10. A Late-Glacial Specimen of *Lucioperca lucioperca* and its Environments.

A study of some Uppsala clay varves.

By

Nils. G. Hörner.

Contents.

Abstract. In the otherwise mostly sterile Late-Glacial varved clays of Uppsala, remains of a pike-perch (Swedish: gös), *Lucioperca lucioperca*, were found in a varve, datable by the De Geer chronology. The fish died and was buried — in a water depth of 100 to 150 meters — by the settling Baltic sediments approximately 7500 years B. C., about 35 kilometers off the retreating edge of the land ice, which had only 140 years before withdrawn from the spot where the fish was to be deposited. The species, now living in fresh and slightly brackish waters, indicates that salinity off the waning ice sheet at the time can have been, at best, only slight. That conclusion is strongly supported by the structure of the varves, especially their particle size distribution, studied in some detail and indicating a very incomplete coagulation during sedimentation. The temperature requirements of the species implies a rather warm Fini-Glacial climate, which is in good agreement with studies by others along different lines, partly summarized and briefly discussed.

1. Introduction.

Scarcity and importance of fossils in Fini-Glacial Baltic deposits. New find. Some problems of surrounding varve clays actualized.

Many questions concerning Late-Glacial conditions in the Baltic and its surroundings still remain open, partly because of the scarcity of fossils, especially of informative and datable fossils, in some crucial strata and regions (cf. MUNTHE 1892, pp. 81, 103; 1896; 1940, p. 91; J. P. GUSTAFSSON 1905, p. 275; A. G. HÖGBOM 1915, p. 33).

In the Fini-Glacial Baltic deposits of east central Sweden there have, to be sure, been quite a few finds of small, poorly developed shells of *Portlandia arctica* since the first discovery by IGELSTRÖM in 1858 (see MUNTHE 1892, p. 83—84; G. DE GEER, 1932, p. 32), but their distribution is rather limited. (See Fig. 8, p. 211, where they are given as compiled by MUNTHE 1940, Pl. II. Some further *Portlandia* finds are known within the map area west of those stated.)

Late-Glacial vertebrate remains have very seldom been reported from our region. The most thoroughly studied and best known is an almost complete skeleton of *Phoca groenlandica* found in Kungsträdgården in Stockholm »3—4 fot under der befintliga lerlagers nivå» (»3—4 feet under the level of the clay layer there existing» KINBERG 1869, p. 14). The information on stratigraphical position is not nearly as complete as the osteological data.

In Bockhussätra, some 25 km W.S.W. Stockholm, a skeleton of whiting, *Gadus merlangus*, was found in varved clay (MUNTHE 1924). The localities of both the vertebrate remains mentioned, *Phoca* and *Gadus*, are marked by crosses on map Fig. 8 (by stars on MUNTHE 1940 Pl. II).

Of great interest is a herring, *Clypeus harengus*, in varved clay at Nora, 30 km W. of the western end of map Fig. 8, and at latitude 59° 30′ (MUNTHE 1932, also DE GEER 1931). Before the Uppsala find now under consideration was made, the Nora herring probably was the northernmost vertebrate remains found in Late-Glacial varved clay in Sweden. Skeletal parts of a whale found long ago on Gräsön — approximate position of find lat. 60° 28′ N. long., 18° 25′ E., near N.E. corner of map Fig. 8 — LILLJEBORG, 1859, thoroughly studied zoologically and originally described as *Balaenopteron robusta* (LILLJEBORG 1860, p. 602, 1862, pp. 39 ff., especially pp. 47—48), later changed to *Eschrichtius robustus*, have been considered by ERDMANN (1868, p. 148—149) to have probably sunk on the spot before the deposition of glacial clay had come to an end. As considerable parts were found in Post-Glacial clay — Litorina sea deposits with *Tellina baltica* and *Mytilus edulis*, a Post-Glacial age seems however much more likely, when LILLJEBORG's descriptions and explanations are considered. Like several others (especially MUNTHE 1892, p. 103), the present writer excludes the Gräsö whale from the list of Late-Glacial finds.

Even if, on the map Fig. 8, the number of crosses marking vertebrate finds in varved clay within the area, should not be quite complete, the impression of great scarcity of such fossils would nevertheless be correct.

In Finland also, vertebrate fossils in corresponding clay series are utterly rare, if found at all. Professor MATTI SAURAMO, who has probably a wider and more thorough experience than anybody else of the varved clays of Finland, has never found any vertebrate fossils (personal communication).

Certain Russian fossil-bearing glacial clays, especially of the Leningrad district, seem to be much older than the period here under discussion and do not add to its biota.

In short, the Fini-Glacial Baltic varved clays, when seen with the naked eye, almost always give an impression of absolute sterility. Whereas from some regions rhizopods, foraminifera, and especially ostracods are reported as not infrequent in Yoldia clay, when minutely studied (MUNTHE 1892, p. 102, 1896, 1940, p. 91), in other wide areas even microscopic examination seems to have yielded little or no result.

Biota, interpreted as »Glacial» relict elements, in the Baltic and in lakes in the countries around the Baltic (JÄGERSKJÖLD 1912, A. G. HÖGBOM 1915, pp. 33—34, S. EKMAN 1922, pp. 278—297) suggest, however, that the Late-Glacial marine fauna of our region was considerably richer than the few species of fossils, so far found, lead one to believe (A. G. HÖGBOM 1915, p. 34). Numerous sections in the Late-Glacial varved clays of the Uppsala region — at brick yards and temporary exposures — have long been searched for fossils, searched time and again, but in vain (see also MUNTHE 1892, p. 103; 1940, p. 80—81). MUNTHE thinks that the ostracod *Condona candida* might possibly have immigrated already at the end of the Yoldia time. When he had found it in what was then called »undre grålera», lower gray clay (1893, p. 120 ff.), in a rather complex section on the slope of the esker hill Galgbacken in Uppsala, point 7 on map Fig. 1, he interpreted the clay as belonging to Ancylus time. No shells were found in the bottommost part of the clay, which, because of some very thin varves, was thought to belong to Yoldia time. — On macroscopial examination the varved clay of the region as a rule appears quite sterile, in sharp contrast to certain Post-Glacial deposits (J. P. GUSTAFSSON 1909, pp. 717—722). Nor have suspension and decantation so far led to the discovery of organisms in the Glacial deposits of Uppsala. A microscopical examination of the residuum after treating Uppsala Glacial clay with hydrofluoric acid (method of ASSARSSON and GRANLUND 1924) in some cases reveals organic remains. As yet only some pollen and spores have been identified. (See below, p. 264.)

At certain stages of drying the clay easily splits along layers of slightly coarser material (as exemplified by the crack in Fig. 10 in this paper). Under favourable conditions sedimentation surfaces thus exposed may show imprints caused by different lower animals, not yet identified (A. G. HÖGBOM 1915). There have also been observed some semi-parallel scratch marks, possibly caused by the fin of some larger fish sweeping over and touching the clay surface at the time of deposition (loc. cit. p. 39). Workmen and others concerned with the clays have repeatedly been asked to look out for fossils and report possible finds to some University institution. The fish remains here described were discovered, reported, and presented to the University by Mr. V. ERIKSSON of the brick works of Vaksala Tegelbruks Aktiebolag. On August 24th 1944, Dr. G. WÄNGSJÖ of the Palaeontological Institute informed the writer, then occupied with field studies of the Quaternary of the Uppsala region, that Dr. Å. HOLM of the Department of Zoology had received a report about a fish just found in the clay of the Vaksala brick yard. Dr. WÄNGSJÖ and I immediately set out to examine what had been found in that locality, so familiar to me from numerous previous visits.

One of the innumerable pieces of clay, dug by hand shovel had, when breaking up along a bedding surface, disclosed the remains of some of the more resistant parts of a fish — minus the head, which had evidently already unnoticed disappeared into the brick manufacturing processes. Of the rest of the fish some skeletal parts remained intact, others had left a clear cast in the clay. A considerable part of the scales of the fish was in a fairly good state of preservation though not in its undisturbed original position. Fig. 4 p. 206 gives a general view of the bulk of the remains.

As a contribution to the knowledge of early immigrants in the Swedish fauna the new find seems worthy of an investigation.

The following study is, however, even more concerned with the setting of the fish than with the fish itself. The information arrived at about the fish is a means, not an end — means to an added understanding of certain insufficiently known geographical conditions during the eventful period in which the specimen lived.

Consequently, other nearby approaches to those problems have also been freely tried. In connection with field and laboratory observations on the varved clay in which the fish was found, some more general questions on sedimentation and varve formation are considered and some desirable further studies suggested.

2. Setting.

Location. The clay pits of the Vaksala brick yard, where the fish was found, are located in the eastern part of the town (map fig. 1), on the Uppsala plain. Specification of finding spot: some 200 meters E. or E.N.E. of street corner Norrtäljegatan-Oxelgatan, and approximately 8 meters above sea level of to-day. Here over an only slightly uneven archaean rock surface[1], not exposed at our locality, the complete series of varved glacial

[1] Such evenness of the rock surface by no means characterizes the whole Uppsala district. As one of the factors controlling hydrography and sedimentation — and sediment preservation — topography demands some consideration. The resurrected Pre-Cambrian (Sub-Cambrian) peneplain is, to be sure, the dominating morphological feature of our region, but in detail the topography is rather broken. See map fig. 1, where except for the esker, recognizable through its shape, most wooded areas show bed rock topography, the cultivated plains mean sediments. Small figures on map give elevation in meters above sea level. The bedrock is crossed by faults and joints in different directions. (E. WIMAN 1930, pp. 99 ff., 1942, pp. 289, 298, and works quoted by him.) The bulk of the movements may date too far back to show up in the present landscape. Along some lines at least, movements have been repeated time and again (regenerated breccias, A. G. HÖGBOM 1916, p. 406 f.). Some of the old dislocation zones seem to have been re-actualized by a Tertiary upheavel of Fennoscandia (G. DE GEER 1912 b, p. 857 f., 1924, p. 317, LAURELL and HEDENSTIERNA 1938, p. 129); part of those zones have been subject to small differential movements even in Late-Quaternary time (TÖRNEBOHM 1879, p. 350, SANDEGREN 1916, p. 11, HÖGBOM 1916, WITTING 1918, p. 295, VON POST 1929, pp. 68—69, TANNER 1930, p. 221 ff., 1938, p. 320 ff., 394, DE GEER

Fig. 1. Location of fish remains and samples, Vaksala brick yard, also of some other localities around Uppsala, mentioned in the paper.

On the reprint of a detail of the official topographical map the following localities have been marked. (Circles: sections in varved clay:)

1) Clay pit of Vaksala brick yard where the fish was found.
2) Bärby.
3) Nyåker (pit of the Röbo brick factory).
4) S:t Erik.
5) Kungsängen.
6) Bergsbrunna.
7) Galgbacken.

