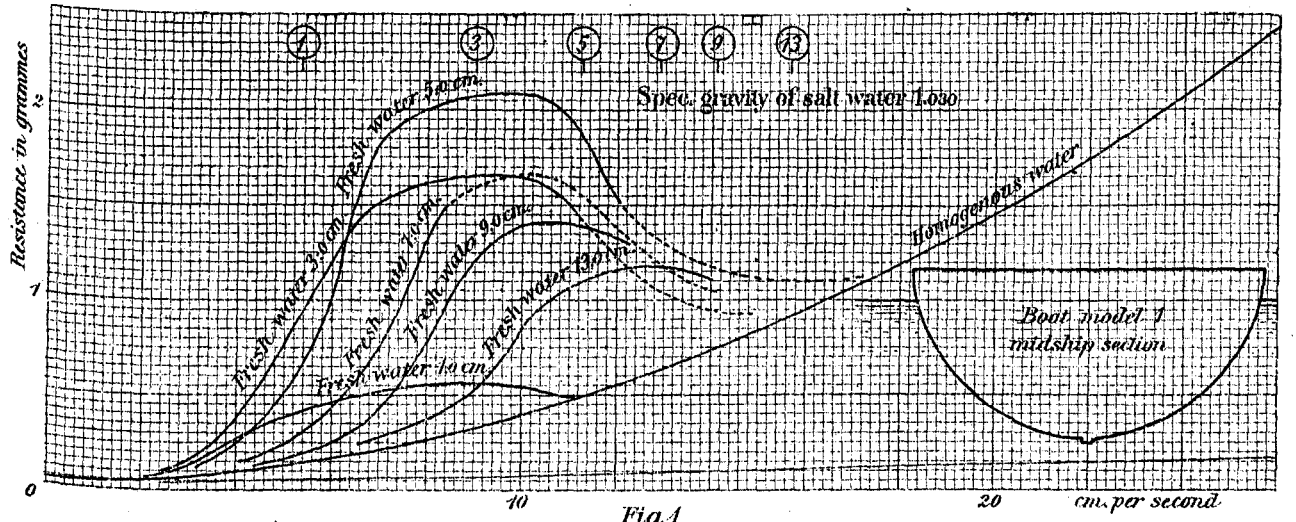

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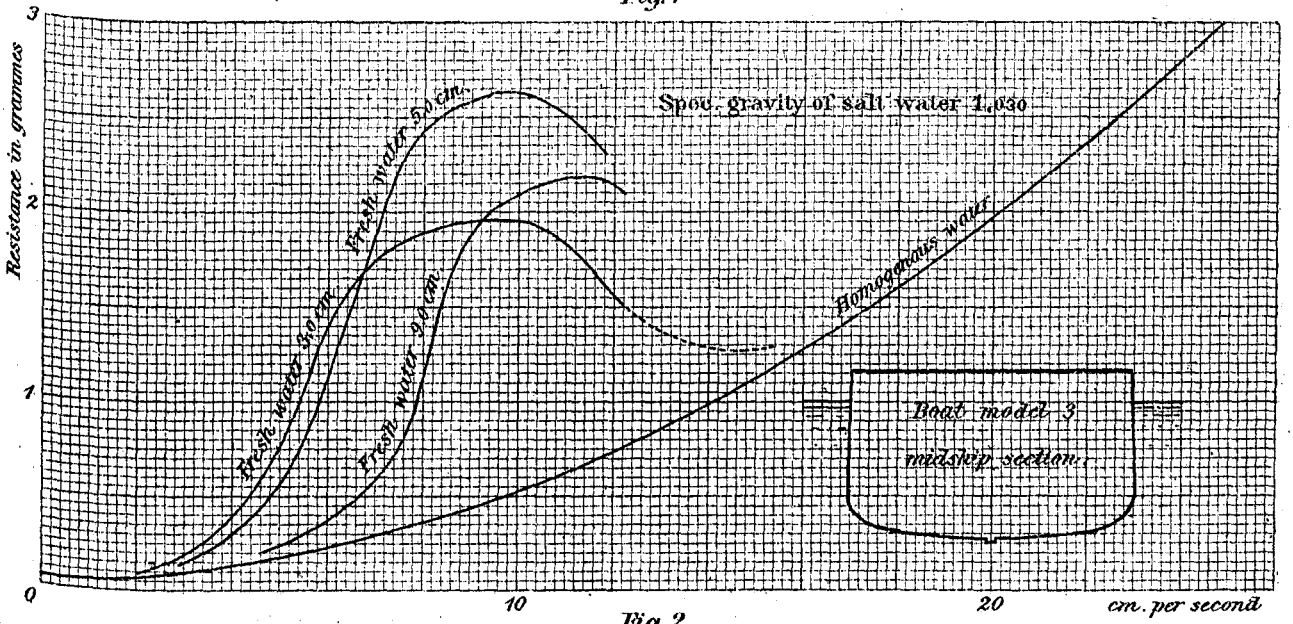

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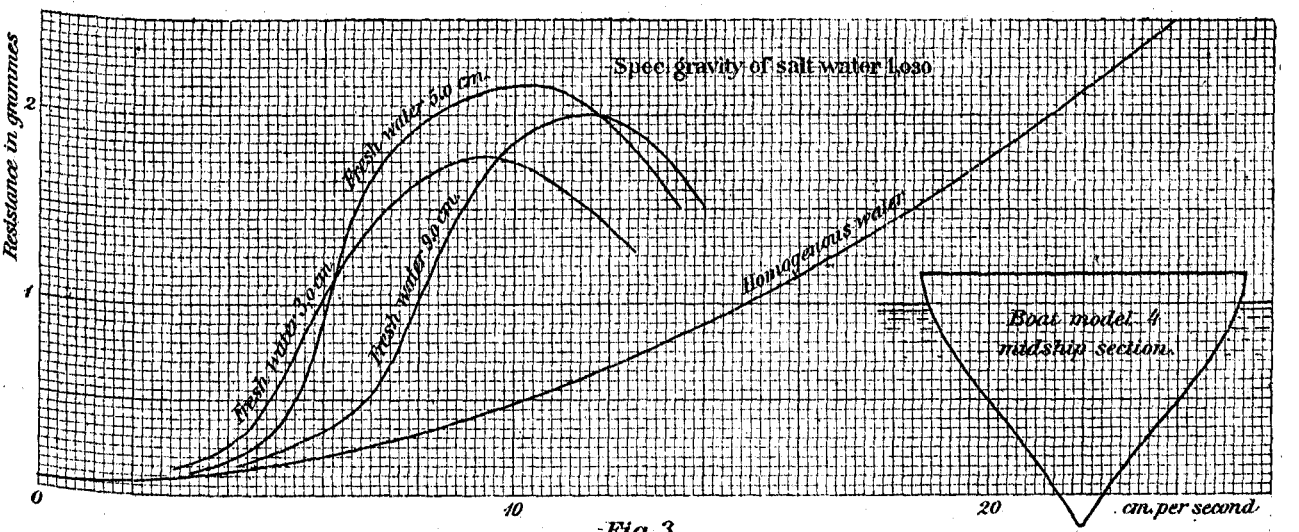

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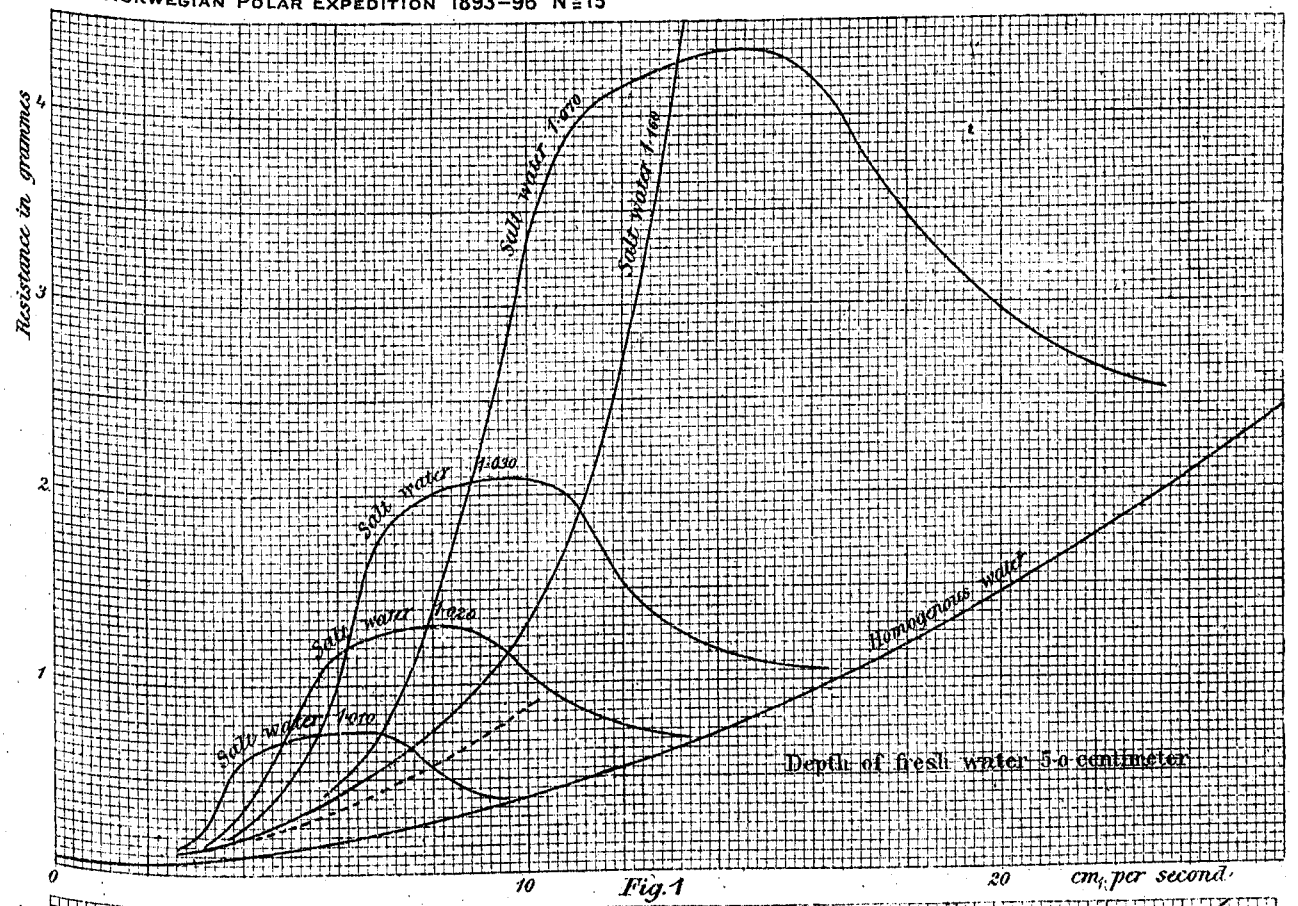

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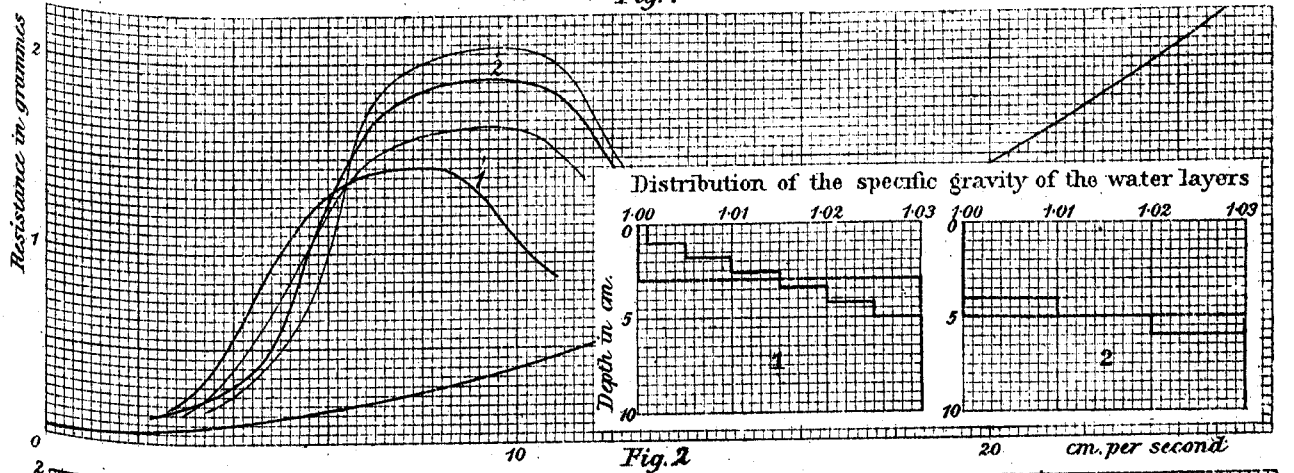

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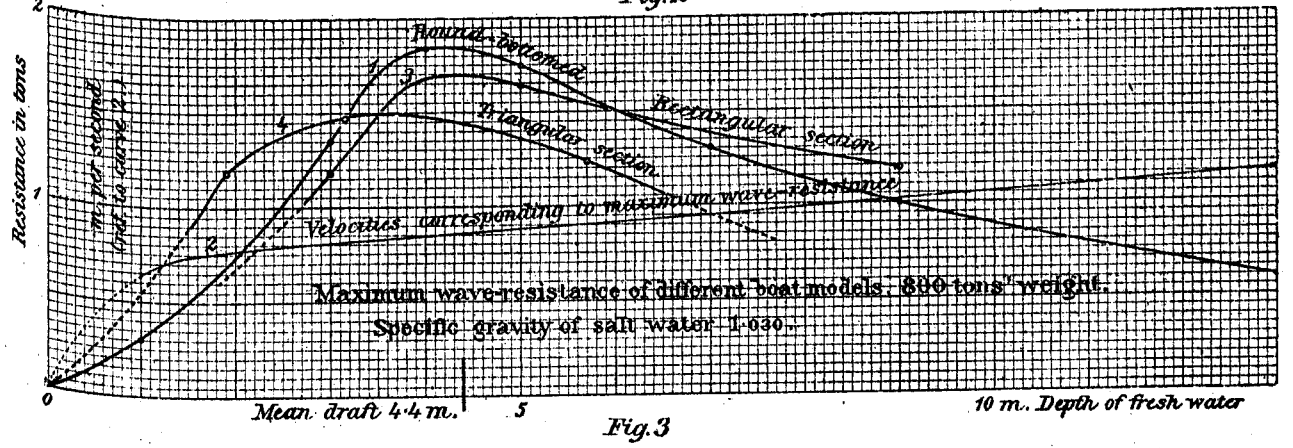

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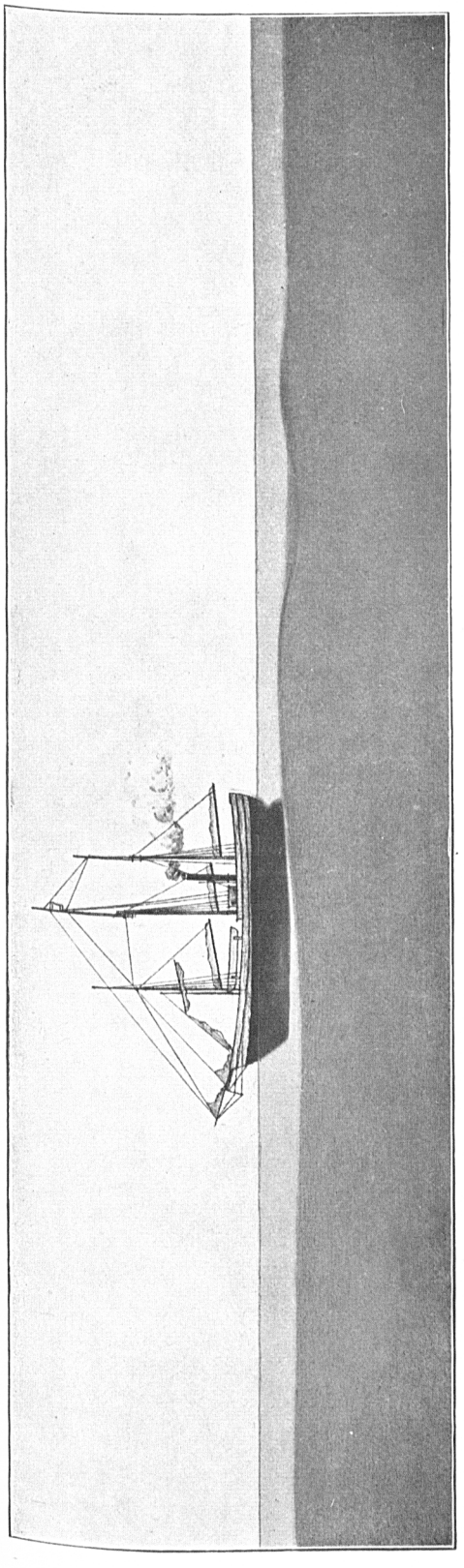

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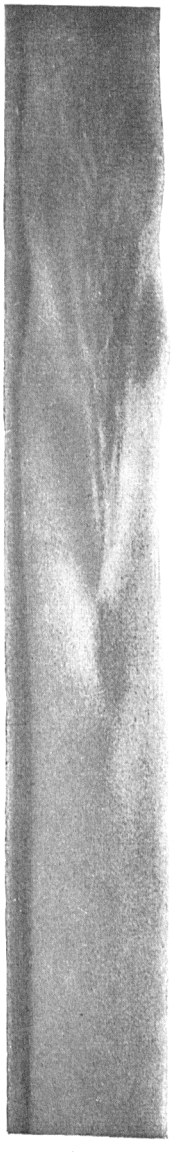

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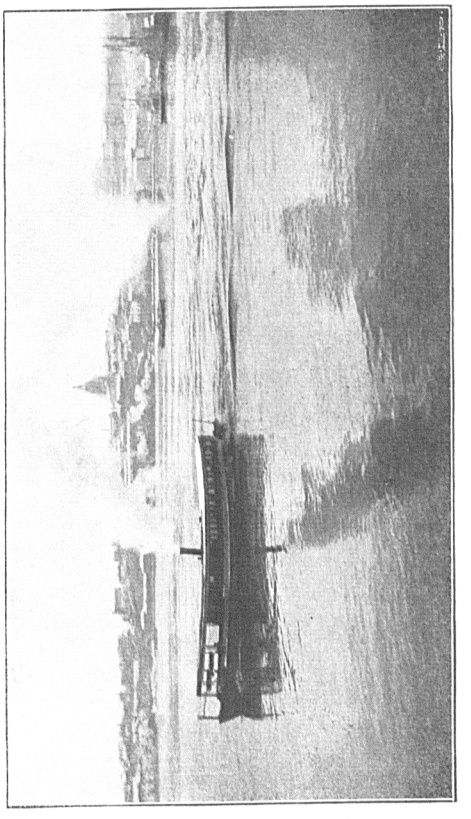

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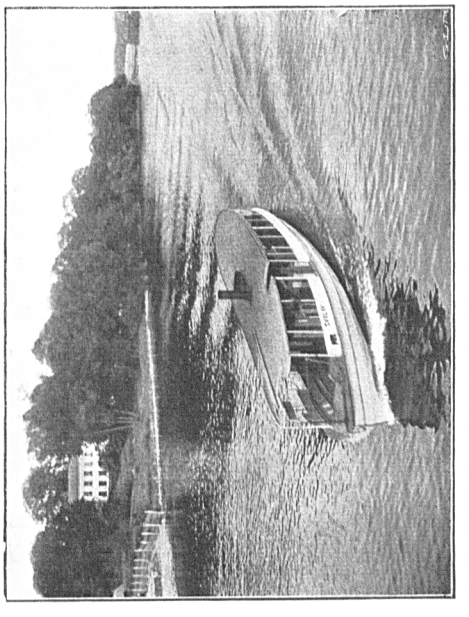

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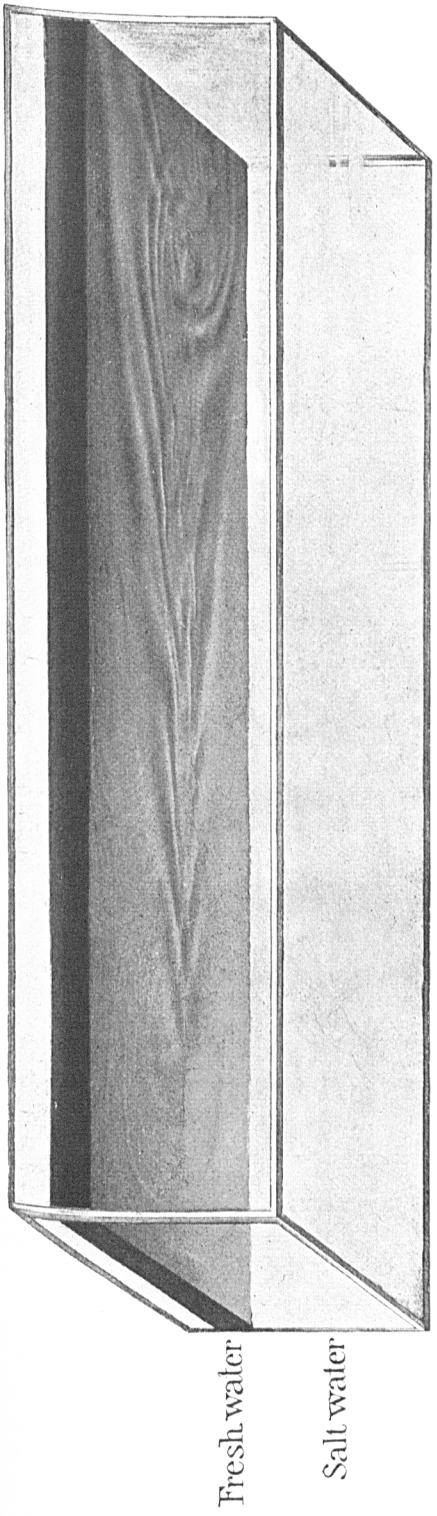

Fig. 1.

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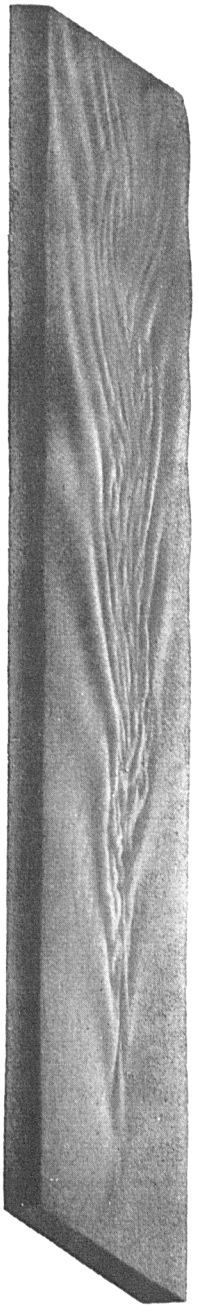

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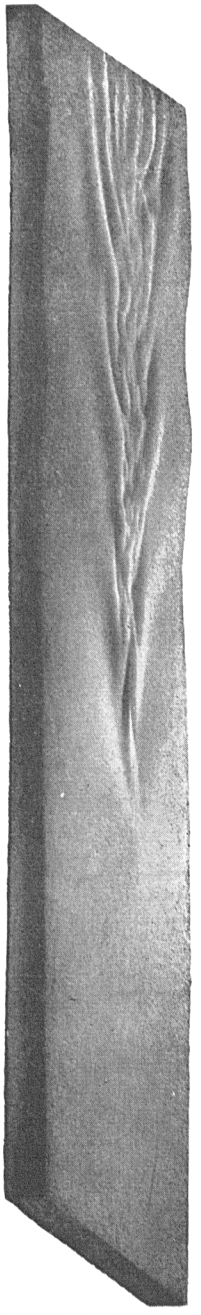

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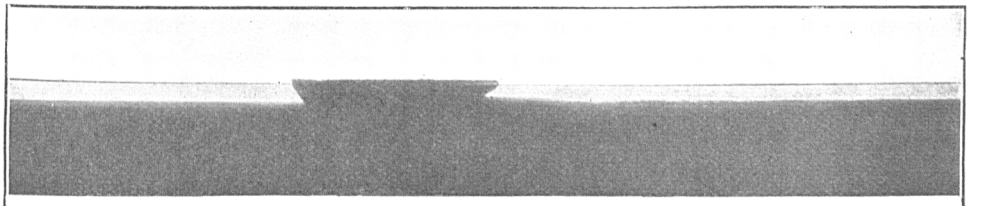

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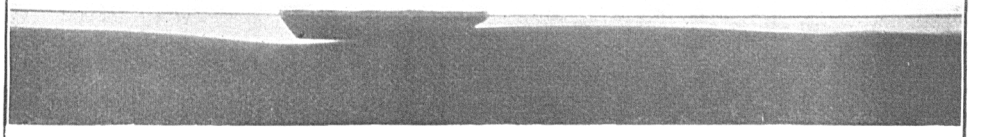

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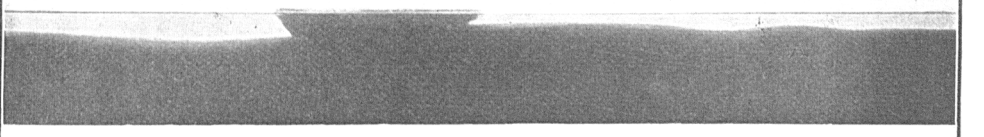

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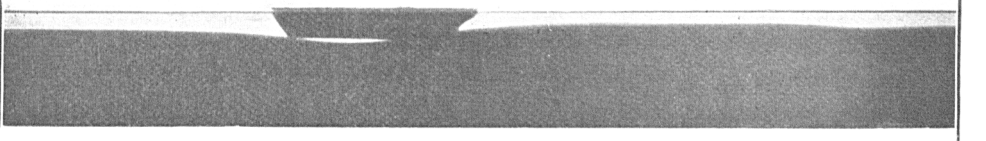

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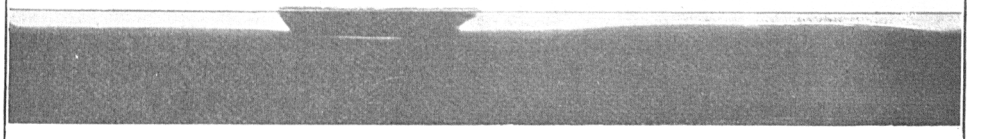

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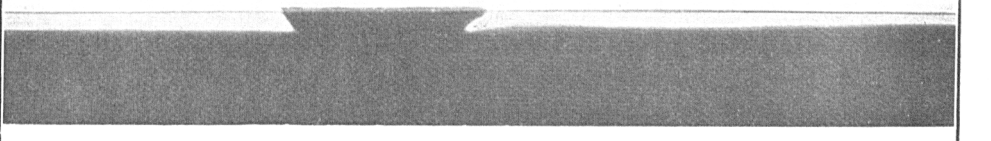

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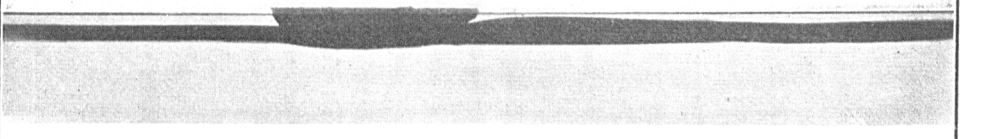

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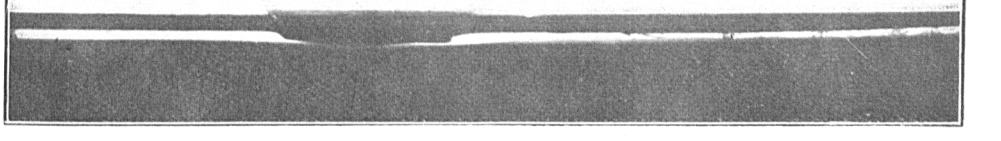

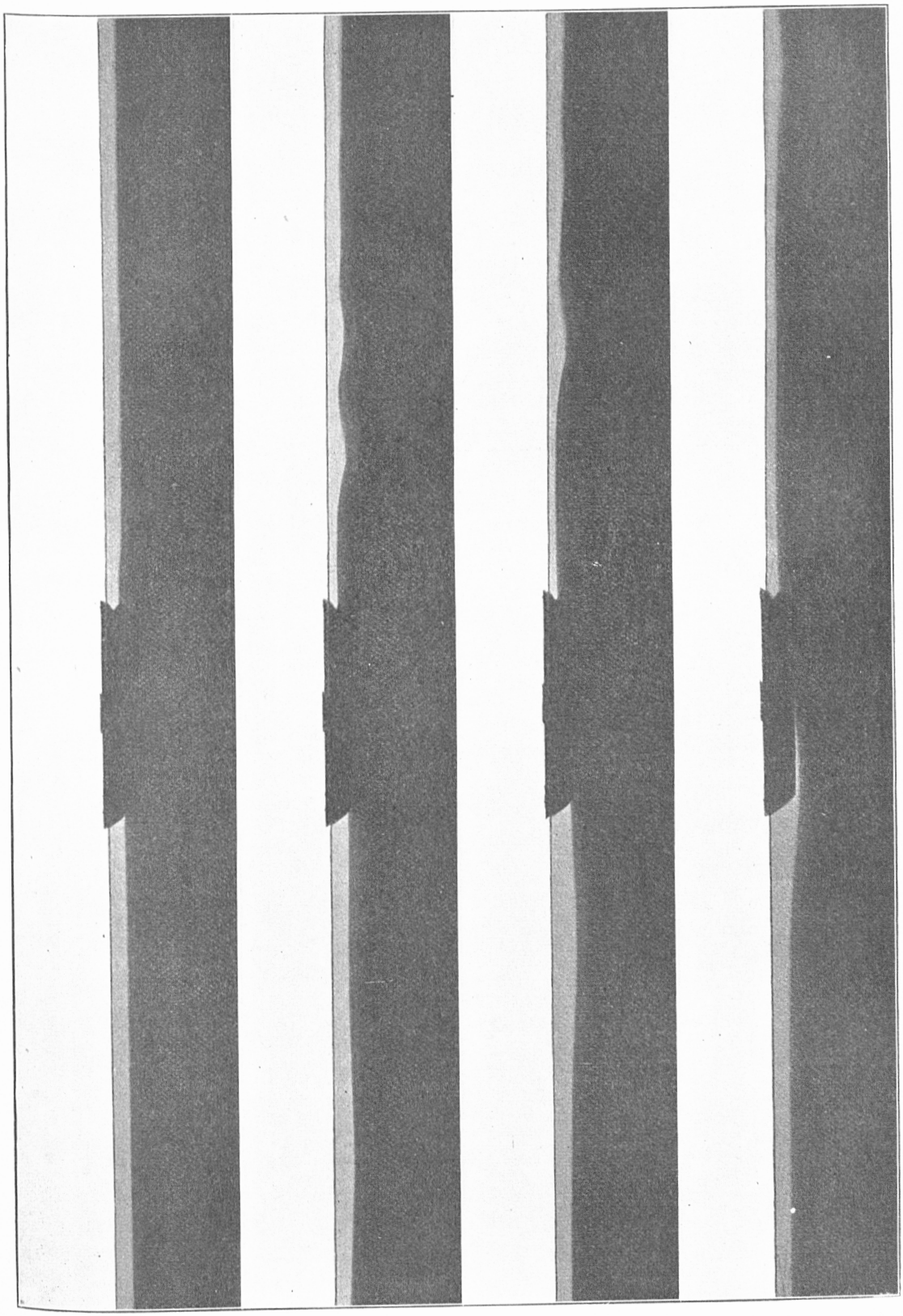

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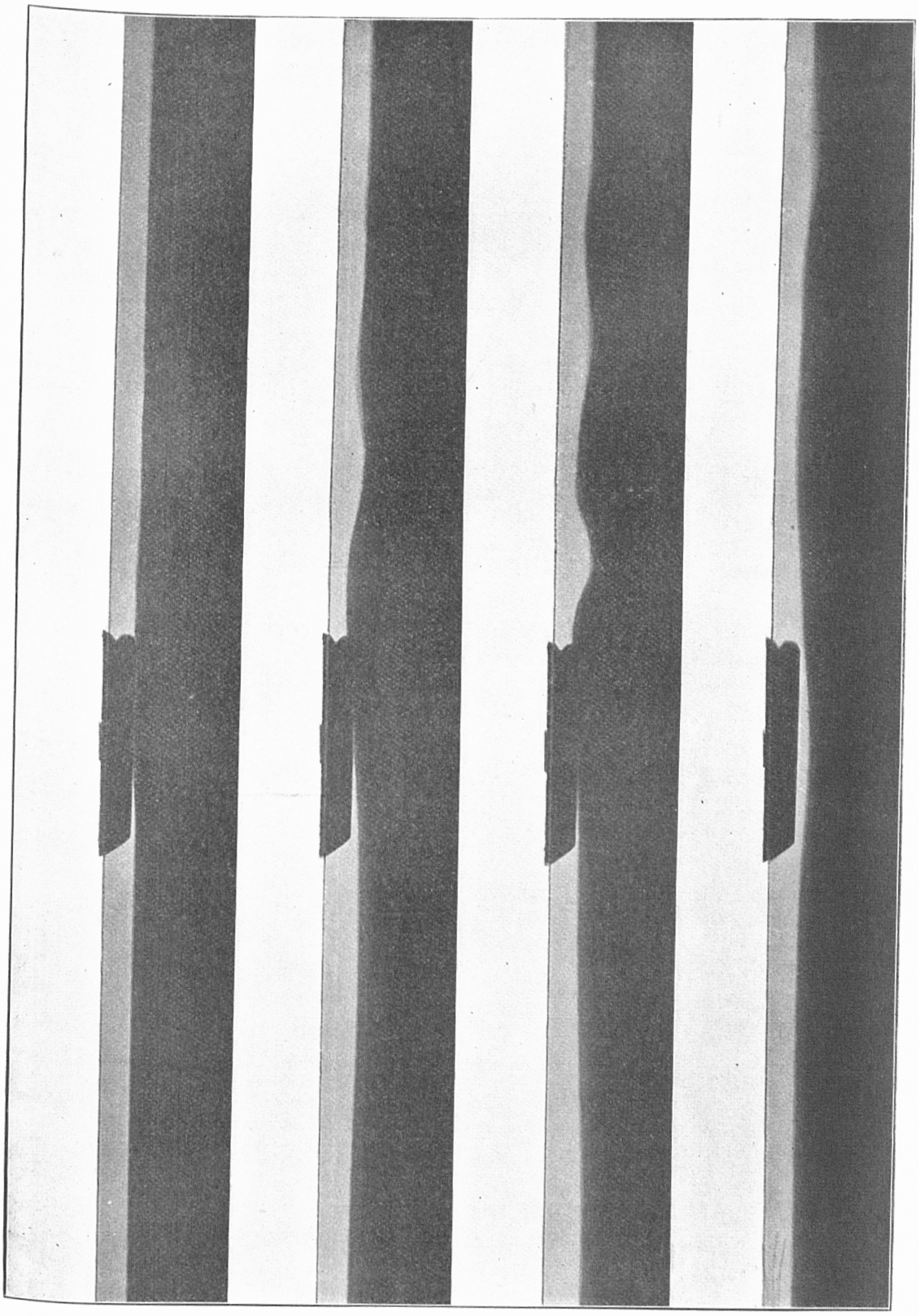