1932, p. 77, SAURAMO 1934, p. 32, 35, 1939, p. 46—47, 1947, p. 96—97, K. E. BERGSTEN 1943). Glacial erosion seems to have been decidedly selective in our region, efficient mostly in dislocation zones, especially when their direction more or less coincides with the ice movement (AHLMANN and LAURELL 1938, p. 11). The rôle of earlier fluviatile erosion in the present landscape is not settled with the recognition that there was no adequate foundation for GUNNAR ANDERSSON's (1903, taf. 1, also p. 53 ff.) admittedly rather hypothetical map of an ancient drainage system of the Mälar region (LAURELL and HEDENSTJERNA 1938, p. 129. See FROMM 1943, p. 158, 167 on a Pre-Quaternary lower base level). The bed rock morphology of our region still holds many problems.

Whatever the explanation, within the distance of a few kilometers there are differences in elevation of the rock surface of up to some 120—140 meters, though partly clays remain intact, save for present digging in the topmost meter or two. Well exposed in the pits are the strata marking the transition to the unvarved Post-Glacial clays (see GUSTAFSSON 1905), the bottom layers of which are also preserved to a somewhat varying thickness; in the pit where the fish was found, from less than a meter to about 2.5 meters. A kilometer and a half further to the southwest, at the water works of the town, where thick sediments fill a rock valley or depression, by far the most of the 107 meters of clay is Post-Glacial (A. G. HÖGBOM 1913, p. 100).

Though deposited in water more than a hundred meters deep, and, on the whole, rather regularly arranged (see fig. 3), even the Glacial clays of the region show considerable variation in thickness from one locality to another. That is especially true near the bottom of the series, in the rather proximal part of the varves. There is no complete section in Uppsala or its immediate neighbourhood of the entire, undisturbed clay series. By combining information arrived at from different localities[1], the total thickness of the undisturbed Glacial clay in the Uppsala region is found to be some 7 to 10 meters — in the bottom of the deep depression under the Fyris region it is probably considerably more.

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concealed by Late-Glacial and Post-Glacial sediments. Such is the case along the Fyris River in the southern part of, and south of the town proper, where the sediments reach a thickness of up to 100 meters and slightly more.

[1] Figures of depth of Glacial clay, arrived at through borings, may be treacherous, as it is difficult to know whether the layers are undisturbed. Investigations for technical purposes have sometimes indicated 20—30 meters, perhaps even more, of Glacial clay. In many cases — as at Uppsala-Ekeby — it is evident that clay has been secondarily added by displacements (earth slides, mainly subaquatic, see also HOLMQUIST 1897). In some other clay pits, boring is avoided, so as not to pierce through protecting water-tight clay layers, holding back slightly artesian water in sand layers in the lower part of the glaci-aquatic sediments. At Nyåker, where for topographical reasons it seems almost certain that no material could have been added secondarily by sliding, a boring gave 5.2 m of Glacial clay underneath varve No. -844 (according to connection in analogy with those illustrated in fig. 3). At the Kungsängen pit of the Ekeby Co. there were somewhat more than four m of undisturbed varve clay exposed above varve -844 without quite reaching the top of the series.

GUSTAFSSON (1905, p. 273), in Erikslund (S:t Erik) in the N.W. part of the town reached the substratum of the clay 1.5 m under the bottom of a section through 6 m undisturbed varve clay. He finds it, however, uncertain whether those 7.5 m represent a complete section through the varved clay. A. G. HÖGBOM (1915, p. 36), referring to Gustafsson, gives a thickness of the glacial clay of some 6 m in all.

Informative sections of sufficient vertical extensions through the undisturbed bottom parts of the varve clays of Uppsala, have to my knowledge, not been available for years.

At Bärby, Gamla Uppsala, in 1943 some excellent sections at the foot of a little hill reached the underlying till. The section seemed normal down to varve -830 (as for numberings of varves see below p. 204). Separating the deepest clearly distinguishable normal varve from the underlying till is an undifferentiated clay layer with an observed thickness of 0.9—1.1 m.

Stratigraphy. In the lower part of the normal varve series of the Uppsala region, the annual accumulations in many cases measure a decimeter or more in thickness. Upwards they grade into varves only a few centimeters thick; those in their turn are higher up succeeded by varves of a few millimeters and finally by those of less than a millimeter. Varves of a thickness of down to about $^1/_2$ cm or slightly less are as a rule clearly distinguishable, being composed of light gray and distinctly brown layers from the warmer parts of the year of sedimentation, and intervening dark gray winter layers. Many varves show considerable differentiation in structure and shades, as exemplified and discussed later.

A general idea of normal Uppsala varves, including those most discussed in this paper can be gained by that excellent and often reproduced photography by ADAM REUTERSKIÖLD, reprinted, with varve datings, by DE GEER (1940, Pl. 18). Representative types of Uppsala varves are presented in colour by DE GEER (1940, Frontispiece, sections 3 and 4), the latter figure also shows the grading from normal varves to microvarves, also the topmost distinct, brown layer. Cf. J. P. GUSTAFSSON 1905, p. 269. (Further pictures, in black and white, of Uppsala varves, DE GEER, loc. cit. Fig. 36, p. 139, and Pl. 50 a. Much information about the development of our varve series is also found in the diagrams, loc. cit. pl. 80.)

Varves of a thickness of 2 or 3 mm down, have as a rule no brown layer but only varying shades of gray. Such microvarves (microdistal varves, G. DE GEER 1940, s. 85) are in many cases difficult to differentiate. Above the topmost clearly distinguishable brown layer, there follow some 0.3 m of fine, dark, mostly »spotless» clay, the greater lower part of which shows microvarves, the structure being especially noticeable when the clay is drying. In the upper part of these three decimeters, the microvarves appear thinner and thinner, fainter and fainter, gradually to be succeeded by a clay, that under normal conditions gives the impression of being homogeneous; no varves are to be seen.

Higher up that apparently homogeneous clay is succeeded by »fläck-zonen», the »spotted» zone, some 0.1 m thick: a clay, dotted with sand and gravel particles, mostly calcareous (see J. P. GUSTAFSSON 1905, p. 271—273, fig. 3—4; G. DE GEER 1940, Pl. 50, »micro-boulders», cf. also MUNTHE 1940, p. 81), once dropped into the sea or lake by icebergs from a region where the land ice was rich in such fragments (J. P. GUSTAFSSON 1905, p. 273).

The varveless, homogeneous clay just above the »spotted» zone is very fine, made up of smaller particles than most of the clay of our region. The layer of unusually fine clay has been considered roughly to correspond to the Ancylus time and consequently been characterized as Ancylus clay (ODÉN and REUTERSKIÖLD 1919, p. 138 ff., cf. however GRIPENBERG 1934, p. 207 ff., see also below p. 250). Towards the top the fine »Ancylus» clay grades into somewhat coarser material — with the rise of land, the shore and the mouth of ordinary rivers approached our region.

One of the main factors determining the transition from varved to unvarved clay, in facies within the varve series, is the distance from the edge of the land ice — or rather, from the mouth of the glacial river. As the ice front was rapidly retreating, similar changes of facies within the deposits may represent different chronological position in different localities (»gliding time scale», CALDENIUS 1941, p. 94).

Dating. As has already been stated, the fish remains were found in a lump of clay just as it was dug out from the wall of the pit. The spot that Mr. ERIKSSON pointed out as a close approximation of the position of the fish was located in DE GEER's varve −738, 0.49 m underneath the topmost clearly distinguishable brown layer. Marks of the digging confirmed the general estimate of location. The preserved varves in the two once contacting pieces of clay to which the fish remains adhered or on which they were registered as casts, seem however to make it almost certain that the fish had been deposited in, or close to, DE GEER's varve −734, here 0.43 m below that topmost brown layer.

The number here attached to varves, and to years of their formation, are identical with those in DE GEER's »Geochronologia Suecica» (1940), except perhaps for some lower varves, discussed later.

Connections are presented here in fig. 3. The number −738, etc. means that the varve, according to DE GEER, was deposited 738 years before his zero year, characterized by the supposed bi-partition of the last ice remnant in Jämtland (G. DE GEER 1911, p. 468 ff., 1940, p. 171 ff.; cf. however G. FRÖDIN 1925, p. 189 ff. and 202, CALDENIUS 1941, pp. 101—102), some 6840 years B. C. (LIDÉN 1938, pp. 402—404, especially fig. 1 p. 403). A minor extrapolation in linking up the LIDÉN chronology to our common historical chronology (loc. cit., p. 402) may involve a possible error of at most some 100 years (FROMM 1938, p. 366), but modern sampling tech-

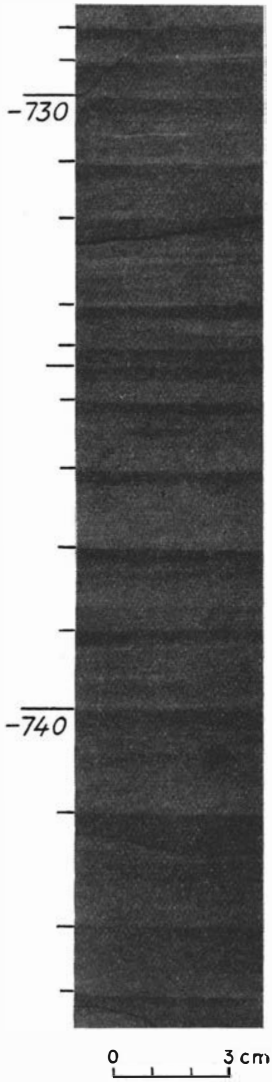

Fig. 2. Some clay varves when saturated with moisture. These are varves, taken at the »Karlskrona» pit, Kungsängen, Uppsala, and preserved with glycerin. Appearance, save for crack, fairly like that of wet clay (same varves) intact in section where the fish was found. Compare fig. 3.