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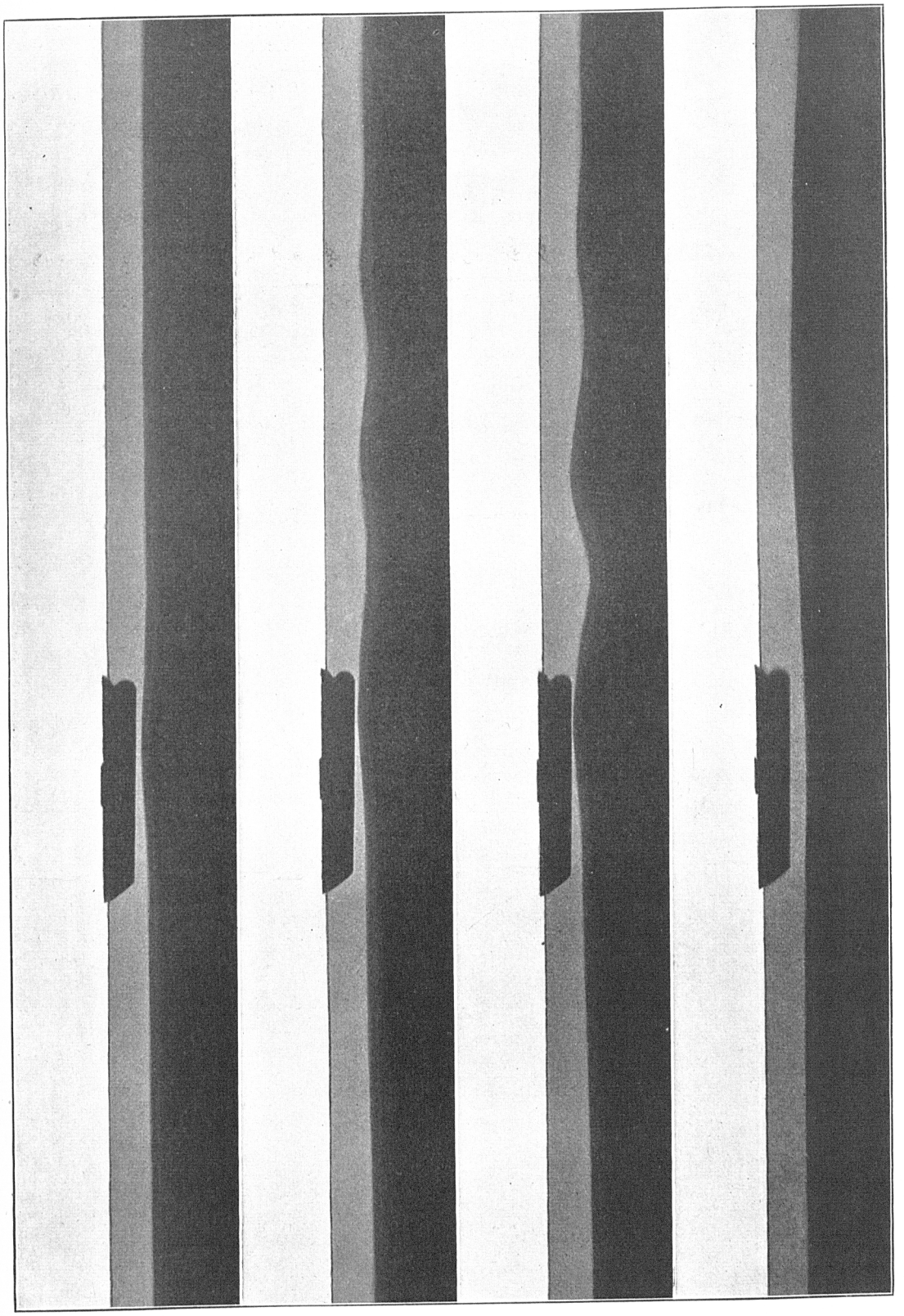

Fig. 1. Fig. 2. Fig. 3. Fig. 4.

XVI.

PROTOZOA ON THE ICE-FLOES OF THE NORTH POLAR SEA

BY

FRIDTJOF NANSEN.

(WITH 8 PLATES.)

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INTRODUCTORY REMARKS.

During my first visit to the East Greenland Sea in 1882, I noticed that in the summer, when the surface of the ice-floes was much melted, it got a very dirty and often brownish colour[1]. This was especially noticeable on thick and very old ice-floes — what I call the real polar ice — which evidently came drifting southward along the East Greenland Coast from very high latitudes, probably after having crossed the then unknown sea near the North Pole. I supposed that this dirty brownish colour was chiefly due to dust from the atmosphere brought down on the ice by falling snow. To some smaller extent I thought it might also be due to impurities or organisms in the sea-water which had been frozen into the ice, and which were now aggregated by the gradual melting of the ice at the surface. During this voyage I also noticed another feature, viz. that the thinner and comparatively new ice, one or two feet thick was frequently coloured reddish brown on the under side[2]. By examination under the microscope on May 9, 1882, I found that the colour was due to a layer of *algae*, chiefly *diatoms*[3], adhering to the underside of the ice, I had, however, no opportunity, nor the knowledge then sufficient to pay more attention to these highly interesting features.

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[1] See "*Naturen*", vol. XI, Bergen 1887, p. 214.

[2] This ice with a red underside was called "seal-ice" by the sealers, because they said that the seal preferred to lie on floes of that kind. This might not be improbable; for where there are so many diatoms in the water, there are probably also many crustacea, which form the food of the seal (*Phoca groenlandica*).

[3] According to the drawings I then made, the diatoms seem to have been *Coscinodiscus*, *Fragilaria*, *Navicula directa*, and others.

In 1888, I again saw the North polar ice in the Denmark Strait, on my way to the crossing of Greenland. I then used the opportunity of collecting a few samples of the mud on the ice-floes. One sample was taken from a thick layer of mud which had evidently somewhere come from the neighbourhood of land, whilst another smaller sample was collected from a greater area of the ordinary ice-surface, which had the common dirty appearance. The ice-floes from which the samples were taken were very old and thick; they had evidently drifted in the sea for several years, and had probably crossed the North Polar Basin. These samples were afterwards examined and described by Dr. A. E. TÖRNEBOHM and Professor P. T. CLEVE[1].

Dr. A. E. TÖRNEBOHM found the samples to be largely composed of mineral grains of different kinds. But the mineral grains in the smaller sample, collected from the greater area of the dirty ice-surface, were extremely small and difficult to determine. In this sample a great many diatoms also occurred, which were examined by Professor CLEVE who found 16 species and varieties, which were all of them identical with species of diatoms collected by KJELLMAN, during the Vega Expedition (in 1879), on an ice-floe near Cape Wankarema on the north-east coast of Siberia, near Bering Straits. These diatoms had been described by Cleve in 1883, and twelve of the sixteen species, found in my samples from the Denmark Strait, were hitherto only known from the ice-floe near Cape Wankarema. This seemed to indicate that the ice I had seen in the Denmark Strait, had actually come from the Siberian side of the North Polare Basin; as I had already assumed for other reasons. But how the diatoms had come on to the surface of the ice, or where they had originally lived neither CLEVE nor I could tell[2].

During the Expedition with the Fram, I had my attention directed towards all kinds of dust on the ice-floes. In the summer of 1894 the snow and ice on the surface of the floe-ice was melted by the sun, and ponds of fresh water were formed on the floes round the Fram. This began in the first part of June. On June 11th, 1894, I noted in my diary that the ice was rapidly

[1] H. MOHN and F. NANSEN, Wissenschaftliche Ergebnisse von Dr. F. Nansens Durchquerung von Grönland 1888. *Petermann's Mitteilungen*, Ergänzungsheft No. 105, 1892, pp. 104–108.

[2] Professor H. H. GRAN, has given a full account of what has been written about diatoms from the polar ice, in vol. IV, No. 11, pp. 5 *et seq.*, of this Report. Most of the papers there cited by him were, however, published after our departure in 1893.

melting on the surface, and many great and small fresh-waterponds had been formed on the floes, so much so that it was not even agreeable to walk about without water-tight shoes. We were then in about 81° 39′ N. Lat. and 122° 9′ E. Long.

In July I observed that numerous small brownish specks or, as it were, small accumulations of sediment were beginning to form on the ice-bottom of most fresh-water-ponds, especially on the thick floes which had been formed before the previous winter. Similar brownish spots were also formed on the socalled "ice-foot" in the channels along the margin of the floes[1]. By examing these brownish accumulations, from the bottom of the ponds and from the "ice-foot", under the microscope, I found in the midle of July, to my aston-

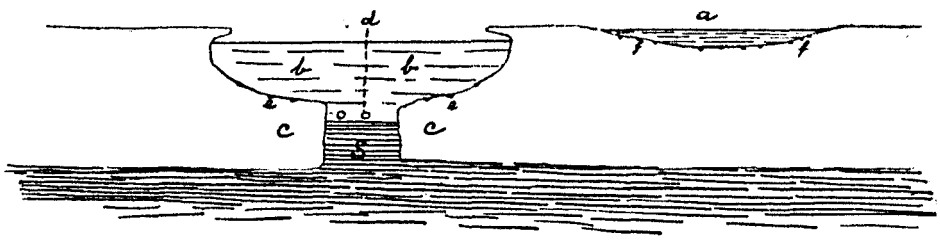


Fig. A. Diagram illustrative of the melting of the ice. *a*, Pond of fresh water on the ice-floe. *b*, Layer of fresh melting-water, resting on sea-water (*S*). *c*, Ice-foot. *d*, Lumps of algæ floating near boundary between fresh-water and sea-water. *e*, Small accumulations of algæ on ice-foot. *f*, Small accumulation of algæ in the pond on the ice.

ishment that they were composed of algæ, chiefly diatoms, living and multiplying in this water on the ice; to some smaller extent they were also composed of a little mineral dust and of dead fragments of diatoms, *Chætoceras*, *Coscinodiscus* etc., which had evidently been frozen into the ice on its formation; and their shell-fragments had now when the surface layers of the ice melted, been set free again and gathered on the bottom of the ponds and on the ice-foot. But among the living diatoms I also observed a good many moving organisms of various kinds. The biggest and most conspicuous

[1] By the melting of the snow and of the upper layers of the floe-ice a good deal of nearly fresh water (with a salinity of 1 or 2 per mille) is formed during the warmest summer months, June, July and the beginning of August. This water either accumulates in hollows in the floes to form ponds (Fig. A, *a*), or runs off into the channels and cracks between the flows where it forms a layer (Fig. A, *b*), 1 or 2 metres thick (see Memoir No. 9, vol. III pp. 305—309) of nearly fresh water resting on the cold sea-water (Fig. A, *S*). This surface layer of water becomes comparatively warm and has therefore a corrosive effect upon the edges of the floes by melting away the ice near the water level, whilst the lower part of the ice situated in the very cold sea-water is not affected, and it consequently projects, often several feet (see Fig. A, *c*), and is called the ice-foot.

were *Infusoria*, but numerous smaller organisms of great mobility belonging to the *Flagellata* or similar groups, were also observed and studied. In nearly all samples of these brownish accumulations I also very frequently observed a comparatively big bassilum, of a simple rodlike appearance, rapidly oscillating, and often forming long chains.

The brown spots on the bottom of the ponds (Fig. A, *f*) and on the ice-foot (Fig. A, *e*) grew gradually larger from day to day; but owing to their dark brownish colour they absorbed more heat from the sun than the surrounding white ice, and then the ice under them was more rapidly melted away, and they sank deeper and deeper into the ice, and formed small cylindrical holes (Fig. B) often several inches deep and perhaps an inch or more in diameter, with very sharp and square edges. The bottom of these vertical holes, was gradually quite filled with a brownish mass of diatoms, which could easily be sucked up with a glass tube.

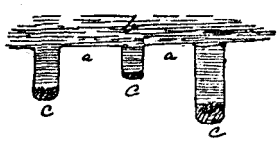


Fig. B. Diagram illustrative of the accumulation of algæ in holes on the bottom of ponds on the ice. *a*, Ice. *b*, Fresh-water. *c*, Holes.

In the channels between the ice-floes, especially in the narrow ones, where there was very little movement in the water, both Dr. Blessing and myself observed in July, 1894, numerous small globular lumps, generally of a reddish colour, or sometimes with a more bleaked whitish colour. These floating lumps grew rapidly larger from day to day; they began as small, hardly visible specks, but eventually attained considerable size, one or two inches in diameter, or even much more.

In my diary for July 18th, 1894, I find the following remarks about these lumps floating in the channels between the floes: First I mention mucous greenish brown masses composed solely of a brown alga, *Melosira*[1], which occurred often in great abundance at a certain depth (of about 1 metre or more) "almost in every small channel, especially the more enclosed ones, and one could see, that on the sides of the ice-floes, at a certain depth, a greenish brown layer spread over the surface of the ice, and far down into the water. It looked as if it was the same alga which here grew on the ice". I then

[1] In 1882 I found quantities of this alga in the stomach of a bear, who seems to eat them as vegetable food.

state that "in the water a great deal of smaller mucous lumps were also floating; they were partly white partly yellowish red, and I collected many of them. Under the microscope they appeared to be composed of accumulations of diatoms and a great many round red cells (see Pl. VIII, Figs. 7 & 8) filled with round refractive red globules". This large, globular alga (Pl. VIII, Fig. 7 & 8) gave the lumps their reddish colour, where, however, this alga had died and become colourless, the lumps got a white appearance externally. "These lumps of diatoms and algæ were all of them floating at a certain depth, about 1 metre[1] under the water-surface, where in some small channels they might occur in great quantity. At the same depth the above mentioned greenish brown alga (*Melosira*) was also chiefly distributed, whilst parts of it rose to the surface. These parts were often greenish brown as usual, but also often whitish and were evidently dead. It is clear that the lumps of diatoms as well as the *Melosira* keep themselves floating just at the depth where the upper layer of fresh-water (melting-water from the ice) rests on the underlying salt sea-water. The water on the surface was perfectly fresh[2] (*i. e.* it could be used as drinking water) and the lumps of diatoms sank in it, whilst they floated when they came lower down". (Fig. A, *d*).

These floating lumps composed of diatoms and the unknown red globular alga (Pl. VIII, Figs. 7 & 8) were very fragile and fell to pieces as soon as they were touched. They therefore had to be collected with great care. In these lumps I also found numerous *Infusoria*, and other small protozoa moving about between the diatoms; they were very similar to those found in the accumulations of diatoms in the ponds on the ice-floes.

The diatoms found in these free-floating lumps, as well as on the "ice-foot" and in the ponds on the ice have been described by Professor H. H. GRAN in a special memoir (No. 11, in vol. IV of this Report).