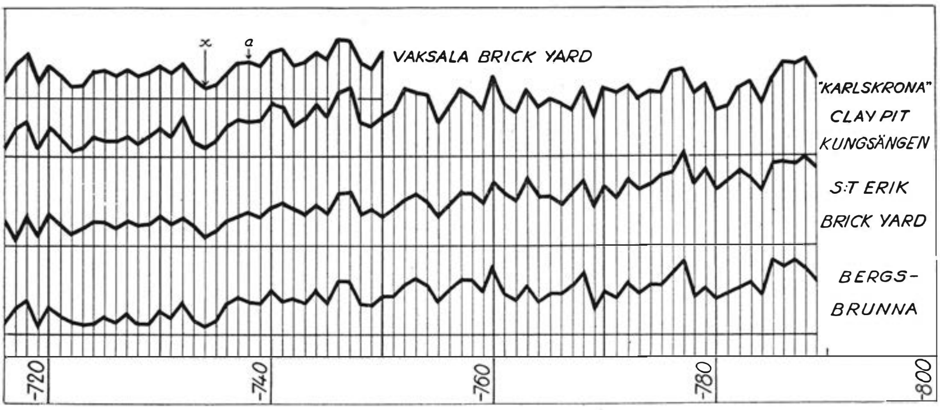

Fig. 3. Graphs of parts of new varve measurements from the location of the fish (Vaksala brick yard, Uppsala) (measured by HÖRNER 1944) and from the »Karlskrona» pit on Kungsängen, Uppsala (measured by S. RUDBERG, G. FLODKVIST, HÖRNER and others 1940), connected with G. DE GEER's diagrams from S:t Erik and Bergsbrunna, measured in 1916 and published 1940, plate 80. On the Vaksala graph *a*) marks the layer pointed out as approximate level of fish, *x*) location of fish according to examination.

nique for varves still under water (KULLENBERG and FROMM 1944, KULLENBERG 1947) is likely soon to eliminate any uncertainty and also to check the results of attempts in dating by combination of geochronology and teleconnected dendrochronology (EBBA HULT DE GEER 1936). When, on the strength of the perfect agreement of the new varve measurements here used for connections, and those of »Geochronologia Suecica», the DE GEER numbers are adopted here, the writer does not overlook the possibility that further studies may somewhat modify the number of varves between those represented in fig. 3 and DE GEER's zero (cf. CALDENIUS 1941, p. 101, LUNDQVIST 1946, p. 285; cf. the discussion by SANDEGREN, DE GEER, CALDENIUS, a. o. on continual retreat or oscillation of ice border in the Gävle region, reviewed by LUNDQVIST 1943, pp. 114—116; see also SANDEGREN 1946, p. 70 ff.). MUNTHE (1931, p. 22—24), explains an irregularity observed by him in an Uppsala clay section (thick varves, separated from underlying thin varves by a zone of disturbance) as possibly indicating a considerable oscillation in the Uppsala region. Judging from the description, such a disturbance may have been caused by later sliding. A stronger indication of an oscillation in the Uppsala region was found by G. FRÖDIN: distorted varves underneath a frontal moraine at Lurbo (see HJULSTRÖM 1944, p. 342). FRÖDIN's oscillation was, however, earlier than the final withdrawal of the ice from our Vaksala locality and thus does not influence our datings.

Judging from measurements by DE GEER (1940, p. 257, also pl. 61 D and 80) and his collaborators a couple of kilometers W. and S.E. of the place where the fish was found, the ice border retreated from that spot between the years —870 and —880, probably nearer —880. A kilometer and a half southwest of this place I have measured good sections down to what should correspond to varve —871, figuring from excellent connection between upper part of cut and the diagrams published by DE GEER. Underneath what is supposed to be —871 there evidently still remained quite a few varves. Below, say, varve —820, there are however great local variations in the development of the varves, and in a few cases it may even be difficult to decide what is a real varve or merely a structural feature inside a thicker varve. It has not been possible so far to get a satisfactory continuous agreement below varve —820 between the new diagrams of the Uppsala region and those published by DE GEER. The long interruption around —820 and downward of most of DE GEER's (1940, p. 80) diagrams from the Uppsala region may have to do with the local variations and sometimes somewhat problematic development of a number of varves. Among DE GEER's (1940, pl. 80) six Uppsala diagrams spanning the period 820—830, etc., four show longer or shorter interruptions around the period mentioned. Of the two series continuing through that time, number 9, Röbo, is notoriously difficult because of frequent abnormalities in original deposition (close to esker) and secondary disturbances of sediments when already deposited. New measurements in the Uppsala region show considerable variations, especially from approximately varve —820 down, and for the same period they all differ from the Bergsbrunna diagram. So far, I have not been able to reach, even in that short distance, anywhere near the 90 % highly qualified agreement that E. HULT DE GEER (1943, p. 228) found between Bergsbrunna and Finland (Cf. CALDENIUS 1944 a, p. 85). For the lower part of the varve series of the Uppsala region the measurements as yet available are not sufficient to solve certain questions involved. Here further studies are needed, and seem to promise interesting results. So far, our immediate connections to the DE GEER diagrams of the region keep within the range —716 to —820; when numbers of varves below (numerically higher numbers than) —820 are given, there is a direct field relation between those varves and the properly connected ones above. With allowance for some minor uncertainty concerning the lowest varves, a linking up to the DE GEER and LIDÉN chronologies leads to the conclusion that the fish was deposited some 140 years after the withdrawal of the ice, or some 9500 years ago when the retreating border of the land ice had reached the central part of what is now the parish of Vendel (almost to Örbyhus), some 35 km north of Uppsala. (G. DE GEER 1940, p. 254 — see also pl. 68, and 72, P. Q. compared with text p. 150 and map fig. 37 p. 117).

3. The fish.

Note on its deposition and state of preservation. The fish evidently did not stir, once having slowly sunk to the bottom, the strata being undisturbed save for some minute settling caused by the compression of the carcass. The carcass seems to have been in a rather advanced state of decay already when it sank. The abdominal skin (see the fairly even light part above the »0» in the measurements to fig. 4) has been torn somewhat out of its right position in a way that can hardly be explained by the pressure of the load after embedding. The scales of the fish, partly well preserved on that displaced skin, are hardly discernible in the picture. The fossil has been considerably damaged during salvage, transports and examination. If the part above the crack in the upper section of the figure really stays, as I believe, in the same position as when found, the remains of fins in the upper left corner indicate a rather thorough distortion of the carcass already when it came to rest on the sea or lake bottom; they are probably the ventral fins completely dislocated in a way that could not possibly have been brought about by post-embedding pressure alone.

Identification. The identification of the species, rendered difficult by the incompleteness of the skeleton, where the head and part of the spine are missing, was kindly taken over by the Department of Vertebrate zoology, Swedish Museum of Natural History in Stockholm. The writer is much indebted to Professor H. RENDAHL and Miss GRETA VESTERGREN, M. Sc.,

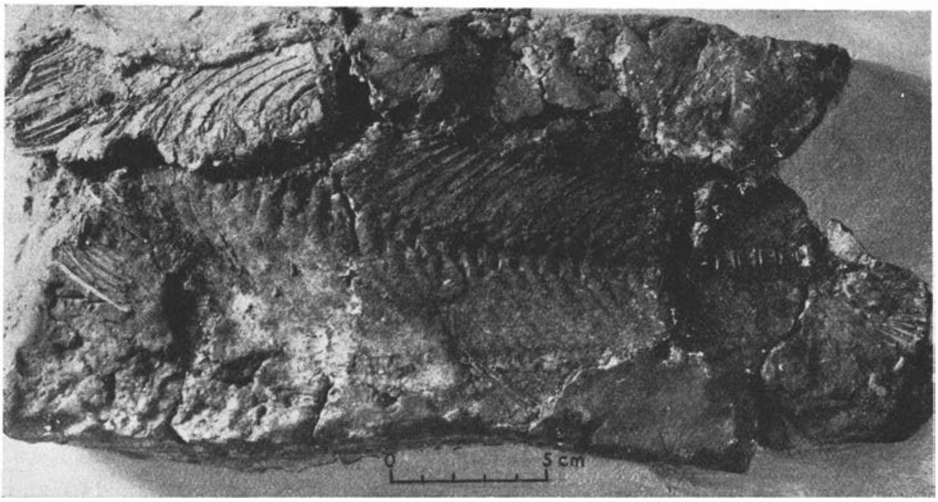

Fig. 4. Remains of *Lucioperca lucioperca* on a piece of glacial clay. Photography: N. HJORT. The picture, unfortunately, was taken when the specimen had already passed through the hands of different specialists, and waited long for examination, not to speak of a minor accident during the transport. A photograph taken by the same expert camera man immediately when the specimen was first found, would have given a much better result.

for their expert examination of the fragments. The statement of the Museum is summarized as follows (translated by the writer from the original Swedish):

»Considering the shape of the fish, the structure and position of the fins, and the morphology of the scales it can immediately be stated that the fish must have been either a perch (*Perca fluviatilis*) or a pike-perch (*Lucioperca lucioperca*). A closer examination proves however, that perch is out of the question but shows identity with *Lucioperca*, as exemplified by the following table.

	Perca fluviatilis.	*Lucioperca lucioperca.*	Our fossil.
Number of vertebrae	41—42	46	38 + ?
Number of rays in front dorsal fin . . .	14—16	14—15	13 + ?
Number of rays in posterior dorsal fin .	I: 13—14	I—II: *21—22*	*19* + ?

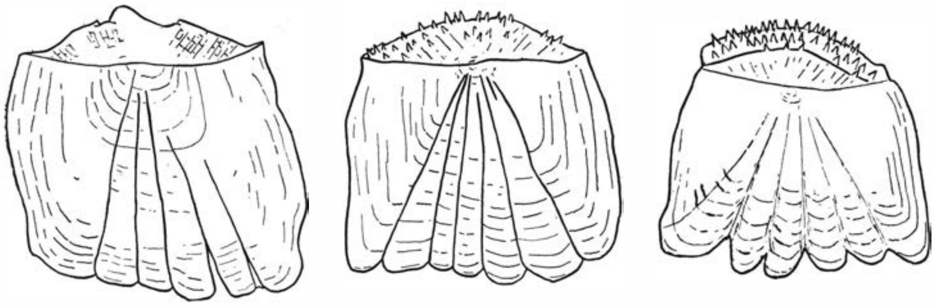

Fig. 5. A scale of the fish remains in the clay, a), compared with scales of present day *Lucioperca lucioperca*, b), and *Perca fluviatilis*, c). GRETA VESTERGREN del.

As a further support of the identification may be pointed out the very good agreement between the scales of *Lucioperca lucioperca* and of the specimen found in varved clay, as is seen from the figure. For comparison, also a figure of a scale of perch is presented.

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GRETA VESTERGREN.
M. Sc.»

Miss VESTERGREN's drawings of scales are here reproduced as fig. 5.

Size. The fragmentary state of the *Lucioperca* remains prevents a direct measurement of the total size of the fish. By comparing the dimensions of the sections preserved with corresponding parts of modern pike-perch, it should be possible to approximately estimate the original size of our Late-Glacial specimen. For comparison, I procured a fresh specimen, about 0.46 m long, caught in lake Hjälmaren. Through the courtesy of Dr. BENGT LUNDHOLM — who has also otherwise in many ways furthered

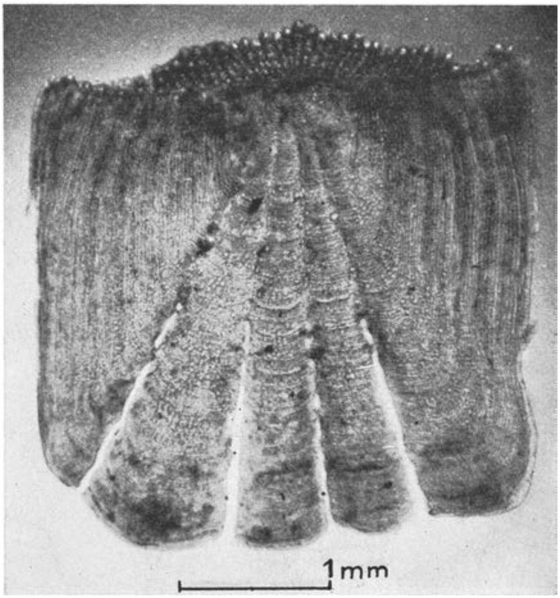


Fig. 6. A scale of the fish remains of fig. 4. The closer spacing of the concentric grooves, the darker zones (best visible on certain side parts of the scale) are interpreted as marking winters: period of slow growth. Photography: N. HJORT.

the zoological approach to this study — the Dept. of Zoology kindly undertook the dissections and measurements necessary for the purpose.

As seen in Fig. 4, five vertebrae remain intact in our fossil, forming the end of the spine. That the last vertebra left really marks the end, seems proved by the fact that it is smaller than the rest.

Comparison between the length of each of the five last vertebrae of the fossil and that of the Hjälmaren specimen (numbers beginning from the rear end of the spine):

Vertebra number:	I	II	III	IV	V
Modern specimen	3.5	5.0	5.0	5.2	5.1 mm
Late-Glacial specimen	4.5	5.0	6.0	5.9	6.2 »

Length of section of spine occupied by last five vertebrae in Late-Glacial specimen 30.5 mm, in the L. Hjälmaren specimen 25.0 mm.

Distance vertebrae 1—35 from end in Late-Glacial specimen 241.5 mm, in the L. Hjälmaren specimen 222 mm.

The total length of the modern L. Hjälmaren specimen was:

from tip of snout to tip of caudal fin	. . . . .	459 mm
» » » » » concavity of caudal fin	. .	435 »
» » » » » Basis caudalis	. . . . .	398 »

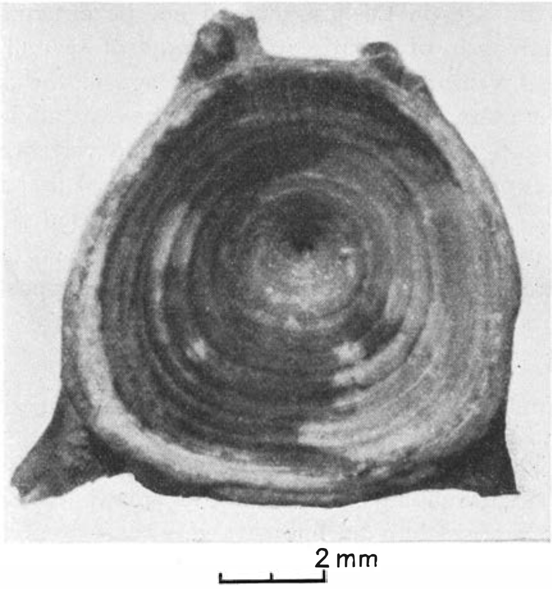


Fig. 7. A vertebra of the »clay fish». If the »rings» are annual, the specimen should have been around 8 years old. Photography: N. HJORT.

being 2.07, 1.96 and 1.79 times the part of the spine occupied by the 35 last vertebrae. Identical relations for the Late-Glacial specimen would mean that its length would have been:

from tip of snout to tip of caudal fin about . . 500 mm
» » » » » concavity of caudal fin about 470 »
» » » » » Basis caudalis about . . . 430 »

If only the last five vertebrae had been used for comparison, the corresponding values of total length of the fossil specimen would have been estimated to be c. 560, c. 530, and c. 490 mm, respectively. As the longest distance identifiable on the fossil ought to be best suited for comparison, it seems likely that the total length of the Late-Glacial specimen was about 50 cm — naturally an approximate figure only.

Age. As the favourableness or unfavourableness of its surroundings highly influences the rate of growth in a fish, it would be of interest to know how long a time it had taken our Late-Glacial pikeperch to reach the size just estimated. Such information might also throw some light on the conditions in our finiglacial waters.

Through the courtesy of Doctors G. ALM and E. DAHR scales of the fossil were examined by every expert of the Swedish Government Bureau of Fisheries. Unfortunately the scales were not well enough preserved to satisfy the authorities when seeking to determine the age. Their conclusion was that the age, in this case, could not be determined. Therefore, when on the strength of microscopical study of several scales like that shown in fig. 6, I venture an estimate of the age of the fish to have been about 8 years, it must be clearly stated that the estimate lacks the backing of any ichtyologist. A later examination of some vertebrae (not available to the Fishery experts), counting the »rings» (fig. 7) as suggested already by H. HEDSTRÖM 1759, a method now commonly used (K. A. ANDERSON 1942, p. 509), seemed to support the impression as to age gained from the scales. Therefore our Late-Glacial pikeperch is supposed to have been about eight years old when it died.

4. Introductory note on the Baltic at the time of the fish.

Any discussion on the possible living conditions of the fish, any consideration of its surroundings, implies some orientation about what is already known or supposed to be known about the waters of the region at that time.

The strata containing the fish remains were deposited in that Baltic stage generally known as the Yoldia Sea, a name still current in spite of G. DE GEER's well-grounded objections (1913, p. 308, 1940, p. 103—104, see also below). In SAURAMO's more specified system they should be assigned to Y IV (see for instance SAURAMO[1] 1939, fig. 4, p. 44, or 1942, fig. 7, p. 226, compared with p. 239); Y IV corresponds to what SAURAMO in earlier papers (1934, 1937) called *Rhoicosphoenia* stage, Rho. Sea (1939, p. 44; in reprint edition p. 8). HYYPPÄ's term (1943 a), Bothnian Sea, for a certain ancient stage in the development of the Baltic may lead to some confusion, as that very name Bothnian Sea is already frequently used for the southern and wider part of the present day Gulf of Bothnia, between the Åland Sea and the Kvarken (see for instance GRIPENBERG's map, 1934, fig. 1).

The Yoldia Sea was connected with the Ocean through straits across what is now Central Sweden, straits decreasing in depth and width with the rise of land. The main communication across the now Baltic-North Sea divide was located between the present lakes Hjälmaren and Vänern (see for instance MUNTHE 1940, Pl. II).

Yoldia (*Portlandia*) *arctica*, which has given the Yoldia Sea its name, has never, to the writer's knowledge, been found in the Uppsala region.

[1] Even though there are some differences of opinion about the exact connection between Finnish and Swedish Late-Glacial chronology (see, in addition to several of SAURAMO's publications, including 1926, 1934, p. 48, reprint edition p. 21, and DE GEER's »Geochronologia Suecica», 1940, also MUNTHE 1929, G. DE GEER 1930, EBBA HULT DE GEER 1943 and CALDENIUS 1944 b, p. 380—381), those differences do not seem to imperil the referring of our find, and the varve in which it was included, to Y IV.

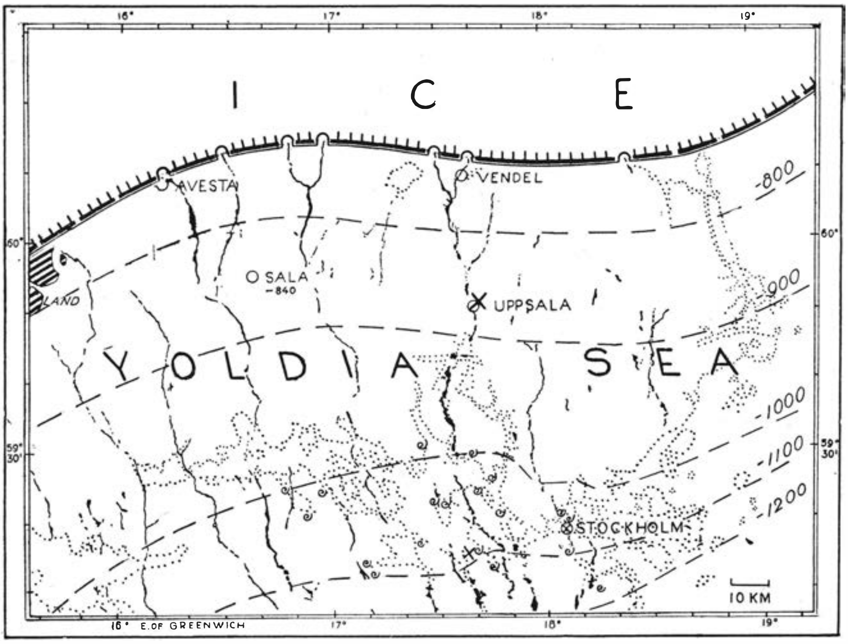


Fig. 8. Approximate map of condition when and where the fish was deposited. Present day coast and larger lakes marked with dotted lines. Oblique ruling near western edge of map: Late-Glacial land. All the rest land ice and sea, resp. Black serpents: eskers. Broken lines equicesses according to G. DE GEER 1940. Crosses: Vertebrate remains in varved clay, the biggest cross the *Lucioperca* in Uppsala, described in this paper. The little spirals: Finds of *Portlandia arctica* according to MUNTHE 1940. (The figures –840 at Sala represent a measurement to the bottom varve by HÖRNER October 18, 1945, not concerning this paper). Fig. 9 is mainly compiled, modified and adapted from G. DE GEER 1940, plate 68, and H. MUNTHE 1940, plate II. Without further entering here upon the discussion on the nature of the melt water rivers on or in the ice (BRENNER 1944) the mouths of the rivers are given symbols suggesting tunnel openings.

Around Stockholm, *Portlandia arctica* occurred as a dwarfed form for »scarcely more than 100 years» (G. DE GEER 1940, p. 103). On the map fig. 8 in the present paper the distribution of finds of *Portlandia arctica* is inserted as given by MUNTHE 1940, pl. II. It does not claim to be a complete index of known finds around Stockholm, but gives a fairly good general idea of distribution, see above p. 196. *Portlandia* seems to have disappeared from the Baltic some 200 years or somewhat more before the time of our fish, whether because of too low salinity or because of climatic conditions, or for both reasons combined, may deserve further consideration (cf. MUNTHE 1892, p, 104, DE GEER 1940, p. 103, cf. CALDENIUS 1941, p. 89).