I studied on the spot, as well as I could, the Infusoria and other Protozoa found along with these diatoms, and at the same time I made numerous drawings of them, some of which have been reproduced on Pls. I—VIII.

Not being conversant with the subject and having no literature of the kind at hand, I could not determine the species, but could only make

[1] Somewhat later in the season, the thickness of the layer approached 2 metres, at least in some channels.

[2] It contained between 1 and 2 per mille salt.

drawings and notes of what I saw; I hoped however that I should later get an opportunity of studying them more closely. My time was taken up in many ways, so that I could not give much time to them then, and before the middle of August, 1894, the ponds and channels between the floes were covered all over with ice, and I finally got no further opportunity of studying them at all, as I left the Fram on a sledge journey before the next summer.

Since my return from the expedition I have been occupied with other investigations, and have not been able to give any time to the matter.

Miss KRISTINE BONNEVIE has, however, done me the great favour of looking through my drawings and notes and of selecting those figures which may be of most interest. They are here reproduced on Pls. I—VIII. I owe her much gratitude for her very valuable assistance. We both agree, that the material at hand is hardly sufficient for the determination of the species even in the hands of an expert on the subject.

As it appeared possible, however, that my observations, imperfect though they are, might be of some interest to future students of this matter, I publish them in the form they were brought home. They may at least serve to draw the attention of future travellers to this interesting life on the drifting ice-floes of the North Polar Sea.

It might seem puzzling, how these organisms have got into the ice in the first place. The probability is that they, or rather germs of them, have been frozen into the ice when it was formed at the surface of the sea. When the ice again melted the germs developed in the ponds on the ice in the same way as the diatoms, and were also carried by the melting water into the channels between the floes.

It seems probable therefore that these infusoria were of marine origin like most diatoms found in the same ponds. The water of these ponds contained, however, only between 1 and 2 ‰ salt.

It seems hardly probable that the infusoria could have been carried by the wind through the air. Some of them were found on comparatively white and clean floes, which had been formed during the previous autumn and winter, far from any sea-shore or open water.

LACRYMARIA, sp.

Pl. I, Figs. 1—14; Pl. II, Figs. 5—7.

This infusorium occurred very commonly both in the free-floating lumps of algæ, in the channels between the ice-floes, and in the accumulations of diatoms in the ponds on the ice. The individuals were as a rule transparent and colourless, but in some ponds on the ice I also found numerous animals of exactly the same external appearance and shape, but which were dark-green, or almost black. The normal, transparent form will first be described.

When it moved actively about, as was generally the case, it had a more or less elongated form (Pl. I, Figs. 1, 2, 14). The body was very mobile and could be stretched out and become rather slender (Figs. 2 and 14) or it could be shortened and become thicker (Figs. 1 and 3, Fig. 1 is the same individual as Fig. 2) or it could even be contracted into a spheroidal, motionless globe (Fig. 10). When the body was stretched out, it could bend in a worm-like manner (Fig. 2) and then it could rapidly wind its way between the diatoms, or could swim freely through the water while its body generally rotated slowly round its longitudinal axis. The length of the body of well developed individuals might, under these conditions, be 0·10 mm. (Fig. 1) or more.

At the anterior end there was a *proboscis*-like protuberance, which during activity could be pushed out and bent vigorously to the sides whilst, in a more quiescent state when the animal was contracted into a spheroidal form, the proboscis was partly retracted. This proboscis was provided with numerous long cilia, which were in perpetual active motion. Whether these long cilia were actually fixed to the proboscis, or merely situated at its base, surrounding it, was not ascertained. The surface of the body was provided with somewhat shorter not very dense cilia. They were somewhat more numerous at the posterior end.

In most individuals furrows could be seen, winding in left handed spirals along the outside of the body (Figs. 3—7, 13—14).

As far as I could make out, the spirally wound ribs thus produced, were seven in number (at least on the anterior part) and ended in seven(?) lobes round the proboscis (see Fig. 5, where four of these lobes are seen). These ribs could sometimes also be seen on the posterior end (Fig. 3).

On several occasions I believed I was able to make out that the cilia on the surface of the body, were situated along the spirally wound furrows.

Some individuals did not, however, show any indications of these spirally wound furrows. This was, for instance, the case with the one, illustrated in Figs. 1 and 2. This specimen was on the whole very big and well fed, and it is possible that the furrows may have apparently disappeared, owing to the abundance of food which had dilated the outer membrane. I consider this explanation more feasible than that this was another species. It behaved othervise in every respect, like the other specimens.

The nucleus of the animal was situated in its central region, and was, as seen in individuals killed with osmic acid, sometimes oblong (Figs. 6—8), and sometimes extended into four branches (Figs. 4—5).

A vacuole was very frequently seen in the posterior region (Figs. 4, 5, 14). It was sometimes divided into three smaller vacuoles (Fig. 10).

The cell-contents were as a rule, at least in the outer layer, filled with numerous globules (see Figs. 1 and 2), which were probably drops of oily substance (nourishment) dispersed in the protoplasm. These globules were more sharply defined by the effect of a very thin solution of osmic acid (Fig. 9). After the specimens, having been stained, had been transferred to Canada Balsam the globules had entirely disappeared; they (Figs. 4—6) had probably been dissolved. The oily globules were in the transparent specimens colourless, or they might have a slightly greenish tinge.

In the interior of specimens transferred to Canada Balsam, refractive grains were often seen (Figs. 4 and 5). They were evidently mineral grains or debris of diatom-shells, which had been taken in with the food. In Fig. 4 a good many such grains are seen accumulated in the vacuole near the posterior end. In Fig. 5 similar grains outside the individual are also seen adhering to the cilia round the proboscis.

When the animals were killed with osmic acid or chromo-aceto-osmic acid, they sometimes retained more or less of their elongated shape (Figs. 4—7). But sometimes they contracted into a more or less spheroidal form like Figs. 8 and 9, and the spirally wound furrows then often disappeared. Fig. 9 was an individual which while alive, had the same appearance and was in the same preparation as Fig. 3.

The dark specimens. I mentioned above that among the diatoms in some ponds on the ice, I sometimes found numerous individuals actively moving about; they had externally exactly the same shape and appearance as the above transparent forms, and behaved in the same manner, but they had a dark green, or almost black colour (see Pl. II, Fig. 5). I do not think that this was a different species, but rather believe that the dark colour might be due to the food which the animals had eaten; and the stain might probably be contained in the oily globules. For the colour was evidently due to dark grains or globules dispersed in the otherwise colourless protoplasm (see Pl. II, Fig. 5). Unfortunately I did not get an opportunity of examining whether these dark grains or globules were dissolved by transferring the specimens to Canada Balsam[1].

When individuals of this infusorium, either of the transparent form (Figs. 1 and 3) or of the dark form just mentioned, were followed under the microscope for some hours, they were generally seen at last to contract into spheroidal almost motionless globes shown in Pl. I, Figs. 10—12, and Pl. II, Figs. 5—7. Figs. 10 (Pl. I) represents a colourless individual which was followed from the very beginning, when it had the normal shape, like Figs. 5 and 3, and moved actively about, until it had contracted as represented, and remained nearly motionless. The proboscis was now much reduced, and merely formed a short protuberance. In the posterior region three vacuoles were seen. (The figure is drawn with comparatively small magnification.)

On July 28, 1894, I followed the formation of the quiescent globular form of several dark individuals. The process was quite the same as observed on transparent, and colourless individuals. I have given the following description of it in my note-book:

[1] In my note-book, July 24, 1894, I find the following remarks about these dark specimens: "Individuals of exactly the same shape, appearance, and size as Figs. 6 & 7, but perfectly dark or almost black, are seen in great number in the samples of diatoms from this ice-floe. Whilst the transparent form, Fig, 7, is, as a rule, colourless, some individuals from this floe were greenish brown, I presume owing to chlorophyll obtained from digested algae. I also presume that the dark colour mentioned above, may be due to particles of food. The dark stain seemed to occur in the form of lumps or globules, situated very near each other but still with transparent intervals between them (*cf.* Pl. II, Figs. 5 and 6). On one of these dark specimens I saw a dark sausage-shaped body being pushed out of the posterior pointed end, the animal was then in rapid motion. This dark body adhered for some time to what I consider to be the anal opening, but soon dropped away. The oral opening I have not been able to ascertain".

"In the beginning of the observation the animal had the same regular shape as the ordinary transparent and colourless form (Pl. I, Figs. 3, 4 and 5). The animal now began to swim about rapidly backwards in the open spaces of water, continually rotating round its longitudinal axis the same direction as the spiral-windings which are left-handed. The proboscis was stretched far out, being dragged after, apparently in an inert state. Neither in this nor in a later individual did I see the proboscis move while in this state. The body was now gradually shortened and finally assumed a perfectly globular shape as in Fig. 6, *a*. Whilst this happened the proboscis disappeared to same extent, and only a short protuberance (Fig. 6, *a*) remained. It seemed to me as if the rest of it might have been dropped, but it may probably have been contracted into the body. In certain positions of the animal, *e. g.* in a lateral aspect when the proboscis and anal region were seen simultaneously, it looked as if the cilia were situated several together at certain intervals (Pl. II, Fig. 6, *a*). This was very often the case only on one side of the animal (see Fig. 7, c, under side). I sometimes was able to count 9 such small tassels of cilia on one side between the anterior and posterior end. In other positions of the animal, when it was seen more from one of the ends, and had a perfectly round appearance (Pl. II, Fig. 7, *a*) the cilia were more uniformly distributed along the whole circumference of the body. I got the impression that the cilia were chiefly situated along the furrows of the spiral windings, *e. g.* in Pl. II. Fig. 7, *e* there seemed even to be indications of such small depression, in which the cilia were situated, along the under margin of the animal. In the oral and anal region the cilia were more densely distributed, and were also longer, especially in the oral region.

"After a while the globe, (Pl. II, Fig. 6, *a*) burst, and parts of the contents were spread outside the membrane (Fig. 6, *b*). There were a great number of rapidly oscillating, dark green grains, of exactly the same colour as the cell contents. They seemed to be the cause of the dark colour of the animal. By means of their oscillating movement these grains swam far away in all directions[1]. After about half an hour they became mostly quiet, until finally the whole mass seemed perfectly dead. The observation was made in a drop of water, hanging under a cover-glass, in a microscope aquarium. The

[1] This oscillatory motion may probably have been due simply to the surface tension.

bursting of the cell could not therefore be due to pressure of the cover-glass, as I had thought possible on other previous occasions. Neither was dessication the cause, for as far as I could see there was plenty of water in the drop. But something may probably have been wrong with the water, because another infusorium that lived in the same drop, also seemed to die after a while."

"Pl. II, Figs. 7, *a*, *b*, *c*, and *d*, are drawings of another individual of the same kind, which I observed under the microscope for 12 hours, on July 28, 1894. Fig. 7, *a* was drawn at noon, and the last drawing, Fig. 7, *d*, was made at midnight on the same day. The animal was living all the time, and the cilia were in rapid motion, especially during the first six hours. The long cilia round the proboscis were especially active, and were bent far towards all sides. After 9 hours (Fig. 7, *c*), the cilia were still in constant motion in the anal region, but perfectly quiet in the anterior portion of the animal. The proboscis had now almost entirely disappeared, only a small protuberance being left. After 11 hours no cilia moved. The form of the animal was during the first six hours almost perfectly globular, even in a lateral aspect (see Fig. 7, *b*). It became then more oblong, like a short egg (Fig. 7, *c*), thus it remained for four hours (Fig. 7, *d*); but then (after eleven hours) it again became perfectly globular.

"In the anal region at the posterior end, a colourless, limpid space often occurs (Figs. 7, *b*, *c*, *d*); which is sometimes larger sometimes smaller, but always with the same locality in the anal region, where the latter is slightly pointed. Small dark grains frequently occur inside this clear space, and are seen to move. They sometimes pass from the dark cell-contents into the limpid space approaching the cell-membrane. A little later the limpid space is suddenly and entirely closed by the dark mass; which after a while begins to withdraw slowly from the membrane, to give room for the limpid space again. While this is going on the cilia outside are in active motion[1].

[1] On one such occasion I saw a spore-like body come out from these moving cilia, and it appeared to have come through the anal opening, but might nevertheless have come from the surrounding water. It had an oblong form, narrow in the middle like an hourglass or the figure eight; of yellowish colour, it was extremely small, and hardly visible with the magnification used (Zeiss *CC* and 5). It had a rapid oscillating motion; it moved about for a while, and then became quiet. Several similar corpuscles were seen in the neighbourhood. Half an hour later a perfectly similar corpuscle came out from the cilia.

This colourless space is evidently a vacuole. At the posterior, slightly pointed extremity of the body, I believe I have seen a small opening in the membrane of the animal; but I have never with certainty seen anything being pushed out through it".

"The contents of the cell have the same dark green granular appearance in all these dark individuals. Near the margins single grains can be distinguished, of the same size and appearance as the oscillating grains seen in Fig. 6, *b*".

"Here and there small colourless vesicles occur, which might be vacuoles if it had not been for their small size — they are not much greater than the dark grains or globules — nor have I seen any contractile movement in them, although they seem to change slowly".

Pl. 2, Fig. 5 represents a similar spheroidal, motionless form of dark individual seen on another occasion (July 24, 1894). But here the cilia were apparently longer and more numerous than in most other individuals observed, and no indication of the spirally-wound furrows was to be seen.

Pl. I, Figs. 13 and 14 probably represents another but nearly allied species of this same genus of Infusorium. It was observed on August 2nd, 1894, amongst diatoms from a pond in a floe. In its mobile state (Fig. 14) it was conspicuously more extended and slender than the above described form. "I followed it for a long time, and there was a striking difference between the elegant worm-like movements with which it moved through the narrow passages between the algæ, and those of the thicker form above, in both dark and transparent modifications. It was never seen to contract to a shape approaching that of the latter, and the body seemed to be also more "muscular", as the spirally wound furrows became more conspicuous on contraction (Fig. 13).

After a while, it contracted into the motionless stage; but this did not occur in the same manner as often observed (see description above); it took place more gradually and quietly: It did not run about in long circuits backwards through the water; the "muscles" were more and more energetically contracted, the spirally-wound ribs became more prominent and protruded (Fig. 13). They are possibly somewhat more numerous than in the above form. As in the latter there is a vacuole near the posterior end, and this is conspicuously visible both in the mobile and in the quiescent state. In its quiescent state

it is not as globular as is the above form, and further the spiral-windings are very prominent".

To this description from my note-book I may add that it may be possible that this was simply an individual of the same species as above, only less filled with foot.

In the reproduction of Fig. 13 there are a few mistakes. On my original drawing there were many cilia pointing backwards in the anal region, which have been forgotten here. The cilia on both sides of the body are in my original orawing situated in the spiral furrows, and not on top of the ribs.

Both figures (13 and 14) are drawn without the aid of camera lucida, and with comparatively small magnification.

CHILODON(?) sp.

Pl. II, Figs. 1–4; Pl. III, Figs. 1–7, 9, 14.

This form seemed allied both to *Orthodon* and *Chilodon*. It was not, as far as I could see, uniformly covered with cilia, but had its cilia arranged in a double series round along the length of the amind (Pl. III, Fig. 5). This infusorium was very common in the accumulations of algæ in the ponds of fresh-water on the ice-floes where it moved rapidly about. As it occurred sometimes in ponds on comparatively clean and white ice-floes, it seems probable that the animal did not originate from anywhere near the land, for such ice-floes had been formed on the sea, during the previous autumn and winter, far from the coasts.

For a description of this infusorium the reader is referred to Pls. II and III and the Explanations of these Plates.

It is perhaps doubtful whether several species were observed. Pl. II, Fig. 4 was much larger than the species reproduced in Pl. II, Figs. 1—3 and Pl. III, Figs. 1—7, and possibly belonged to a different species. The usual length of the individuals was about 0·05 mm. or a little more, whilst Pl. II, Fig. 4 was 0·099 mm. long.

Pl. III, Figs. 9 and 14, were somewhat smaller than the above individuals, they were about 0·043 mm. long but possibly belonged to the same species or a similar species.

Pl. IV, Fig. 6 probably also represents a species allied to the above infusoria (Pl. II, Figs. 1—4). It was taken from free-floating lumps of algæ, in a channel between the floes, and was of about the same large size as Pl. II, Fig. 4 which it resembled much. Its length was 0·1088 mm.

Pl. III, Figs 10—13 and 15—16 represent small animals which were often seen in preparations from accumulations of algæ in the ponds. They possibly belong to similar species as the above infusoria. For description see Explanation ot the Plate.

STYLONYCHIA(?) sp.

Pl. IV, Figs. 1—5.

This infusorium was often seen moving rapidly about both in the accumulations of algæ from the ponds on the ice, and in the yellowish red lumps or communities of algæ floating at a certain depth (about one or two metres) in the channels between the floes. For description see Explanation of the Plate.