A Late-Glacial connection between the Baltic and the White Sea, a sound across the present Lakes Ladoga and Onega, assumed by several geologists and at times much discussed, has been verified by HYYPPÄ (1943 a, b), but seems to belong to an earlier stage than Yoldia, or the Bothnian Sea, to use HYYPPÄ's terminology (— — — »in southern Finland the maximum of the Bothnian Sea is identical with Yoldia I», loc. cit. p. 115). HYYPPÄ's thorough reinterpretation of the late quaternary development is not as yet available in full, and the preliminary announcements, without diagrams, do not enable the reader sufficiently to penetrate the consequences.

SAURAMO (1947, pp. 91—92) interprets the Baltic—White Sea connection as belonging to a temporary Goti-Glacial transgression. The connection according to SAURAMO was broken again at the first Salpausselkä stage — perhaps a millennium or so before the time of the fish. Cf. also ASKLUND 1947, p. 48).

A. CLEVE-EULER still claims (1946, fig. 6, p. 23 etc. and especially 1947, fig. 7, p. 77, p. 79), as she has long done (see 1943 and earlier works quoted there), important connections between the Baltic and the White Sea — connections, which East of Uleåborg should have been rather wide around 7500 B. C. — about the time of our fish. Cf. however, in addition to the discussions of Finnish geologists, the low salinity stated further on in this paper.

The depth of the water where the fish was deposited cannot now be directly determined. No nearby point reached the surface of the sea at the time. Interpolation and extrapolation from observations by different specialists at greater distance (announced in different publications and used for compilation of marine limit and Yoldia Sea isobases by many writers such as GRANLUND (1936, p. 189; MUNTHE 1940, pl. IV; SAURAMO 1942, p. 228) suggest however a depth of roughly, say, some 120 to 150 (or, to be on the safe side, 100 to 150) meters at the time of the fish, provided that there were no great local irregularities in the changes of level of our region. Such irregularities have been discussed by Dr. ASTRID CLEVE-EULER especially in papers, read and discussed at meetings at the Institutes of Geology and Plant Ecology in Uppsala, the main content published 1947. See also 1946, fig. 6, p. 23, where the isobase for what Dr. CLEVE-EULER calls Yoldia level suggests that the depth at Uppsala at Yoldia time would not have been more than 50 m. In the opinion of the writer, there is not sufficient field evidence given to support such a statement, but Dr. CLEVE-EULER's indefatigable studies along independent lines is a reminder not to take established ideas or conclusions for granted.

5. Present life environment of *Lucioperca lucioperca*, and the species as possible indicator of conditions in the waters off the waning land ice.

Though it has long been known that *Lucioperca lucioperca* was a fairly early immigrant that must have lived in Scandinavia already in Ancylus time (LÖNNBERG 1898, as quoted by EKMAN 1922, p. 299, 301) and though the species is nowadays reported (in freshwater) from the south-western coastal region of the White Sea (EKMAN 1922, p. 510), it was rather surprising to find it so near the ice border in what has often (though not without protests) been characterized as an Arctic Sea (»ishavslera» — Arctic Sea clay — is a common name for the varved clay, in which the fish was found). EKMAN (1922, p. 299) points out that »when the Yoldia Sea was suceeded by the milder Ancylus Lake, possibilities opened for new groups of animals to immigrate to Scandinavia. — — — Earlier the low temperature of the Yoldia Sea — and to a certain extent perhaps also a too high salinity in the middle parts — had placed obstacles in the way.» (Translated from the original Swedish of EKMAN's book.) Among the fish, considered as relicts from the Ancylus Lake, is *Lucioperca lucioperca.* For double reasons, the change from Yoldia Sea to Ancylus Lake might be expected to clear the way for our species: increase in temperature, and the change of the Baltic to fresh water. But evidently, the species lived in the Baltic centuries before that change is generally supposed to have taken place. It should, however, be kept in mind that the exact time or extension in time of that transformation from the Yoldia to the Ancylus stage of the Baltic cannot as yet be indisputedly stated though reasons have been given for a time fairly close to the »bipartition» of the ice sheet in Jämtland, marking DE GEER's »zero year» and dividing Fini-Glacial and Post-Glacial epochs (see above p. 203). SAURAMO 1934, p. 38, 50; reprint ed. p. 11, 23, and pl. VI; 1942, p. 226, 238 (SAURAMO's Rha is »the end stage of the classical Yoldia Sea»); FROMM 1938, p. 378; VON POST 1947 a, p. 210; MUNTHE (1940, p. 79 ff.), on the contrary holds that »Ancylus» conditions began when retreating ice edge had reached a bit north of the Gävle region. ODÉN and REUTERSKIÖLD (1919, p. 37) even claim that the Ancylus Lake stage probably began already when the edge of the ice stood a few tens of kilometers north of Uppsala. SANDEGREN, too, (1934, p. 141, 1939, p. 134, 1943 a, p. 97, 1946, p. 96) reckons with a beginning Ancylus stage several hundred years before the »bipartition», DE GEER's zero. ASKLUND (1935, pl. 3 and pp. 15 ff.) sets the beginning of the Ancylus stage somewhere between 7500 and 8000 B. C. but still finds that in central Gästrikland, a hundred kilometers or so northwest of Uppsala it was preceded by a marine Yoldia stage. The shallowing up of the waters connecting the Baltic with the ocean is registered in the general type of the clays already south of Uppsala (CALDENIUS 1941, p. 89—90). S. FLORIN in a recent paper (1947, p. 232) considers the extension of the term Ancylus Lake to include also a certain Sub-Arctic, Fini-Glacial fresh water stage in the complicated sequence traced by him. The present study, as will be seen later, supports the idea that the transformation to practically fresh water conditions was already well on its way at the early time here discussed.

Lucioperca is essentially a fresh water fish, and, more specifically, prefers lakes to running water. It also occurs in brackish waters (GASCHOTT 1928, pp. 56—58) and is frequently found in parts of the Baltic as exemplified and specified for Swedish and Finnish regions by Professor SVEN EKMAN in a personal communication, in which references were given for each statement. Among the many references that Professor EKMAN called my attention to, in addition to what he states in his classical work of 1922, p. 299, and map, fig. 73, p. 301, were numerous notes and communications in Svensk Fiskeritidskrift (for instance 1919, p. 187, on occurrences among the islands off the Stockholm region; Fiskeritidskrift för Finland, for example 1939, p. 120, further confirms the long known presence of *Lucioperca* among the skerries of SW Finland. See also same periodical 1939, p. 59 and 1942, p. 135, on occurrences in the Gulf of Finland and in the Bothnian Gulf).

In the places mentioned, the salt content of the surface waters does not exceed 0.6 %. In the middle of the mouth of the Gulf of Finland the surface salinity is 0.57 %, slowly increasing to 0.91 % at a depth of 80 m. At the mouth of Viborg Bay the surface salt content does not reach 0.3 %. Far out in the midst of the open part of the North Baltic, northeast of Gotland, the surface salinity rises to 0.64 %, 0.94 % at a depth of 80 m, and about 1 % or slightly more at still greater depths (from a table by K. BUCH, 1945, p. 83, compared with his fig. 2, p. 28).

The pikeperch also occurs in the somewhat more saline southern Baltic, where it seems to migrate between the semi-enclosed Haffs and the open Baltic proper (GASCHOTT 1928, p. 58). Between the Island of Rügen and the Riga Bay even the surface salinity reaches 0.7—0.8 % (VALLEUX 1933, p. 654).

The limited distribution of the pikeperch in the Fennoscandian fresh waters is by some authorities explained as a result of the geological development during the time of its existence in this part of the world; in certain localities, for instance, the occurrences in southeastern Norway — the only ones in the whole country — the geological explanation seems indispensable.

Some authors hold that the present distribution at large of the pikeperch is mostly due to milieu (ALM 1942, p. 548). The northern limit of distribution east of Fennoscandia, as given on older maps (for example LUNDBERG 1899, pl. III, 17) suggests ecological factors — perhaps some temperature requirement. A modern map by a most outstanding authority (L. S. BERG 1933 b, fig. 11, p. 161) shows a distribution, to such an extent limited by divides between water systems that it seems explainable by the history of spreading rather than by present temperature conditions. (See EKMAN 1922, p. 299 about active advance upstreams when not steep.) For certain ecological information, the distribution outside northern Europe should also be considered — even in addition to what is shown on BERG's map, already mentioned (1933 b).

Lucioperca lucioperca abounds for example in the sea of Azow and in the Strait of Kertsh and occasionally is found even in the Black Sea, as at Anapa (L. S. BERG[1] 1933 a, pp. 626—629). According to ARCHANGELSKI, as quoted by D. VOLANSKY 1933, p. 397, the surface salinity in the north-eastern part of the Black Sea is nearly 1.4 %. BERG (loc. cit.) stresses the very considerable contrast between different races of pikeperch; even in some cases where no morphological distinctions have been noticed, biological race differences may be profound. To the writer's knowledge, the pikeperch of northern Europe does not thrive in waters nearly as salty as those of the Black Sea.

The statement sometimes made that *Lucioperca lucioperca* occurs also in North America is probably due to too wide a definition of species. Professor JOHN VAN OOSTEN of Ann Arbor, Michigan, in charge of the Great Lakes Fishery Investigations, has in a letter kindly informed me about the pikeperch in America: »The European pikeperch, *Lucioperca lucioperca*, is not found in North America. The three species of pike-perches inhabiting this country are all placed in the genus *Stizostedion*. The genera *Lucioperca* and *Stizostedion* are so closely related that American ichthyologists are coming more and more to the opinion that the two should be combined.

Our species most nearly comparable to your *Lucioperca lucioperca* is *Stizostedion v. vitreum* which is correctly called the yellow pikeperch. — — — None of the American pikeperches is found in brackish water. I know of no data on the exact temperature requirements of the yellow pikeperch. It seems to prefer cold water as the greatest abundance is in the northern part of its range.» (Athabaska Lake—the Hudson Bay region—Labrador).

Lucioperca is said to prefer waters with a low visibility. Some give as a reason that it could more easily get at its prey unnoticed (see f. i. ANDERSSON 1942, p. 548), the pikeperch itself being favoured by a special anatomical adaptation of its eye (EKMAN 1946, p. 422). Some experts seem more inclined to think that the abundance of plankton in certain muddy waters attracts the species. The scarcity or apparent lack of fossil remains in some of our Late-Glacial varved clays, even of micro-fossils, stated elsewhere in this paper, does not disprove the existence or even abundance of plankton. Many planktonic organisms dissolve completely even before reaching the bottom, of many others, the last traces may disappear after sedimentation. Note also how diluted any plankton necro-

[1] BERG's great work, in Russian, has not been accessible to me, but through the courtesy of prof. H. RENDAHL I have received a complete translation of the parts on *Lucioperca*.

coenose (concerning term see HESSLAND 1942, p. 31) must be when embedded in a minerogene sedimentation as abundant as when the bulk of our varved clays were formed. About the possible plankton content in the water in which our pikeperch lived we know hardly anything; that the water, at least at certain seasons, was exceedingly muddy is evident.

There seems to be some difference of opinion as to how deep or shallow waters the *Lucioperca* prefers, some authorities state shallow waters, others deep (see GASCHOTT 1928, p. 57—58). At any rate it is often found in lakes a few meters to some ten meters deep. When occurring in deeper lakes, in some cases very deep — ALM 1942, p. 548 even states that it prefers rather deep lakes — it seems, according to some authors to keep mostly near the shores (GASCHOTT 1928, p. 58), whereas, according to others, it is pelagic, seldom visiting the shore regions (ALM 1942, p. 549). As to the kind of bottom, it is said by some observers to prefer comparatively firm material such as sand and gravel, »jedenfalls darf es kein Schlammboden sein» (GASCHOTT 1928, p. 57—58). Our locality was decidedly »Schlammboden», which really ought to agree rather well with a fish showing such a preference for clayey waters (ALM 1942, p. 548). Besides, if the species is as pelagic as has been claimed, the kind of bottom under ordinary conditions ought not to be of such great importance except at spawning time. See below.

As for temperature requirements and temperature tolerance, a very wide range is indicated by the geographical distribution, reaching from north of the Polar circle in Fennoscandia to near the Mediterranian and south of the Caspian sea (BERG 1933 b, p. 161, map Fig. 11); biological differences in races should be considered (BERG 1933 a, pp. 626—629). The corresponding American form, the yellow pikeperch the northern limits of which have already been quoted, reaches as far south as Georgia and Alabama. Evidently *Lucioperca lucioperca* can survive long, hard winters, enduring temperature as low as + 4° C or less for many months of the year. The demand that the water itself be clean (save for mud) and rich in oxygen (ALM 1942, p. 549) was probably well satisfied in the Late-Glacial »sea».

The water temperature required for spawning may be especially informative. The season runs through April—May—June, depending on geographical position etc., often also as late as July (GASCHOTT 1928, p. 60, ALM 1942, p. 550). That the water temperatures during spawning are known to be around 12°—15° C may not necessarily mean that spawning would under all conditions be impossible at lower temperatures, but it makes it likely that in the spring or early summer such temperatures were reached or approached.

Those temperature estimates do not necessarily apply to the spot where our fish succumbed, which may have been rather far from where it began its life. The pikeperch strays over considerable distances, thus in Lake Mälaren marked specimens are known to have migrated from the westernmost to the easternmost parts (ALM 1942, p. 544, also personal communication); a distance about 100 km or more. Wanderings between enclosed bays and the open Baltic have already been mentioned, regular migrations in the waters of southern USSR are reported by BERG 1933 a, pp. 626—629. Note the considerable difference in behaviour between different races.

The *Lucioperca* is rather particular about its spawning ground. It may wander far for a suitable place: fine sand or clay bottom at a depth of about a metre or two. Sometimes it even swims up slow rivers for spawning (ALM, loc. cit. and personal communication, cf. also BERG 1933 a). Thus even if we can draw no conclusion as to whether the pikeperch really had a sufficiently favourable spawning place in the Late-Glacial Baltic itself or whether spawning occurred in some tributary system, rather high spring or early summer temperatures must have prevailed at not too great a distance from the ice border.

Even with rather high summer temperatures of the air, the Baltic itself cannot be expected to have been particularly favourable. The presence of melting icebergs far from the edge of the ice sheet is proved by their having unloaded numerous erratics including »micro-erratics» found embedded in otherwise quite undisturbed varved clay. Such erratics in the varves prove that icebergs drifted more than 100 km (how much more we do not yet know) before melting. The seasonal distribution of the icebergs is not yet sufficiently known, but their cooling effect must have been considerable.

As already mentioned, the scale structure supposed to mark years, was so vague on our Late-Glacial pikeperch that the experts consulted preferred not to make any statement as to age. Fig. 6 may give some idea of the appearance of that structure and of the degree of probability of the estimate of living age here made, the writer being fully aware of the approximation. Another question suggested itself: Why are the annual structures of those Late-Glacial scales so difficult to make out? Probably they were poorly developed from the beginning, the state of preservation seeming on the whole fairly good. Was the vagueness in the development of those annual structures due to the rhythm of the year being in one way or another less pronounced in the »sea» off the land ice? Perhaps already the size of the body of water, to some extent modifying the seasonal rhythm, also modifies the distinctions of the annual structure discussed?

Through the courtesy of Mr. A. ANDERSSON, district fishery inspector, the writer received scales from two pikeperches just caught in Baltic waters (Järnafjärden), to compare with those from fresh water lakes already at hand. No consistent difference in the way of development of annual pattern between pikeperches from lakes and those from the Baltic was suggested by the small material examined.

On part of some modern scales, be it from freshwater or the Baltic, the registration of years may seem almost as indistinct as in our Late-Glacial case. Needless to say, a real comparison cannot be made on stray samples, but only on a material large enough to be statistically satisfactory.

Even with allowance for hereditary differences (cf. BERG 1933 a) and statistical uncertainty, the rate of growth may give some hint about the living conditions. The rate of growth is rather different in different settings and has been subject to considerable study by E. MOHR 1916, also by DÖRCHNER a. o., GASCHOTT 1928, p. 60—61. As concluded from estimates related above, p. 209, it seems to have taken our specimen about 8 years to reach a total length of some 500 mm (to tip of caudal fin, or about 440 mm to Basis caudalis). That is about the time it takes a pikeperch in Lake Toften to-day to attain that size. In some Swedish lakes it grows considerably faster, pikeperches of Lake Ymsen reach half a meter in about six years (ALM 1942, p. 550). According to GASCHOTT 1928, p. 61 (quoting DÖRCHNER and others), about six years seem to be a normal age for a half meter pikeperch in German lakes, in rivers he would be older. In Lake Ilmen in Russia it takes a pikeperch only between 4 and 5 years, in Kuban River in Caucasia only 3 years to reach the same size as our specimen (some 440 mm to Basis caudalis). Even with allowance for uncertainties of several kinds we gain the impression that our Late-Glacial *Lucioperca* had grown considerably slower than the average of to-day, but that there are waters to-day where the growth is just as slow. Whereas the Late-Glacial didn't provide any luxuriant growth, the living conditions of the pikeperch may not have been much worse than in some places to-day, though naturally the balance between various factors differs.

There is little chance for a close analysis of the factors involved. Certain »threshold requirements» must anyway have been satisfied, there must, as already stated, have been waters warm enough for spawning. Sufficient nourishment must have been available for a fish, when young feeding almost entirely on plankton, when grown decidedly a fish of prey, living on smaller fish, crustaceae, etc. See MOHR's study of stomach content of modern specimens, quoted by GASCHOTT 1928, p. 58 ff.

Salinity, if any, was probably less than one percent, probably much less, considering the present distribution. As the degree of salinity is of great geological interest in this case, other biological indicators have naturally been searched for to sharpen the somewhat vague conclusion drawn from the *Lucioperca* find. Diatoms have naturally been looked for when examining the clay microscopically. When I did not observe any myself, I turned some fresh material over to the distinguished specialist, Dr. AST-

RID CLEVE-EULER, who kindly undertook a systematic search without however finding any micro-fossils. All she found was a few minute fragments of fish scales. See however about pollen and spores in a later chapter. As for biota, found elsewhere in the »younger Yoldia Sea», see MUNTHE 1940, p. 89—94 (also 1892, especially p. 102 ff.). Non-organogene confirmation of the fact that salinity, if any, can have been only very low will be discussed later.

The particular cause of the death and embedding of our pikeperch just where it was found remains an open question. It may have gone astray into too inhospitable waters close to the ice border where it finally succumbed, if because of starvation or for other reasons we shall probably never know. Incidently, if the connections suggested are correct, it seems to have died a year when the summer was exceptionally cool, judging from the thinness of the varve, suggesting an unusually small amount of melting. The deposition of that very year should, however, be studied over a much more extensive area in order to eliminate local factors. (Cf. DE GEER 1940, tables p. 268 and 304, pl. 72, 77, 80.)

6. The sediments and some indications they furnish on settling environment.

Not only a fossil itself, but also the sediment in which it is embedded, may furnish information about conditions at the time of deposition. »Unsere Kenntnisse der Eiszeit und ihrer Geschehnisse gründet sich in erster Linie auf die Bodenarten, die in der Eiszeit und ihrem Ausgangsstadium entstanden sind — — —» (LEIVISKÄ 1941, p. 153). As in this case particle size distribution seemed likely to yield informative data, some mechanical analyses were made — a detail of a more comprehensive study concerning different aspects of the Quaternary of the Uppsala region. Naturally the problems of clay cannot be reduced to size only, but particle size is nevertheless one of the most important characteristics of a mechanical sediment and one of the main indicators of its settling environment. The limitations admitted, size determination remains a suitable opening for a close study of such sediments.

Laboratory methods. With one exception, No. 219, the samples were dispersed without first having been dried. Dispersion was accomplished by soaking and manual brushing, further automatic stirring, rodding, and shaking the sediment in water — or rather in the very diluted peptizing solution referred to below. The result was checked microscopically. As no sign of clusters could be detected after a standard stirring, shaking etc., I consider the dispersion to have been practically complete. Microscopical checking was renewed now and then during the operations. At the first sign of flocculation, noticed either by the naked eye or through the microscope, the analysis was interrupted either for good, or to be resumed after satisfactory re-dispersion.

For the analysis, the pipette method was used, with sodium oxalate as peptizer (concentration N/100). Except for minor modifications, the procedure followed was that recommended by KRUMBEIN and PETTIJOHN (1938, p. 166).

After each completed pipette analysis, the residium in the sedimentation cylinder was washed through a sieve with 0.06 mm openings. If anything remained on the screen, it was further sieved in standard manner (also coarser sieves, with close intervals in sizes). The material washed through the 0.06 mm screen was dried at 100° C and weighed. After subtraction for oxalate content, the original dry weight of the analysis sample itself was computed by adding together all that had once been included in it. Already from the very outset of the preparations for an analysis, the dry weight of the weighed wet sample was preliminarily computed by determining moisture content on a parallel sample, used for that purpose only. The percentage figures given, refer however, as already stated, to the total net weight of the sample analyzed.

A double analysis was made for each number, the mean of the corresponding values used in constructing the diagrams. When necessary for further checking (there were some disturbing differences in the first double sets), or when a more detailed fractionation seemed desirable, the method described by HÖRNER, 1946 b, was used. In some cases, certain »narrower» detail fractions of an analysis were taken together and presented as joint fractions in the histograms instead of rechecking every detail individually. Some details may have been lost through those generalisations, but the method used does not warrant unlimited detailization.