The species seems to be at least nearly allied to the genus *Stylonychia* but it does not perfectly agree with the descriptions of the latter.

ORGANISM REPRESENTED PIS. V AND VI.

While I was examing the big organism represented in Pl. VIII, Figs. 1—3, a small organism came slowly "padling" into the microscopic field. It was oblong and, with the small magnification I then had, seemed to resemble exactly, the individual reproduced in Pl. V, Fig. 7, *A*. I changed objectives to look at it under higher magnification (Zeiss *F*, oc. 4) but it was imposible to find it. Instead, I found on the same spot the organism reproduced Pl. V, Fig. 1. At first this individual did not move; but gradually the protoplasmic part, *b*, began to contract (Figs. 2 & 3), and then to separate from the more refractive part, *a* (see Fig. 4). It soon separated entirely and moved away from *a* (Fig. 5). Then a cilium also suddenly became visible, which had not been seen proviously. At the same time *b* got a more regular oval form (Figs. 5, 6), and a longitudinal line was seen along the the middle, dividing the contents into two parts with a round corpuscle or vacuole in each (Fig. 6, *b*).

By means of the cilium, *b* now took a small circuit, all the while in rapid motion, while the movements had previously been slow and more amoeba-like. It returned, however, to *a* and vibrated round it, as though linked to the spot. After having observed this stage for some time, I was interrupted by other observations (see below).

When I returned I found the organism Fig. 8, whilst *a* had apparently disappeared, it had probably again been enclosed by *b*. A stalk was also visible by which the organism was affixed to the glass slide. Whether this stalk had existed before under *a* (in Figs. 1—6) without being seen, it is difficult to say, but it is hardly probable. The individual was vibrating rapidly on its stalk by means of the cilium *p*.

I had not observed it for very long before it began to divide (Fig. 9), *b* being, as it were, skinned off from *a* by reversing. The *a* in this figure had a striking resemblance to *a* in Figs. 1—6, it had the samme clear, refractive and sharply defined contents, in which three small globules or grains were visible. After a while *b* had entirely separated from *a* except only for a slender thread (Fig. 10). At the same time another immobile thread or tail (*h*) was seen adhering to *b* besides the cilium.

The thread fixing *b* to *a* was stretched and lengthened (Fig. 11)[1], *b* moving rapidly towards different sides; but then the thread was again shortened, and *b* enclosed *a* (Fig. 12), as it were, for a last intimate embrace, before finally parting. *a* was now entirely enclosed in *b* (Figs. 13—16; Fig. 15 drawn from above, and the stalk (*s*) not visible) and at last it could not be distinguished any more; apparently it dissolved in the protoplasm of *b*. In certain attitudes two threads (Figs. 16 and 17, *h* and *h'*) could now be seen attached to it; one of which may later have again disappeared. A longish body now became visible in the interior of the cell (Figs. 17—20, *k*). This body became more distinct and gradually changed somewhat in shape (Pl. V, Figs. 17—20, *k*, Pl. VI, Fig. 1, *k*) at the same time as the organism changed its outer form. Suddenly *a* reappeared inside the cell, and *b* again began to separate from it (Pl. VI, Fig. 2); soon after to leave it finally (Fig. 3). At the same time a drop-like protuberance was formed on the side of *a* (Fig. 2 *d*). I believed at first that it was simply a viscous drop, as a similar globule was

[1] The figures Pl. V, Fig. 11—Pl. VI, Fig. 10 are drawn with smaller magnification (Zeiss obj. *F*, oc. 2) than Pl. V, Figs. 1—10 and Pl. VI, Figs. 11—19 (Zeiss obj *F*, oc. 4).

seen close by (Fig. 2, *x*), but I now rather think that it was the first beginning of the bud-like formation which was further developed later (Figs. 13—19).

I followed *b* (Fig. 4) on its wanderings for some time. It moved forwards by means of the cilium (*p*) and dragged the tail (*h*) after itself, exactly in the same way as the organisms in Pl. V, Fig. 7. It gradually changed its shape (Figs. 4—9). The longish body or nucleus inside, which was at first situated athwart it (Figs. 4—6) in the posterior end, gradually took a more longitudinal position (Figs. 7—6) and the organism itself became more oblong. At the same time the "nucleus" began to narrow in the middle (Figs. 6—7) just as though a division were being prepared, but it came to nothing during the time of observation. At last it became fixed to the glass slide by the tail or stalk (*h*). The motion which had hitherto been rather rapidly vibrating, now became quieter, and at the same time, the exterior shape of the body became more spheroidal (Fig. 10). I observed this stage for a long time, but no changes were seen. At last I had to break off for dinner. When I came back dew had formed on the cover-glass, and when this had been removed I sought in vain for the organism Fig. 10. I then returned to *a* in Fig. 3, which had remained almost unaltered all the time (Fig. 11)[1]. The two corpuscles or grains in Fig. 3, *a* had now united into one (Figs. 11, *k*), and this globule which I called "nucleus", moved slowly towards the end where the stalk adhered (Fig. 12). The bud-like protuberance (*d*) remained in position but gradually increased in size (Fig. 12). Near the "nucleus" (*k*) one or sometimes two grains were seen. In the evening (Fig. 13) a drop was being secreted from the cell. The protuberance, *d*, now gradually increased in size very slowly (Figs. 13—19. In fig. 15, the body is turned over so that *d* is seen on the under side, whilst in Fig. 12, it was on the upper side). In the meantime the "nucleus" underwent several changes. In Fig. 16 it had ejected a grain (*x*) which moved towards the protuberance, but again returned to the "nucleus" (Fig. 17). In Fig. 17 the "nucleus" was lengthened and constricted, and a grain or "nucleolus" was thus given off (Fig. 18, β. The grain β is somewhat too small in this figure). After this, two grains were always

[1] This and the following figure (Figs. 11—12) are drawn under much greater magnification (about 1800 diameters, Zeiss *F*, 4. and long tube) than Figs. 1—10 (Zeiss *F*, 2; magnified 760 diameters) Fig. 11 was drawn with the aid of the camera lucida. Figs. 13—19 were drawn somewhat smaller magnification (Zeiss *F*, 4, and medium long tube, about 1500 or 1600 diameters), Figs. 13 and 14 with the aid of the camera lucida.

seen, one on each side of the "nucleus" (Figs. 18—19), β being the larger. Other small dark grains (Fig. 18, α) were seen moving slowly round in the cell, and keeping near to the membrane.

The observation was now interrupted by an accident.

While I was studying the above organism, the organism Pl. V, Fig. 7, *A* came into the microscopic field. Thinking it was the same kind of organism in an earlier stage I followed it for some time. It moved forwards by means of the cilium *p*, and dragged a long thread or tail (*h*) after it, which was perhaps destined to become a stalk. For some time no apparent changes took place. The anterior part, somewhat resembling Fig. 1, *a*, was very mobile towards both sides (see Fig. 7, *B*). I saw also another individual of the same kind, but in this case the anterior part was still more loosely connected with the posterior part; and it was still more mobile. At last this organism disappeared entirely from sight between some diatoms. While hunting for it I also came across another similar individual which was, however, less elongated (Fig. 7, *C*), and with no indication of anterior or posterior parts. In the interior two corpuscles or possibly nuclei(?) were, however, visible.

SUCTORIA gen.? sp.

Pl. VIII, Figs. 1—3.

When this organism was first seen under the microscope, in the morning of July 30, 1894, it was spheroidal tentacles like Pl. VIII, Fig. 2 and with radiating filiform tentacles. A nucleus(?) in the centre and a great vacuole (v_1) on one side were visible. While I was observing it, it ejected at its left hand side a part of its grained cellular contents (see Fig. 1). As, however, the water began to diminish under the cover-glass, a fresh drop was added; at the same instant the individual contracted energically and got the appearance of Fig. 1. I could not decide whether this contraction was due to the new water, or to ejection of the cellular contents. After a while it again assumed its regular spheroidal form (Fig. 2). During the contraction of the animal the great vacuole, v^1, was partly divided into three vacuoles; a similar division sometimes happened, when the vacuole contracted while the body had the regular form. After some hours another vacuole (v^2) also appeared on the other side of the nucleus. It may possibly have been there before, as it was

on the under side of the body; but I had not seen it. At the same time the first vacuole (situated on the upper side of the body, nearest the eye) was considerably reduced. The contractions of the vacuoles occurred curred at long intervals. The *tentacula* were slender, straight, apparently rigid, and ending in a slight knob. I did not see them move, but there was occasionally a slight vibration in them.

The interior of the body was filled with big refractive globules (Fig. 3, *b*) which may be reserve nourishment. They do not approach close to the surface. In the outer layer a great mumber of small grains occur (Fig. 3, *a*) which have a rapid, half oscillating movement, while swimming about. What I assumed to be the nucleus, was situated near the upper side of the body, and there were i Fig. 2 no refractive globules but only small oscillating grains between it and the outer membrane or surface. It appeared as an oblong granular mass. The granular appearance might have been due to threads.

The *colour* of the organism was slightly yellowish green, which seemed to be due to the refractive globules (Fig. 3, *b*).

Length of individual about 0·095 mm.

I kept this organism under observation during the whole day (July 30) till interrupted by a bear in the evening.

When I came back after a few hours, the water had been much reduced by evaporation, and the organism had become much contracted on the one side. When fresh water was added it again expanded, and then I left it for the night.

The next morning, July 31, 1894, it had about the same appearance, but was slightly contracted, and somewhat reduced in size. Later on in the day, while I was following the developement of the organism illustrated on Pls. V—VI which was in the same preparation, the cover-glass was several times touched by the microscope, and great masses of the cell contents were twice squeezed out. The first time the organism was much contracted and of irregular shape (like Fig. 1) but after a while it returned to its regular, spheroidal form. After the second time I could observe no motion in the organism. The big globules (Fig. 3, *b*) which had been spueezed out into the surrounding water appeared at first to have a nearly homogeneous structure, but the contents of rome began in the evening to be differentiated into one or two "nuclei" and a granular mass (Pl. VI. Fig. 20, *c*). Some of them also began to

show a slightly vibrating movement, see Pl. VI, Fig. 20, *b* and *c*. Whether *a* (Fig. 20) had also originated from the above organism, I could not say for cerlain; it was lying between some of these globules and had the same appearance, but gradually began to move away by means of its cilium.

The small grains (Fig. 3, *a*) which had been squeezed out with the cell contents, appeared to have grown in size during the afternoon, and were darker than the bigger globules. I did not directly see, that any of them began to move, but they had a striking resemblance to many small dark globules which were seen vibrating between the greater globules.

Pl. VI, Figs. 20, *d*, was a body which was in rapid vibration near some of these globules.

GLOBULAR ALGA.

Pl. VIII, Figs. 7–8.

I have mentioned above (p. 7) that, in July and first days of August, 1894, numerous globular lumps of algæ were floating in the channels between the ice-floes, at a depth of about 1 metre; somewhat deeper in August, perhaps nearly 2 metres. These lumps were composed of diatoms, and to a large extent, also of a yellowish-red globular alga, of considerable size. This alga, when alive, gave the floating lumps a reddish colour, and the colour was the deeper the more numerous were the algæ.

The diameter of this alga (Pl. VIII, Fig. 7) was generally between 0·10 and 0·16 mm.

Its cell contents were surrounded by a layer of yellowish red refractive globules (see Fid. 7), situated near the membrane. These globules varied somewhat in size. They gave the alga its reddish colour, and were formed by an oily substance, which was blackened by osmic acid. While Fig. 7 was being drawn, and when the alga probably began to die, the oily globules united and became continually larger until they fused into one mass which left the cell as big reddish drops; the cell-contents then became entirely colourless. These globules, were as already mentioned, dispersed in the outer layer of the cell-contents, while its interior was transparent and colourless. The red globules were, however, as a rule lying so close together, that it was impossible to see through them into the interior of

the cell. Only at one place on the upper side of the cell, near x in Fig. 7, was there an opening not covered by the globules where it was possible to see into the interior. Here a small oblong body (x) was seen. When the under side of the cell came into focus the red globules were again seen, as indicated by the grey dots in the figure. Whether the open space (at x) was a vacuole, could not be decided, as no movement was observed in it.

The cell was surrounded by a membrane.

No *nucleus* could be seen in the living specimens, as the oily globules covered the interior from sight. It seems hardly probably that the open space at x, could indicate the situation of the nucleus, as it was not defined by any membrane.

In a specimen killed with chromo-aceto-osmic acid, and stained with para-carmine an oblong nucleus, about 0·037 mm. long, was seen near the outer membrane, on one side side of the cell.

Forms of this alga like Fig. 8 (Pl. VIII), with a furrow on one side, were often seen. The structure was the same as in Fig. 7, but the oily globules were more closely situated, and made the cell still less transparent. The globules near the furrow were colourless in this specimen. The diameter of the cell was 0·11 mm. It is possible that this form with the furrow has been produced as the result of violence from outside.

I got no time for studying the life of this alga, and can therefore say nothing about its development. or process of multiplication.

PL. I.

Fig. 1. *Lacrymaria* sp. Living individual, from a lump of algæ floating in a channel between the ice-floes. July 20, 1894. Transparent and colourless. Magnified about 760 diameters (Zeiss obj. F, oc. 2; cam. luc.).

— 2. *Lacrymaria* sp. Same individual as Fig. 1; while moving. Less magnified (Zeiss obj. F, oc. 2; freehand sketch).

— 3. *Lacrymaria* sp. Rough sketch of moving individual; from same place as above. July 20, 1894. The globules of the cell-contents are in the figure only indicated in the anterior part (Zeiss obj. F, oc. 2; freehand).

— 4 and 5. *Lacrymaria* sp. An individual of the same species as Fig. 3, and probably also, as Fig. 1, but with the spiral-furrows very distinct. Killed with Osmic Acid, stained with Borax-Carmine, and transferred to Canada Balsam. From a pond on the ice. July 24, 1894. In Fig. 4 the specimen is seen somewhat obliquely; the anterior end being lifted slightly upwards. Fig. 5 represents the same specimen turned over on one side and at full length. Magnified about 1150 diameters (Zeiss Hom. Im. $^1/_{18}$, oc. 2; cam. luc.).

— 6 and 7. *Lacrymaria* sp. An individual killed with Osmic Acid (Fig. 7) and afterwards stained with Borax-Carmine (Fig. 6). From a pond on the ice. July 24, 1894. Magnified 760 diameters (Zeiss obj. F, oc. 2; cam. luc.).

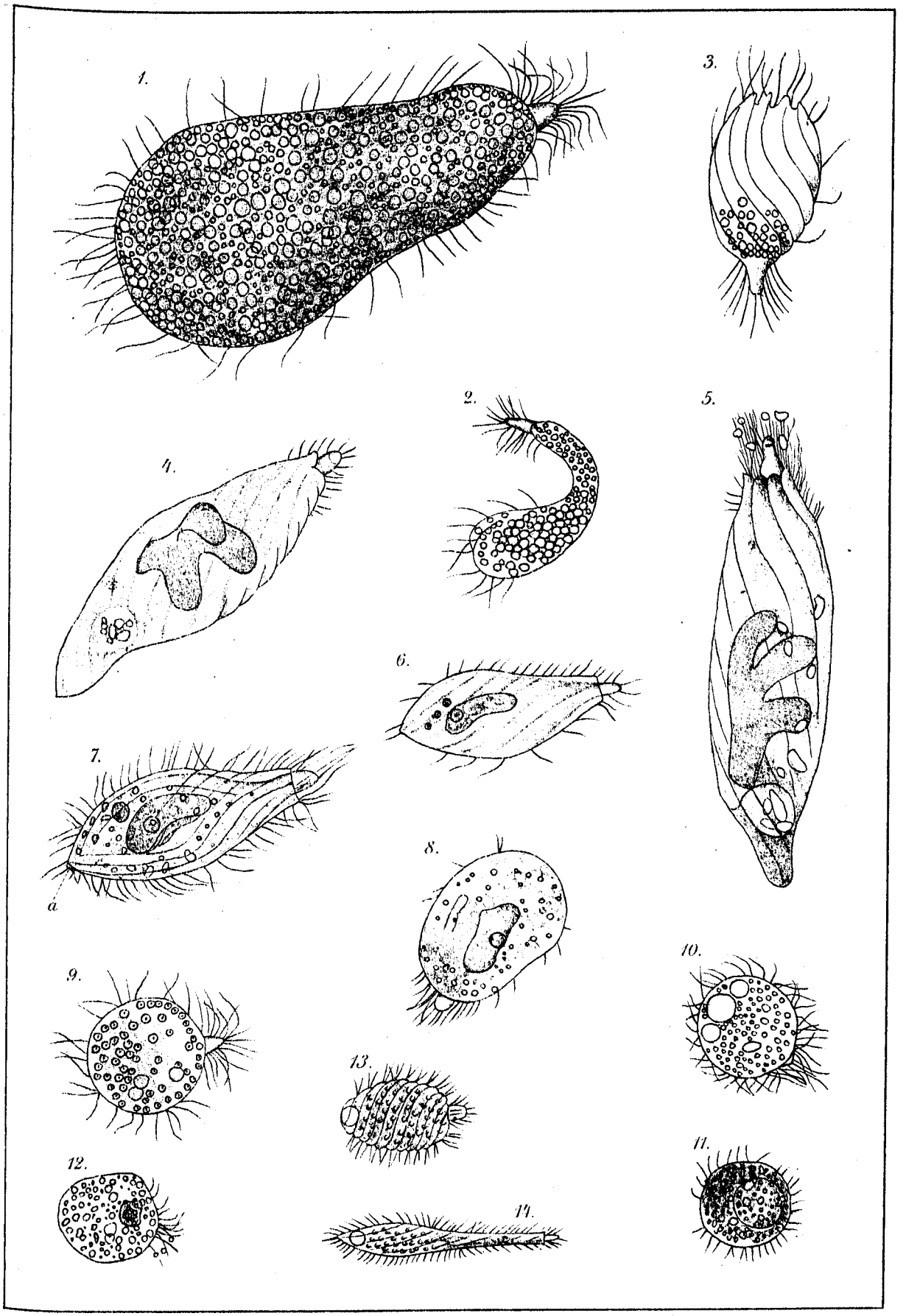
— 8. *Lacrymaria* sp. From the same sample as the previous individual. Killed with Chromo-Aceto-Osmic Acid.

— 9. *Lacrymaria* sp. From the same samples as Figs. 1 and 3. July 20, 1894. Killed with Osmic Acid. Magnified about 760 diameters (Zeiss obj. F, oc. 2; cam. luc.).

— 10. *Lacrymaria* sp. Living specimen, in contracted, motionless state. From a pond on the ice. July 28, 1894. (Zeiss obj. F, oc. 2; freehand sketch).

— 11 and 12. *Lacrymaria* sp. Living specimen, of the same species as above, in contracted motionless state. From a pond on the ice. July 28, 1894. Fig. 12 was drawn 6 hours later than Fig. 11; the individual was then possibly dead; there was no movement of the cilia, and a great many of them had disappeared. Magnified 460 diameters (Zeiss obj. CC, oc. 5; cam. luc.).

— 13 and 14. *Lacrymaria* sp. Living individual, from a pond on the ice. Aug. 2, 1894. Magnified about 500 diameters. Length of animal in Fig. 13 0·044 mm. Freehand sketches; Fig. 13 drawn after Fig. 14.



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PL. II.

Fig. 1. *Chilodon* (?) sp. Living, from a pond on the ice. August 1, 1894. It was just brought under the microscope, but remained motionless, and there was no motion of the cilia. The small corpuseles, *a*, in the protoplasm (ectoplasm) were in rapid oscillation. The animal showed no change of form or structure as long as it remained under observation. Magnified about 1600 diameters (Zeiss obj. F, oc. 4, with lengthened tube, cam. luc.).

— 2. *Chilodon* sp. Living but motionless, from a pond on the ice. August 1, 1894. Probably same species as above; seen from the side. The small grains in the protoplasm were in rapid oscillation. *m*, Mouth. *a*, Anal opening. A vacuole is seen in the posterior end on the dorsal side. The animal was much contracted and probably died while being drawn. The great accumulation of food (?) on the anterior dorsal side was sharply defined as if by an apparent membrane. This accumulation, was externally surrounded by a layer of refractive globules. Apparently dark spots, looking like grains or even like a reticulum (see Fig. 2), were formed by refraction between the globules. These globules formed only a layer in the exterior part of the formation, for when the microscope was focussed on its central part a more homogeneous mass was seen, filling the interior, whilst only a layer of globules was seen along the margin under the apparent "membrane" (see Fig, dorsal side). This accumulation of globules changed after a while. The stripes passing along the animal are possibly contractile fibres. They also seemed to pass towards the place in the posterior end, which is presumably the anal opening. A distinctly defined, tract or belt passed towards this place, and looked as if it might be a canal. Magnified about 1600 diameters (Zeiss obj. F, oc. 4, with lengthened tube, cam. luc.). Length of animal was 0·049 mm.

— 3. *Chilodon* sp. Living but motionless, from a pond on the ice. Aug. 1, 1894. Seen from the right-hand side. The animal was possibly dying, as the body seemed more contracted and rounder than was generally the case. In this individual there were six greater and smaller accumulations of globules of the same kind as the one in the previous figure. (I have also seen similar accumulations in other individuals in the same sample). Even here the layer of refractive globules surrounded a more homogeneous mass. Some similar globules were seen dispersed in the protoplasm of the animal as in Fig. 1, either single or united, some few together. After a time some of the great accumulations of globules began to dissolve and the globules were dispersed singly in the protoplasm, in the same way as usual (Fig. 1). Both in this and the previous specimen only a few oscillating small grains were seen, but the refractive globules were in slow motion. In the anterior end of the animal several longer cilia were seen situated near the margin and on the ventral side. The right margin of the animal is seen as a dark streak or belt along the figure. *m* Oral opening.

Magnified about 1600 diameters (Zeiss F, 4, with lengthened tube, cam. luc.). Length of animal 0·054 mm.

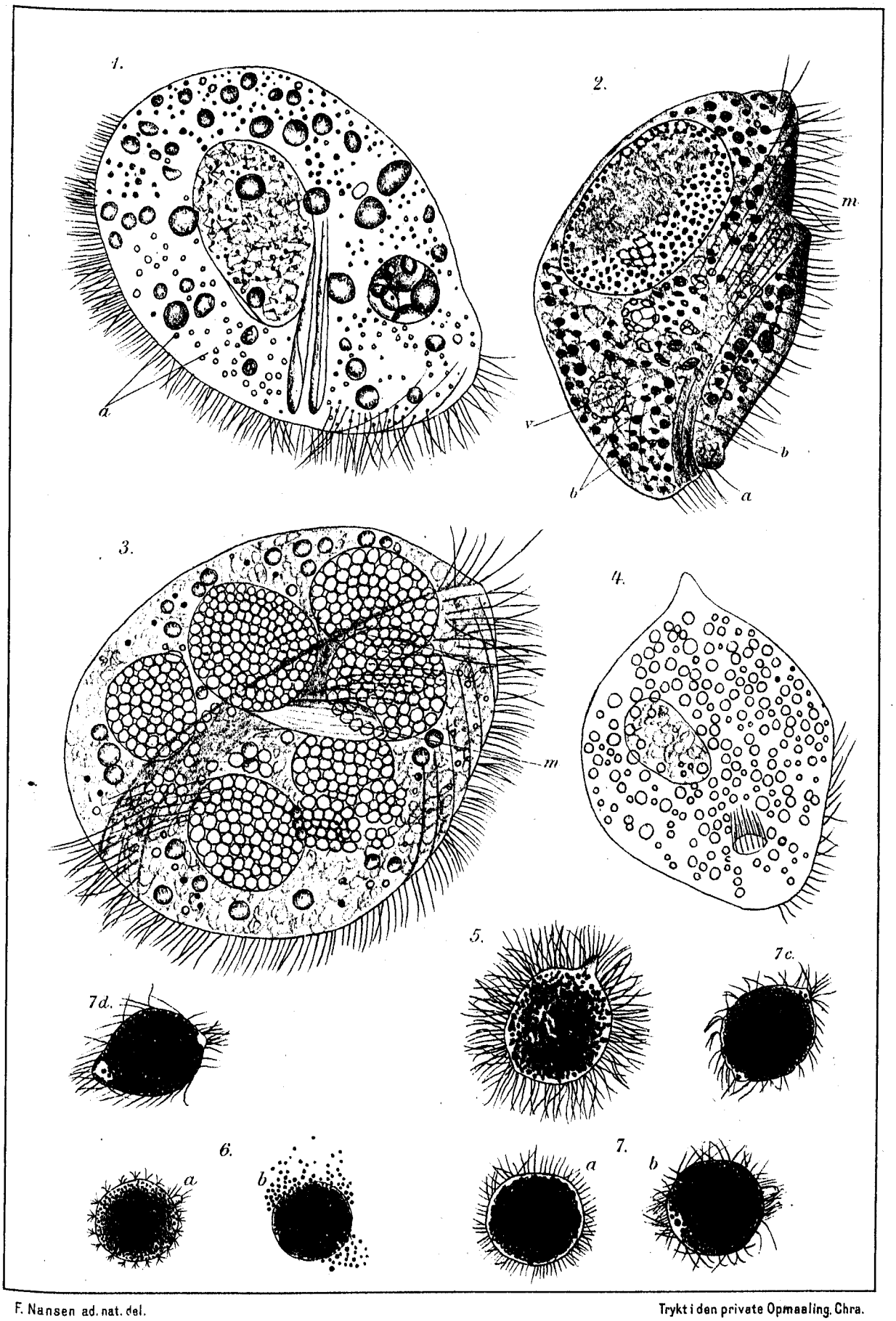
— 4. *Chilodon* sp. Living, from a pond on the ice, Aug. 1, 1894. Probably a species different from the above. This specimen was somewhat more oblong before it was sketched. Its exterior form was much like the above, only that it had a more pointed posterior end, and the oral opening was perhaps somewhat differently situated, being directed more towards the anterior end (?).

This individual is evidently somewhat contracted, perhaps in the act of forming a cyst, as after a while some colourless mucuous substance was secreted from the left side (on the right hand side of the drawing). At this period the cilia seemed to have nearly disappeared. The refractive globules were in active oscillation. Magnified about 760 diameters (Zeiss obj. F, oc. 2, cam. luc.). Length of animal 0·099 mm.

— 5. *Lacrymaria* sp. Living but contracted and motionless. From a pond on the ice, July 24, 1894. Magnified about 760 diameters (Zeiss obj. F, oc. 2, cam. luc.).

— 6, *a* & *b*. *Lacrymaria* sp. Living, but contracted and motionless, only cilia moving in Fig. 6, *a*. From a pond on the ice. July 28, 1894. *b*, some time after *a* with membrane burst, and oscillating grains spread towards all sides. Magnified 460 diameters (Zeiss obj. CC, oc. 5; cam. luc.).

— 7, *a*–*d*. *Lacrymaria* sp. Living, from a pond on the ice. July 28, 1894. The individual was observed during twelve hours from noon (Fig. 7 *a*) till near midnight (Fig. 7 *d*). The cilia were seen moving the whole time, but ceased to move near midnight. Magnified 460 diameters (Zeiss obj. CC, oc. 5; cam. luc.).



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PL. III.

Fig. 1. *Chilodon* (?) sp. Fresh state, probably dead. From a pond on a comparatively clean ice-floe. July 24, 1894. Magnified about 760 diameters (Zeiss obj. F, oc. 2, cam. luc.).

— 2. *Chilodon* sp. Fresh state, probably dead. From another pond on the same ice-floe as above. July 24, 1894. Magnified about 760 diameters. (Zeiss obj. F, oc. 2; cam. luc.).

— 3. *Chilodon* sp. Living. Conjugation of two individuals probably of the same species as above. From the same place as above. July 24, 1894. They were observed in this situation, unaltered for a long while. The vacuoles were the whole time pulsating. They were moving rapidly, being dragged along by the one individual. At last they died. (Zeiss F, 2, freehand).

— 4. *Chilodon* sp. Killed with Chromo-Aceto-Osmic-Acid. From same place as above. July 24, 1894. *v* Vacuoles. Magnified 760 diameters. (Zeiss obj. F, oc. 2; cam. luc.).

— 5. *Chilodon* sp. Fresh state, probably dead. From the same ice-floe as above. July 24, 1894. Individual of the same species as above seen from the end. The pointed (anterior) end is near the margin at *a*, and would have become visible, outside the margin, by a slight turn of the animal. The double series of cilia wind round the body and meet at its end, at *c*. Magnified about 760 diameters. (Zeiss F, 2; cam. luc.).

— 6. *Chilodon* sp. Fresh state, just dead. From the same sample as Fig. 3. Magnified about 760 diameters. (Zeiss obj. F, oc. 2; cam. luc.).

— 7. *Chilodon* sp. Killed with Chromo-Aceto-Osmic-Acid, stained with Borax-Carmine. From pond on the ice. July 27, 1894. Same species as Figs. 1—6, the specimen is seen from the anterior pointed end which is at *a*. *v* Vacuoles, a third vacuole is seen on the other side. The oral opening was very distinct. Magnified 1000 diameters. (Zeiss F, 3; cam. luc.).

— 8. *Stylonychia* (?) sp. This is probably a cyst of the same form as Pl. IV, Fig. 1. It was quite motionless. Thick stiff hairs at both ends.
Magnified 460 diameters (Zeiss obj. CC, oc. 5; cam. luc.).

— 9. *Chilodon* (?) sp. Living, in active motion. From a pond on the ice, seen with specimens of the dark and the transparent forms of Lacrymaria. July 28, 1894. Magnified about 460 diameters (Zeiss obj. CC, oc. 5, freehand sketch).

— 10. Living, in active motion. From a pond on the ice. July 28, 1894. Small animals of this kind were often seen amongst the algæ from the ponds on the ice. It is possibly the same species as Fig. 12; but as far as could be seen the cilia were chiefly situated on the one side only. They had a twitching and one-sided motion. Half an hour later all movement had ceased, and two colourless water-clear refractive drops were secreted from each side (Fig. 11). During this process the animal was reduced to a small globule (Fig. 11); its protoplasm became less transparent and the grains closer together.
Magnified perhaps about 460 diameters (Zeiss obj. CC, oc. 5; freehand sketch).

— 11. Same individual as Fig. 10. (Zeiss CC, 5; freehand sketch).

— 12. Living, in active motion. From a pond on the ice together with specimens of *Chilodon*. July 28, 1894. Animals of this small kind were often observed. They were always as small as this individual. They seemed to be round in transverse section and covered with cilia all over. Magnified about 460 diameters. (Zeiss obj. CC, oc. 5; freehand sketch.

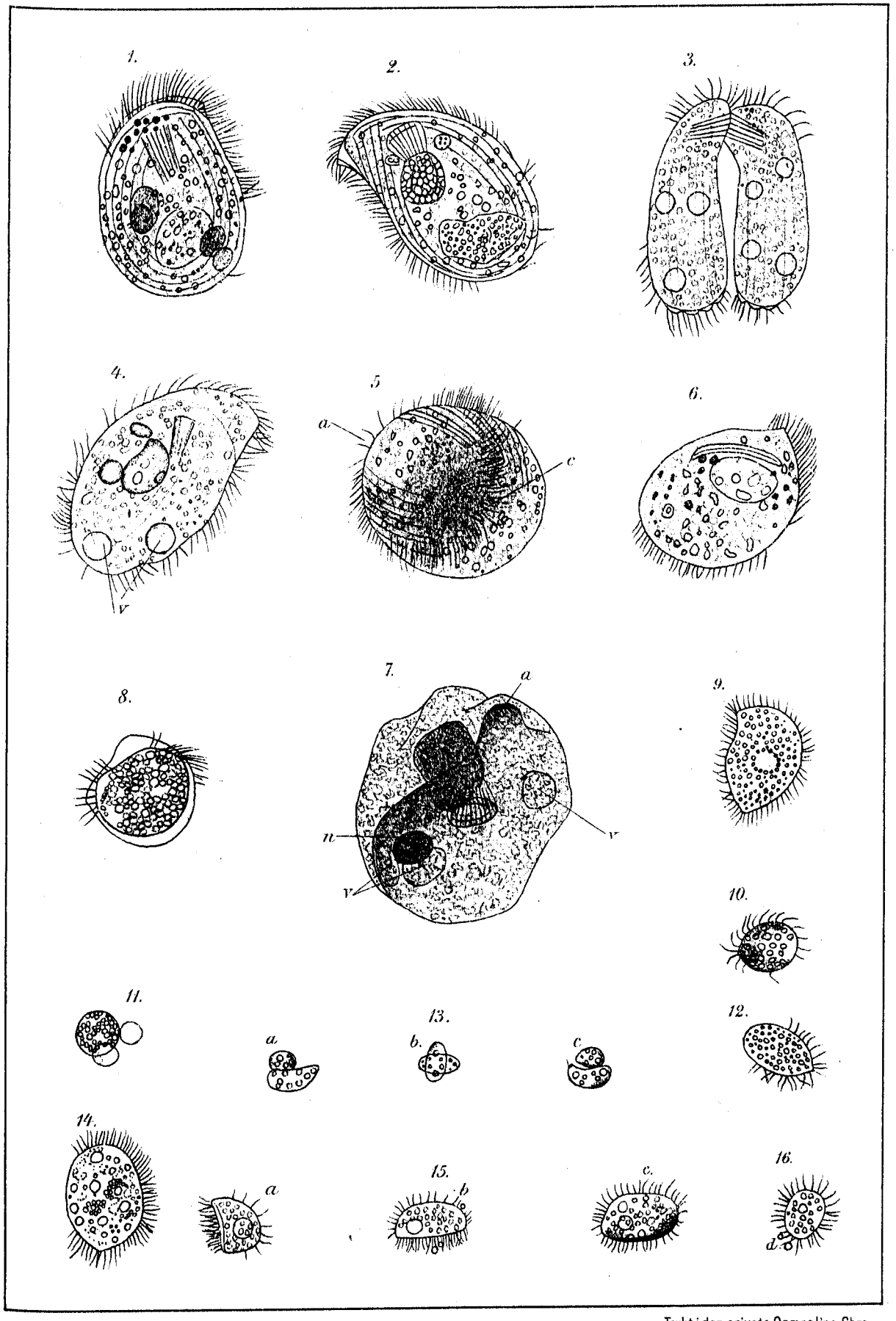
Fig. 13. *a—c*. Living. From a pond on the ice. July 29, 1894. Conjugation (?) of small oscillating infusoria, of a similar appearance as Figs. 15, but they were smaller so that no cilia could be distinguished. They united during rapid oscillating movements, the one athwart the other, with the flat ventral (?) sides against each other. Thus united they walzed rapidly around for a while; then the movements ceased, and the one assumed a rounder shape in longitudinal aspect (see Fig. 13 *c*, the upper individual). *a* and *c* are seen from the side, *b* from above (Zeiss obj. CC, oc. 5; freehand sketches).