As always when dealing with pipette analysis, the limitations and weak points of the method should be kept in mind as well as its obvious advantages. Allowance should be made for the possibility of some inexactness, especially in the coarsest and the very finest fractions. For all fractions remember that by the multiplication of the dry weight of each individual pipette sample — or difference in dry weight of two successive pipette samples — necessary in order to compute percentage of total, any small inexactness is multiplied correspondingly. The smaller the total sample suspended in a standard volume, and, particularly, the smaller the difference between two consecutive pipette contents the greater the possibilities of error. On the other hand, the more concentrated the suspension, the more acute the risk of coagulation. Carefully made, a pipette analysis, though far from ideal, seems however to favourably stand a comparison with other methods. There is still room for much improvement.

Hygroscopicity (W_h) in this paper means the water content (in percent of dry substance, cf. below) of the sediment when fully saturated by vapour over a 10 percent H_2SO_4 in vacuum amounting to approximately 15 cm Hg at about + 20° C. The determination was made in the general way described by EKSTRÖM 1927, pp. 87 ff., though using a bigger exsiccator.

The height of the sample above the surface of acid, the changing of acid, and the time of treatment were in agreement with EKSTRÖM 1927. However, for the subsequent drying no heated vacuum exsiccator with phosphorous pentoxide (EKSTRÖM 1927, p. 88—89) was used. As a final operation the samples were only heated, under normal atmospheric pressure, at + 100° C, to constant weight, which was then, without further correction, considered the dry weight of the sample. To be directly comparable, at least approximately, with W_h values referring to dry weight as given by MITSCHERLICH and his followers (see EKSTRÖM loc. cit.), our W_h values should be multiplied by the factor 1.07 (EKSTRÖM 1934, p. 8). Our figures are, as stated above, given without introduction of any correction factor.

Originally I had also planned some determinations of calcium and carbon content. When A. G. HÖGBOM (1889) made his important study of the relation between $CaCO_3$ and $MgCO_3$ in the Uppsala region there was not yet any geochronological frame available for specifying the varves. It would be of interest to make additional determinations in different dated parts of the varve series. All plans on even rather simple chemical determinations were, however, abandoned when I learned that a colleague, Mr. G. ARRHENIUS, was taking up a thorough and far more complete and expert study of the chemistry of the varved clays, a highly interesting and very promising subject. The results became available (ARRHENIUS 1947) when the manuscript of this paper was already completed. Important observations and conclusions by ARRHENIUS, with special bearing on our subject could, in the time available, only be shortly considered and discussed in a few later additions, worked into the places concerned of an already completed text.

Certain constants of the clay, of interest to this study, unfortunately could not be determined immediately when the samples were taken, as they ought to have been, nor were any samples kept air tight. The sample was already advanced in the process of drying when the volume weight was found to be 1.8 and out of that there were some 1.3 grams of dry substance per cubic centimeter. The clay, however, had already then been subject to a linear shrinkage of about 8.3 % in the direction perpendicular to the varve surfaces. The original thickness of the varve, when the samples were taken, was about 1.09 times the thickness when the volume weight etc. was determined. If shrinkage was proportional in all directions (which is not likely to hold exactly true), the volume of the original sample should have been some 1.3 times larger than when examined, without any change in amount of dry substance. Thus there should have been, when the sample was taken, some 1 gram dry substance per cubic centimeter. Provided that the density of the solids is, as seems likely, about 2.65, the volume weight of the sample (at the time of sampling), under above conditions, is computed to have been 1.63, the volume of voids (total volume of voids)/(total volume of sample) = 0.62, voids ratio (total volume of voids)/(total volume of solids) = 1.65. As these figures are not immediately determined but involve computing with two somewhat uncertain factors, they can only be given as approximate. The density of a sediment is a factor that necessarily must be taken into account in many types of study — including certain geochronological investigations, as will be evident from some considerations later on in this paper.

As no assistant was available, I had to do the laboratory work myself, including all routine operations, in the rather limited time at my disposal for the purpose.

The sedimentological study in the process of being carried out on older and younger varves cannot be considered in this paper but must wait a later publication. In dealing with the problems involved in such a study, one becomes more and more aware of the need of further experimental research — for instance on the rôle of coagulation in sedimentation.

Average annual sedimentation. Particle size distribution. A general idea of the size frequency distribution of the sediments surrounding the fish give the analyses 213 and 219 (fig. 9), two different samples taken at some distance from one another — each sample cut as a vertical prism of even thickness through the varves −729—−740. (See fig. 2, p. 203 and Fig. 10, p. 232.) Number 219 had been dried before the analysis, number 213 had not. With the exception of that small part coarser than 20 μ, the agreement between the two samples is fairly good. The differences in the coarsest fraction cannot yet be definitely explained. Probably the horizontal distribution of the sparse »mo» is not quite even in the strata concerned. On the other hand, the difference in the analyses may partly be due to some sampling or laboratory error.[1]

Diagram 219, fig. 9, shows, through the minor accumulation of material of some 30—60 μ, a tendency to a double maxima distribution.

[1] In spite of the precautions taken, part of some coarse layer may have fallen out in cutting the prism. Note also that pipette determinations for fractions coarser than some 30 μ often tend to be unsharp because of the short settling time and the difficulty in getting the beginning and end of that time quite exact. Turbulence disturbs the starting moment, the time it takes to fill the pipette in a satisfactory way complicates the closing of the period.

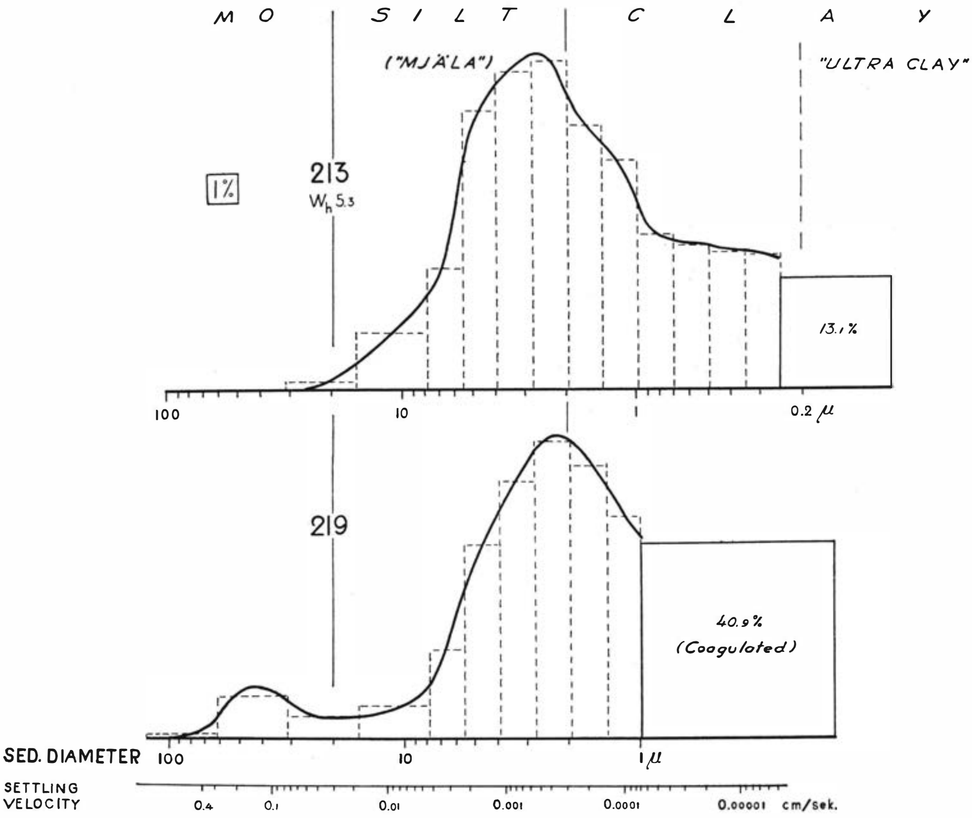


Fig. 9. Frequency diagrams for two analyses of general samples representing the whole of twelve years, −740—−729. To facilitate comparison with ATTERBERG diagrams, the main ATTERBERG classes are given above. Particle size is expressed as sedimentation diameter (diameter — according to STOKES' law — of sphere of density 2.65 settling in water with same velocity as particle studied. The complications and consequences of shape factors, though not here entered upon, should be kept in mind.) At the foot of the diagram, the corresponding settling velocities (at analysis conditions, + 20° C). The velocity scale simplifies comparisons with the scales of ROBINSON, RUBEY and others, see KRUMBEIN and PETTIJOHN 1938, p. 83. (As RUBEY measures velocity in microns/sec and we in cm/sec our values have to be multiplied by 10^4 to be directly comparable.) When using our velocity scale to vizualize Late-Glacial settling conditions, the temperature must be considered.

Within the applicability of STOKES' law, settling velocities are approximately inversely proportional to the viscosity of water at the respective temperatures:

$$\frac{v_{t_1}}{v_{t_2}} = \frac{\eta_{t_2}}{\eta_{t_1}}.$$

The viscosity coefficient is some 0.010 at + 20° C, some 0.016 at + 4° C. (Sample 219 coagulated at a late stage of the analysis and no steps were taken, in this case, to secure an analysis beyond 1 μ).

Because of the pronounced seasonal differences, double topped distribution diagrams would be expected to occur frequently among varved clays. Such distributions are in fact excellently illustrated by S. GRIPENBERG's considerably coarser sample F 31 (1934, p. 191, 193), also by samples F 65

B 1925 b and F 43 B 1925 b of some author (l. c., p. 194). With the exception of the little bump at some 30—60 μ, in diagram 219, the total particle size distribution of the varves closest to the fish seems to be dominated by just one maximum, corresponding to a particle diameter of 2 to 3 μ. Somewhat more than half of each of the general samples 213 and 219 consists of particles finer than 2 μ. In grain size and sorting (though not in other characteristics of the distribution) our material somewhat resembles some of Miss GRIPENBERG's Late-Glacial varved clays from the comparatively deep parts of the Bothnian Sea and the Gulf of Finland, see especially her F 54 1924, p. 194 (that distribution is, however, double topped). Certain other of GRIPENBERG's samples of glacial clays show just one maximum each (such as F 30, 1925, F 33, 1925 c), some are however on the average somewhat finer than ours (see loc. cit.). The differences in general tendency of the skewness should be noticed, GRIPENBERG's distributions are drawn out especially towards the side of the larger grains; the greater part of our general samples lies on the finer side of the modal diameter. The »step» on the fine side of our distribution 213 may deserve further attention.