— 14. *Chilodon* (?) sp. Living, but motionless. From a pond on the ice. July 28, 1894. Perhaps the same or nearly allied species as Figs. 1—6 (?).

Magnified 460 diameters. (Zeiss obj. CC, oc. 5; cam. luc.).

— 15, *a—c*, and 16. Living. From a pond on the ice. July 29, 1894. An individual seen in different positions: *a* obliquely from behind, *b* from the side, and *c* from above. This figure should be more regularly oval than it is here reproduced. The animal had apparently a flat ventral side, and a vaulted dorsal side. Ventral side densely covered with long cilia; on the dorsal side shorter cilia were also visible. One apparent vacuale was visible. At first the animal was in active motion, affixed, as it seemed, by some mucuous threads or cilia to the silica spicule of a diatom, and it could therefore advance only slowly. After some time the movement ceased gradually, and its shape become more rounded (Fig. 16). Several refractive drops were then secreted (Fig. 16, *d*), and its size was reduced. After a while the motion became again more rapid, and during its rotations it was now seen that the flatness on the ventral side had nearly disappeared and it had got a more oval or nearly globular shape.

Magnified 460 diameters (Zeiss obj. CC, oc. 5; cam. luc.).



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PL. IV.

Fig. 1. *Stylonychia* (?) sp. Living, but motionless. From a lump of algæ floating in a channel between the floes about 2 metres deep. August 4, 1894. The small corpuscles inside the animal oscillated rapidly. During the drawing of it the animal contracted, and the notch at the anterior end (left hand side of the drawing), disappeared almost entirely. Then it again expanded and got the same appearance as before, only the caudal bristle, pointing straight backwards in Fig. 1, had now got a spiral winding as in Fig. 1, A. This twisting may possibly be due to the drop of Osmic Acid which was added to the preparation a couple of hours before.

Magnified 1280 diameters (Zeiss obj. F, 4; cam. luc.).

— 2. *Stylonychia* (?) sp. Killed with Osmic Acid. From the same place as above. August 4, 1894. The same species as above seen from the side.

(Zeiss obj. F, oc. 4; cam. luc.).

— 3. *Stylonychia* (?) sp. Living. From a pond on the ice. Taken July 28, 1894, and kept alive in a bottle till August 6, 1894 when it was sketched. Same species as above. *a* seems to be small accumulations of food (small algæ). *b* is a small vacuole just under the outer membrane; the small refractive grains were in a strikingly rapid motion inside this vacuole. No longitudinal furrows could be seen on this side of the body. v_1 and v_2 big, clear vacuoles, but no pulsation could be observed. A third vacuole could also be seen inside v_3, when the animal moved, but it was not visible in this position; it seemed to be situated more on the other side of the body. It was of about the same size as v_2. While the animal was being drawn, the thick bristles at *c* dissolved, as far as could be seen into numerous slender threads or hairs. The two thick bristles at *d*, which at first were undivided and pointed, now dissolved near the end into several threads as is seen in the figure, and was finally transformed into a bundle of threads or hairs. The thick bristles near *a* and v_2 gradually disappeared, and this was also the case with the hairs or cilia near the margin between *e* and v_3. At *e* and on the right hand side of v_3 some dilapidated remains of hairs remained. While this was going on the animal contracted somewhat in length, and assumed a rounder shape; simultaneously two colourless water-clear drops were secreted, one in the posterior end at v_1 and one opposite v_2, on the right hand side of the drawing. The vacuole v_2 had in the mean time disappeared, and so had the small vacuole *b*, or it had been much widened, for many of the small refractive grains were seen oscillating and dispersed over a wider area.

The animal is transparent and colourless with refractive, greenish globules dispersed chiefly in the exoplasm as usual.

(Zeiss obj. F, oc. 4; cam. luc.).

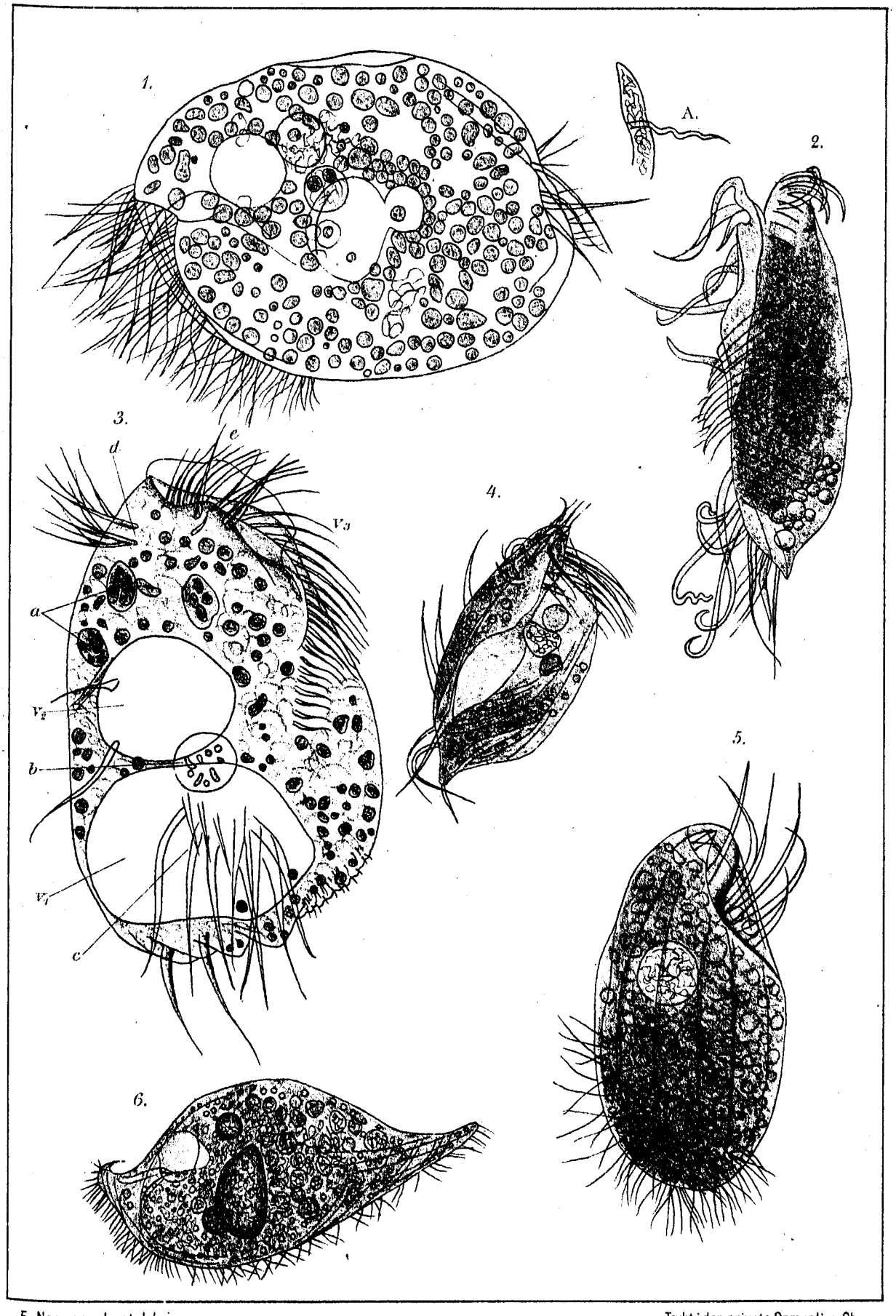
4. *Stylonychia* (?) sp. Same species as above, and from same place as Fig. 1 Aug. 4, 1894. Killed with Osmic Acid.

Magnified about 1280 diameters (Zeiss obj. F, oc. 2; lengthened tube; cam. luc. Length of specimen 0·040 mm.

— 5. *Stylonychia* (?) sp. Same species as above, and from same place. Aug. 4, 1894. Killed with Osmic Acid.

Length of specimen 0·038 mm. (Zeiss obj. F, oc. 1, cam. luc.).

— 6. *Chilodon* (?) sp. Probably a species similar to Pl. II, Fig. 4. From a lump of algæ floating nearly 2 metres deep in a channel between the ice-floes. Killed with Osmic Acid. Length of specimen 0·1088 mm. (Zeiss obj. CC, oc. 5; lengthened tube; cam. luc.).



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PL. V.

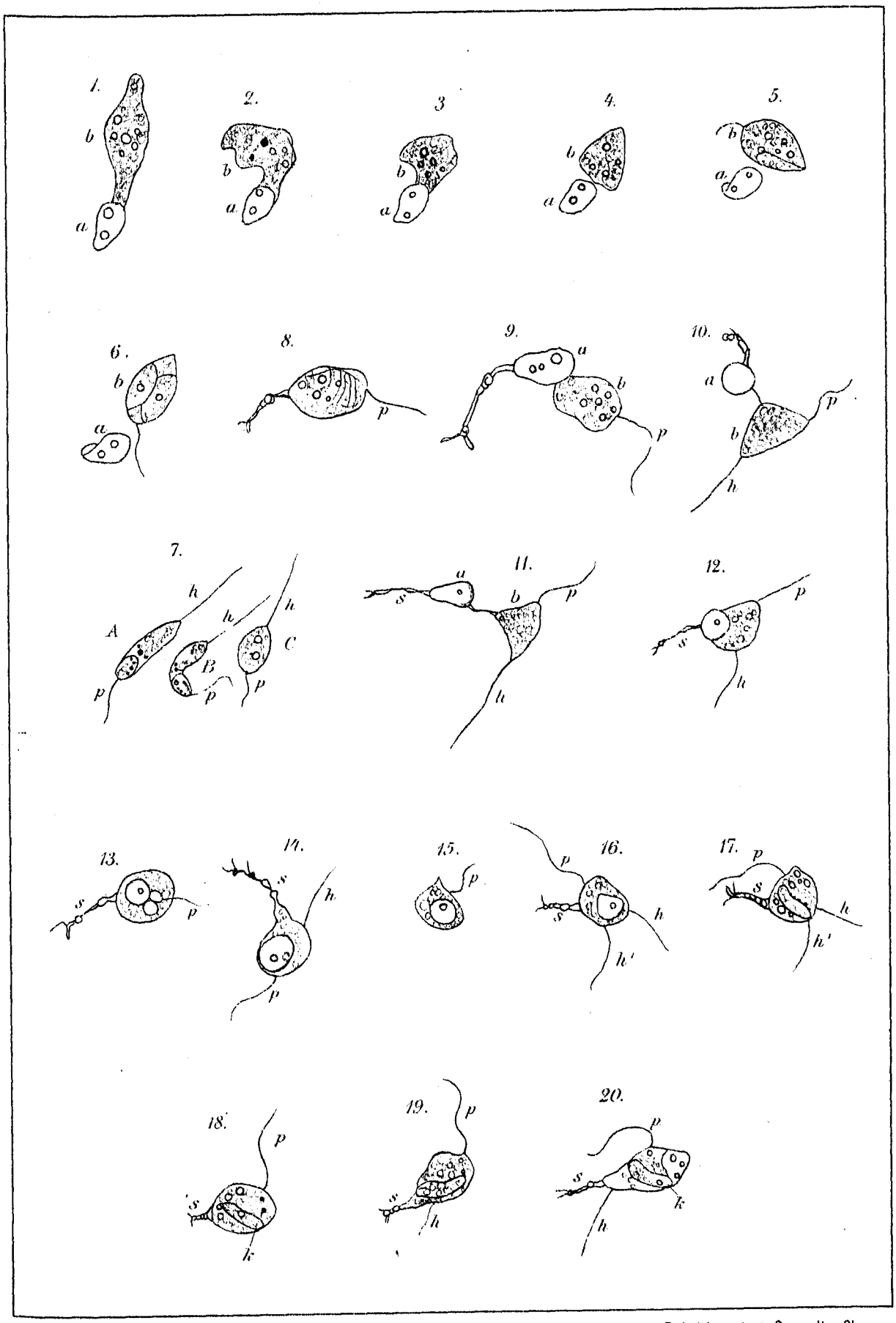
All figures with exception of Fig. 7 represent the same individual in different stages. From a pond on the ice, taken July 30, 1894.

For description of figures see pp. 16—19.

Figs. 1—6. Living. Freehand sketches drawn with Zeiss obj. F, oc. 4, on July 31, 1984 morning.

Fig. 7, A—C. Living. Two similar organisms (A and B is the same individual). Freehand sketches, Zeiss obj. CC, oc. 5. July 31, 1894, morning.

Fgs. 11—20. Living. Zeiss obj. F, oc. 2; freehand sketches. July 31, before and after noon.



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PL. VI.

All figures, with exception of Fig. 20, represent the same individual as Pl. V.
For description of figures see pp. 16–19.

Figs. 1–10. Living. Zeiss obj. F, oc. 2, freehand sketches. July 31, 1894, before dinner.

Fig. 11. Living. Magnified about 1800 diameters. Zeiss obj. F, oc. 4, long tube, cam. luc. July 31, 1894, 6 p. m.

— 12. Living. Same magnification as Fig. 11, freehand sketch.

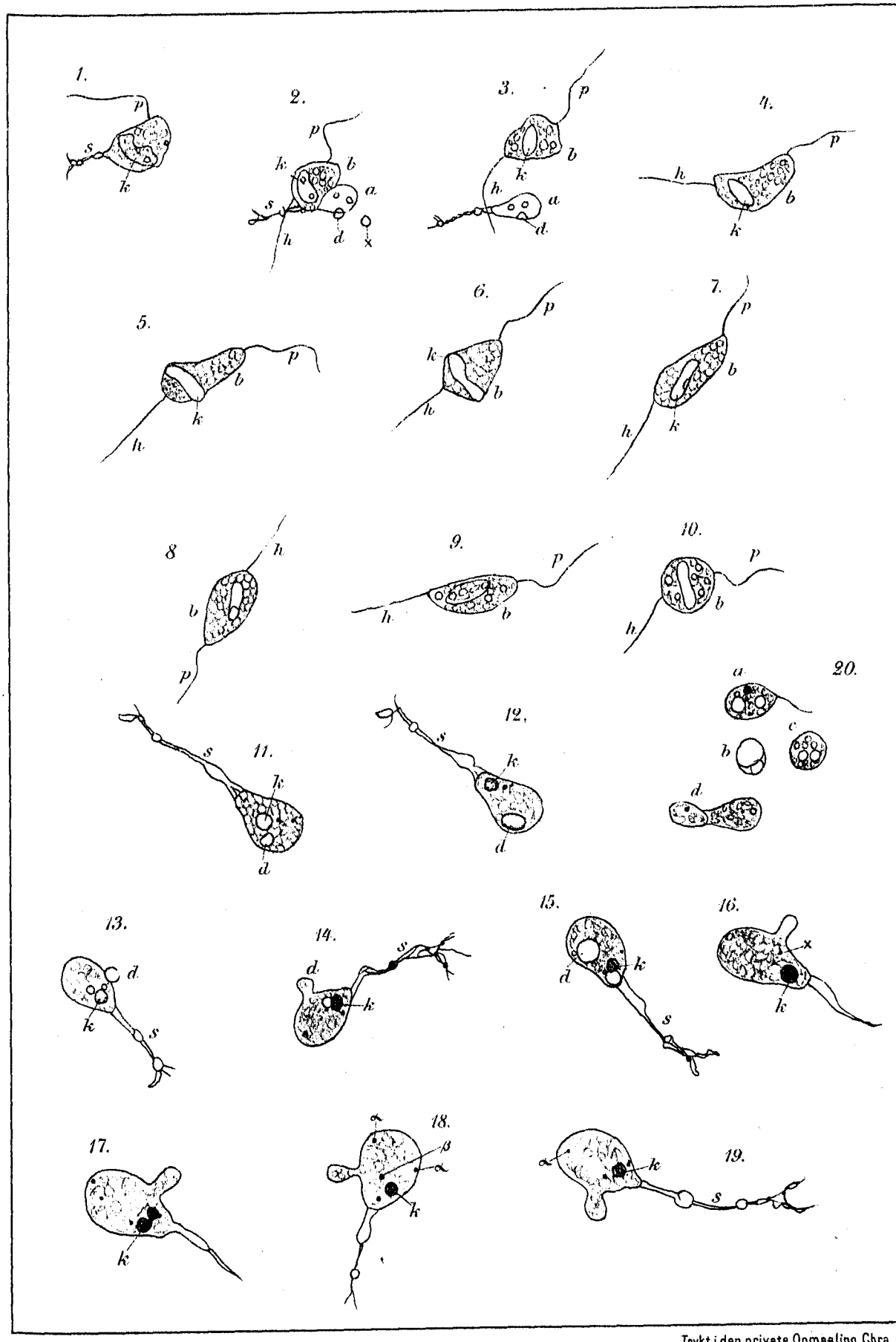
— 13. Living. Magnified about 1500 or 1600 diameters. Zeiss obj. F, oc. 4, cam. luc. but tube somewhat shorter than in Fig. 11. July 31, 1894, 9 p. m.

— 14. Same magnification as Fig. 13, and cam. luc. at 9·15 p. m.

— 15. Freehand sketch. Same magnification. The organism is so turned that *d* has come on the under side of the body, and is seen through it. 9·30 p. m.

Figs. 16–19. Freehand sketches. Same magnification. (Zeiss obj. F, oc. 4). July 31, 1894, night.

Fig. 20, *a–d*. Formations seen while studying the organism Pl. VIII, Figs. 1–3, amongst its cell contents, squeezed out. (Zeiss obj. F, oc. 4; freehand sketch).



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PL. VII.

Fig. 1 *a—c*. Small flagellatum-like organisms (or germs?) of this kind were very common in most preparations taken from the accumulations of algæ in the ponds on the ice. The three figures represent the same individual in different positions, *c* is less magnified (760 diameters) than *a* and *b* (1280 diameters). It is from water taken on July 28, 1894, and kept for six days in a bottle in the laboratory. The refractive small globules in the interior, were generally situated near the one end, but in other, and greater individuals, they seemed to be more uniformly dispersed. Most of the globules were yellow, whilst some were reddish brown (see the greater, darker grain in Fig. 1, *a*). × is a protuberance on the side of the body. In another individual a similar protuberance was situated in the opposite end of the cilium.

The individual here represented was at first more elongated and oval, but when the movement began to cease it became more spheroidal as in the figures. The length of the specimen without the cilium was 0·0139 mm.

Fig. 1 *a* and *b*, Zeiss obj. F, oc. 4; cam. luc. Fig. 1, *c*, Zeiss obj. F, oc. 2; cam. luc.

— 2. Similar organism from a half percent solution of Sodium Chloride made by Dr. Blessing with water from a pond on the ice, and kept in his cabine for several days. Killed with Osmic Acid. Aug. 3, 1894. Length of body was 0·0087 mm. (Zeiss F, 4, medium long tube, cam. luc.). Other individuals were round, but had the same appearance and similar yellow or brownish globules inside.

— 3. Fresh state. Aug. 2, 1894. Bodies similar to this, of the same size and appearance, often observed in preparations of algæ from the pond on the ice. They were sometimes hexagonal like this, and sometimes approached the pentagon. They had a yellowish colour. While being drawn, it changed in shape to some extent, the hexagon became less regular, the two sides were curved inwards, and more corners were formed (*a*). Magnified 1280 diameters. (Zeiss obj. F, oc. 4, cam. luc.).

— 4, *a—c*. Living. Aug. 2, 1894. A great number of organisms of this kind were seen in all preparations taken from the same bottle as Fig. 1, (taken July 28, 1894). They are possibly individuals of the same kind as those illustrated Pls. V and VI. The appearance was much the same, they were perfectly colourless, and there was no distinct structure in the interior. The tail or stalk on all of them was, however, fixed to the side of the body (Fig. 4, *a* and *c*) and not to the end, opposite the cillium. Some had a more elongated body like Pl. VI, Fig. 7. only the anterior end, at the cilium, being more pointed (Fig. 4, *a*). Fig. 4, *b* is the same individual as *a*, drawn a little later; it was not possible to decide whether × had been developed as a protuberance or bud from the body, or had been fixed to it from outside; it was suddenly seen at the side of the organism. At the same time the vibrating movement ceased. It is not known whether the cilium then disappeared, but it could not be seen any more. At this moment the individual was unfortunately carried away by currents in the water, and the observation terminated.

Fig. 4, *a* and *b* magnified about 1200 diameters (Zeiss F, 4, freehand sketch) and *c* 760 diameters (Zeiss obj. F, oc. 2. freehand sketch).

The following organisms (Figs. 5—12) were all of them seen alive in preparations from the same water as Fig. 2, (a half percent solution of Sodium Chloride made by Dr. Blessing). The figures are rough freehand sketches made with the same magnifications (about 1400 diameters, Zeiss obj. F, oc. 2, long tube).

Fig. 5, *a*, *b*. A very refractive, almost structureless body, with no movement. *a* represents a later stage than *b*. The bud-like protuberance on the side was formed during the observation.

— 6, *a*, *b*. Very small bluish organism, fixed to the glass at the end opposite the cilium. *b* was later than *a*.

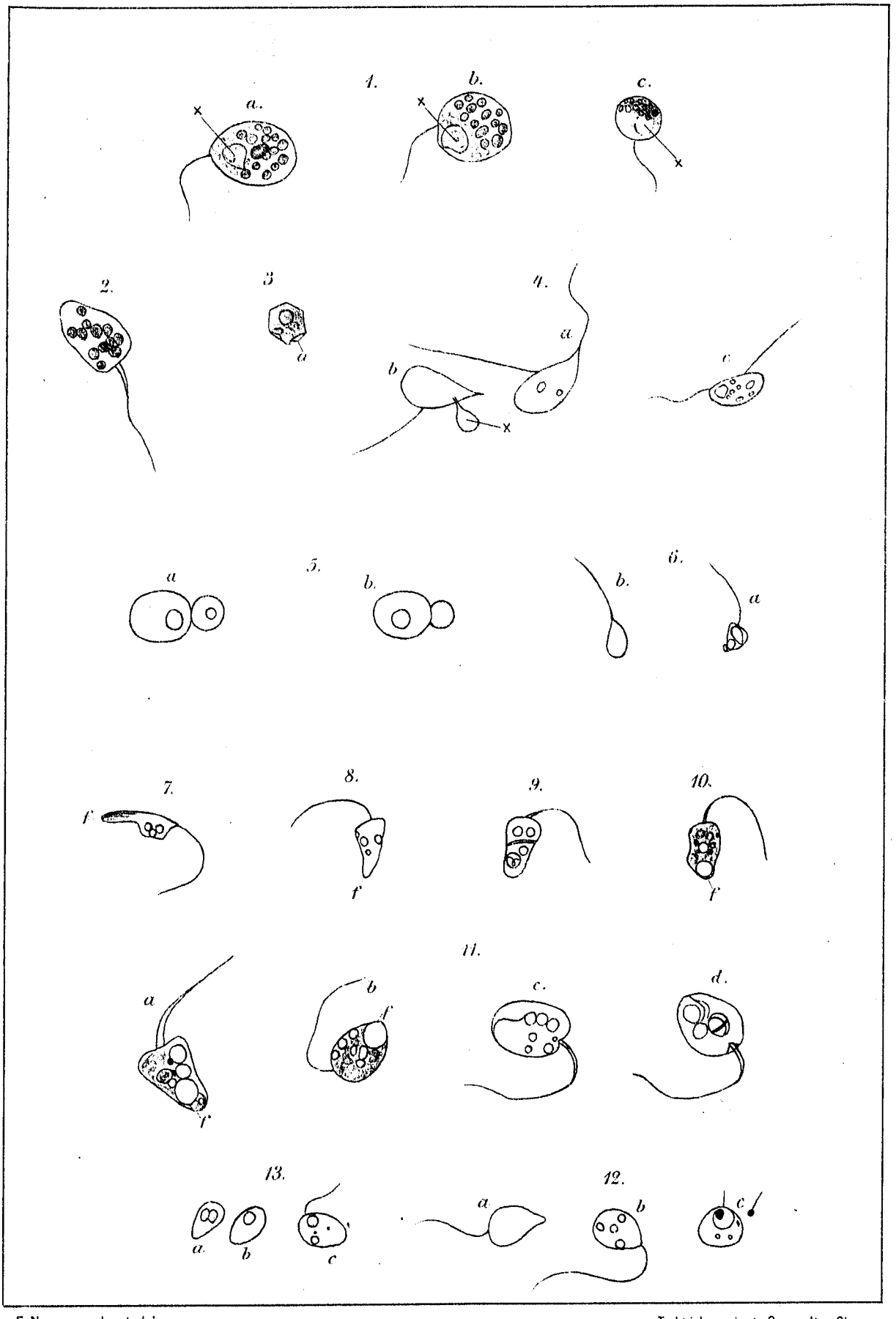
— 7. Very small organism fixed to the glass in the pointed end (*f*).

— 8. Came swimming, and fixed to the glass at *f*. This individual was much smaller than the following.

Figs. 9—11, and 13. Represent the same individual, but the order is wrong. Fig. 13 *a*—*c* was first drawn, and shows the individual as seen from different sides. The other sketches of this individual was drawn in the following order: Fig. 11, *d*, *c*, *b*, *a*, Fig. 10 and Fig. 9. The individual was the whole time fixed to the glass at the pointed end (*f*).

Length of body without cilium, 0·007 mm.

Fig. 12, *a*—c. An individual which was at first fixed (Fig. 12 *c* and *b*); but then loosened itself and swam away (Fig. 12, *a*).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PL. VIII.

Figs. 1—2. *Suctoria* gen. sp. Alive. For description see pp. 19—21. From a pond on the ice, July 30, 1894. Magnified about 760 diameters (Zeiss obj. F, oc. 2; cam. luc.).

Fig. 3. Margin of the same individual. Magnified 1280 diameters (Zeiss obj. F, oc. 4; cam. luc.).

— 4. From a pond on the ice, July 24, 1894. Killed with Osmic Acid, stained with Borax Carmine. Organisms of this kind were often seen in different preparations of algæ from the pond on the ice. They were never seen moving. The whole surface of the organisms was densely covered with sand-grains, some of which were long and very refractive, looked like silica spicules of diatoms. In this figure only those grains are drawn which were visible in one microspic field, simultaneously with the nucleus. The cell did not appear to have any outer membrane. Magnified 1150 diameters (Zeiss Hom. Im. $^1/_{18}$, oc. 2; cam. luc.).

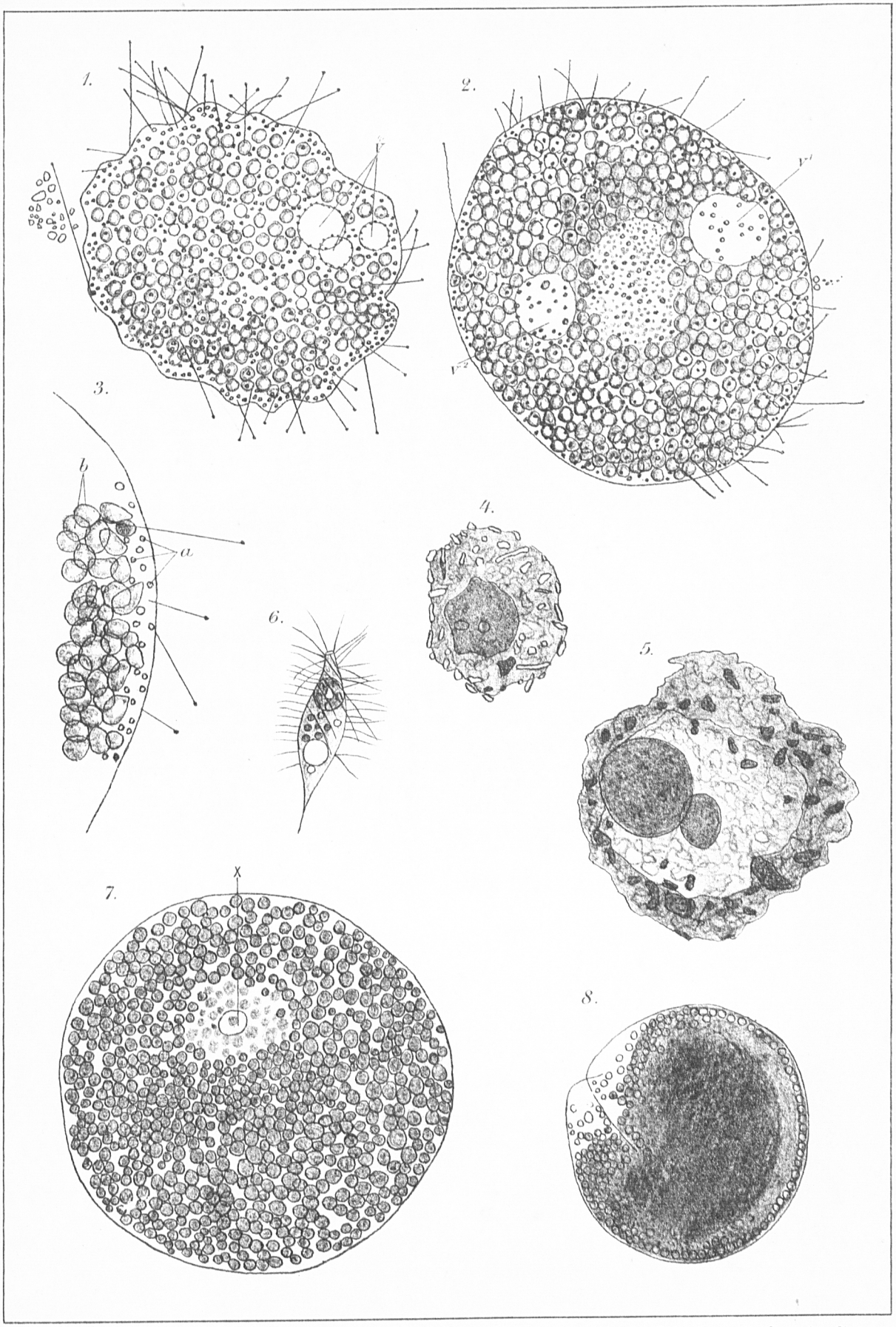
— 5. Probably a larger individual of the same species as Fig. 4. From a pond on the ice, July 27, 1894. Killed with Chromo-Aceto-Osmic Acid, stained with Borax-Carmine. Drawn in the same way as Fig. 4, so that only those sand-grains situated on the same level as the nucleus are visible. Magnified about 1000 diameters (Zeiss obj. F, oc. 3; cam luc.).

— 6. Several individuals of this kind, were seen in a preparation from a lump of algæ floating in a channel between the floes, nearly 2 metres deep. They had a rapid vibrating, flagellata-like movement, but were fixed, as it seemed, to diatoms. The grains inside them were refractive with a greenish blue colour. There was apparently a spiral-winding in the body, which became visible by the effect of the Osmic Acid (see Fig. 6). The cilia were as far as could be seen in ciliatory motion, but it was probably the tail which produced the vibrating flagellata-like movement of the body. The length of the tail could not be decided. A vacuole was very distinctly visible in the posterior end, but was never seen contracting. Where the body was fixed, could not be seen, but to judge from the mouvements it was on the under central part.

Aug. 3, 1894. Magnified about 1000 diameters (Zeiss obj. CC, oc. 5, long tube; cam. luc.). Length of body without tail 0·027 mm. Killed with Osmic Acid.

— 7. *Globular Alga.* Living. From channel between the ice-floes, Aug. 3, 1894. Magnified about 460 diameters (Zeiss obj. CC, oc. 5; cam. luc.). Diameter of cell 0·15 mm.

— 8. *Globular Alga.* From same place as Fig. 7, Aug. 3, 1894. (Zeiss obj. CC, oc. 5; cam. luc.). Diameter of cell 0·11 mm.



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