Compared with most analyses in SAURAMO's (1923) classical work on the varved sediments of southern Finland, the general samples (213 and 219) in this paper show a higher degree of sorting, probably because they represent a smaller number of varves.

The average fineness of the sediments surrounding the fish indicate rather quiet conditions; the good sorting even in such general samples as 213 and 219, representing the accumulation of no less than 12 years, suggests that the variations during seasons and years kept within comparatively narrow limits in our part of the once »sea». There is, as one might expect, a striking contrast to the examples given by HJULSTRÖM (1935, p. 403 ff.[1]) of mechanical composition of the sedimentary load of the present day little local river, slow-moving though it mostly is, carrying reeroded material from glacial and postglacial sediments of the surrounding plains. HJULSTRÖM's examples not only show, on the whole, a considerably coarser material, but as far as his analyses go (to sedimentation radius 2 μ, thus sedimentation diameter 4 μ) they also, though momentary samples, indicate (with the possible exception of the samples from the very quiet summer low water) a considerably more incomplete sorting than our 12 year representation. (The summer low water sedimentary load, here referred to, according to errata by HJULSTRÖM himself, was taken 30/6 1933; compare also ibidem, p. 513.)

[1] Compare also the information given for the days concerned in the table at the end of HJULSTRÖM's monograph.

Amount of annual accumulation. The total thickness of the twelve varves together (−729 to and including −740) at the time and place where the fish was found measured 16 cm, the average thickness pro varve thus being some 1.3 cm. As already stated, each cubic centimeter of clay in that state contained only about 1 gram of solids. Thus on each square decimeter of that part of the »sea» bottom settled then, on an average 130 g of solids a year, 55—59 % of it, say, some 75 g, clay proper, sensu ATTERBERG (finer than 2 μ).

Some considerations on the sedimentary load of the water. There are still too many unknown factors and too little accurate information to justify any attempt at a more pretentious, complete treatment here, of the complex problems of sedimentation in the »sea» off our retreating ice border. Among factors influencing sedimentation are: amount and kind of material carried out to the »sea», the dimensions (including depth) of that »sea» (especially depth at the localities concerned), the movements in that body of water, its temperature, electrolyte content and pH (cf. TAMM 1925, p. 4 ff.), hydrographic stratification, seasonal changes and their consequences. A crucial point is the state of dispersion or flocculation in which the small particles were transported and settled.

Any addition to the fund of adequate, informative facts concerning water or solids involved in the formation of our varved clays is likely to be of use for further study along theoretical and empirical lines. A full treatment of the problems of the varved clays of a representative region should be inviting to a sedimentologist with the necessary qualifications and resources of time and equipment; the varved clays promise to give some clue to certain general principles of sedimentation.

One of the suitable starting points for a discussion of the sedimentation in our special case would, of course, be an estimate of the sedimentary load of the contributing rivers, especially the nearby glacial rivers. With such an estimate as a goal further quantitative data on the total amount of Late-Glacial deposits over an adequate area are being collected, but do not yet suffice to give even rough figures of reasonable dependability. Nor is the hydrology of the waning land ice sufficiently well-known for an estimate of the concentration of sediments in a glacial river and at its mouth.

In this special paper, only that part of the sediment reaching to and beyond the resting place of our *Lucioperca* will be considered, and in the form of some very simplified consideration only, based on the limited facts available and on some admittedly crude and too generalized assumptions. It is hoped, however, that advancing the discussion on some points may help to call attention to the necessity of further research in a field wide enough for several investigators.

If the sediment particles, the size distribution of which is given in analyses 213 and 219, had been completely dispersed in the water and (to the total annual amount already stated) evenly settling as independent individual units in a motionless body of water, it would be possible to compute the concentration necessary to provide for that sedimentation.

Of the comparatively fast-sinking larger particles quite a low concentration would evidently be enough to secure the small amount deposited, of slow-sinking, finer particles a considerably higher concentration would be needed.

That every particle settled independently is however most improbable (cf. also ANTEVS 1925, p. 32, BRENNER 1925, GRIPENBERG 1934, p. 210); some coagulation is likely to have taken place, though the study of general samples and average composition gives no clue as to the extent. The complicated question of coagulation will be slightly touched upon in a following subchapter. Here, trying to form some idea of possible suspensional concentration we shall permit ourselves very considerable simplifications even to the extent of disregarding for the moment certain rather important points of the theory of coagulation.

Within limits, coagulating fine fractions of a polydisperse suspension sink faster the higher the electrolyte content of the liquid. Clusters of fine particles sink with a velocity characteristic of larger individual particles — the sedimentation diameter being larger, the higher the electrolyte content. There will be combinations of electrolyte contents and suspensional concentrations where a polydisperse system containing a considerable amount of very fine particles clears up as if the content of particles finer than 2 μ was negligible; other concentration combinations cause an originally similar suspension to clear up as if 1 μ were the lower limit (consider for instance the consequences of DREVESKRACHT and THIEL's, 1941, p. 693, table showing time required for sediments to settle 10 cm). The limit between very muddy suspension and almost clear liquid is often (though not in DREVESKRACHT and THIEL's experiment) surprisingly sharp and sinking regularly, giving the impression that one certain settling velocity (corresponding to some definite sedimentation diameter) dominated the clusters of fine particles.

Though, as already stated, a closer study of the coagulation reveals a considerably more complicated process, some simplified assumptions pertaining to the above observations may suggest a way of getting — once sufficient data become available — some idea about the concentration of the suspension from which a sediment of known composition was deposited.

The maximum settling velocity of individual clay particles (sensu ATTERBERG: 2 μ) in water at $+4^\circ$ C (a likely temperature of the bottom water) would be something like 2.3×10^{-4} cm per second (under standard laboratory conditions, $+ 20^\circ$ C, some 3×10^{-4} cm/sec), as computed from STOKES' law. Now, suppose, admittedly unrealistically, that all the clay settled by this maximum velocity and that the settling went on with equal intensity all the year around (which evidently it could not have done). Disregarding, for the time being, the material coarser than clay, let us consider what the concentration of the clay suspension under the above assumptions would have to be in a cube of water $1 \times 1 \times 1$ dm just above the sedimentation surface.

At 4° C it would take some 12 hours (at 20° about 8 hours) for those maximum speeded clay particles, at a certain moment contained in a decimeter cube of water, to sink to the bottom. As there are some 8760 hours (or 730 twelve hour periods) in a year, the clay deposition in 12 hours would represent some 0.14 % of the »clay income» of a whole year, that is, for a square decimeter 0.14 % of 75 g = some 0.1 g. In the decimeter layer just above the bottom, the water thus would under the above conditions, have held in suspension some 100 mg of clay. To secure such continual deposition, during a quiet period with no lateral transport, the bottom water just mentioned would have to be fed by an equal amount of clay from above — which means a corresponding concentration in those layers.

As an individual clay particle of maximum size would sink not quite 90 m in a year, and the depth at our locality seems to have been some 120 to 150 m, such particles could not sink from the surface to the bottom in one year; in the deeper part of the water, clay may have been carried in suspension all the year around, even when the upper layers cleared up during long quiet periods (winter) with little or no contribution of additional material. E. M. KINDLE (1930, p. 85) has experimentally proved that something similar actually holds true for the little present day Lake Cavell in the Canadian Rockies: »the water in the deepest part of the lake — — — would, if no fresh sediment were introduced for one year, still retain enough sediment in suspension near the bottom to render it very murky». (In Lake Cavell, sediments and water alike are practically free of $CaCO_3$, and the electrolyte content of the water seems to be very low.) As S. HANSEN, 1940, p. 375, 465, points out, both FRASER and ANTEVS consider that with great depth and pure water not much of the finest detritus will sediment in the course of a winter.

(The statement, in previous paragraph, about the time required for certain particles to complete their sinking from the surface, is made with the assumption that the sedimentary material was really distributed from surface to bottom in our part of the then Baltic. If the discharged glacial water, heavy in its coldness and through its load of sediments, spread only as a bottom layer under the Baltic water (DE GEER 1909, p. 320, 1912 a, p. 250), the maximum height of settling would have been much less than the total depth of the water, and the time required correspondingly reduced. Theoretical reasons (ANTEVS 1925, pp. 36 ff., 1932, pp. 314—315), supported by actual observations (JOHNSTON 1922, p. 381—382), indicate however that under temperature and density constellations characteristic of glacial lakes, the released melt water formed no bottom layer. Instead it seems to have entered a circulation involving, at least in certain seasons, the main part of the vertical section and thus to have spread the fine fractions of its sedimentary load accordingly (see ANTEVS 1925, fig. 21, p. 41). It should be remembered that the simple, uniform sedimentation assumed in some of our calculations is a theoretical construction only, in reality there must have been considerable complications — for instance through seasonal changes in the thermal stratification.)

If part of the sediment under conditions free of disturbances settled more slowly than 2 μ particles, the concentration, other conditions unchanged, must have been greater than calculated for that size. According to our two general analyses, No. 213 and 219, see fig. 10 and cumulative diagram, fig. 14, some 35—40 % of the material was as fine as 1 μ or finer. Suppose that 35 % of the average annual accumulation, or some 45 g, settled with a velocity characteristic to 1 μ, say, some 5.8×10^{-5} cm per second. It would take some 48 hours, or 1/182 (= 0.55 %) of a year to clear a decimeter cube of water of its 1 μ particles. If we assume, as above, that in a year to each square decimeter of the bottom 45 g ot sediments arrived with the falling velocity of 1 μ particles, a cubic decimeter of water must have contained 0.55 % of 45 g = 0.25 g of such particles. The concentration of the fraction sinking like 1 μ particles should thus have been some 250 mg a liter. The considerable amount of material, because of its larger elementary particles necessarily sinking faster, must have substantially added to that concentration. (For a local comparison, it may be mentioned that even in its muddiest times, swelling after rain, the nearby muddy little river of to-day, the Fyris, hardly ever, or at least very seldom, reaches a similar concentration of sedimentary load. In the five years in which the load of solid matter of the Fyris was daily meausured by HJULSTRÖM, see HJULSTRÖM 1935, such high concentration was never observed. Only two days in those five years was a silt content over 200 mg/l registered, three days between 100 and 200 mg/l (loc. cit., tables pp. 455—525, and plate VIII, concerning sedimentary loads of present day glacial river see for instance SAYLES 1919, pp. 31 ff. and references there). If, in our Glacial clay, particles even smaller than 1 micron settled individually, the concentration must have been still greater than computed for the case »1 μ settling velocity» above. In certain types of clays, the sedimentation diameter of aggregates can hardly have exceeded 0.5—1 μ (GRIPENBERG 1934, p. 211.)

(In connection with the discussion, elsewhere in this paper, of the likelihood of biota in the waters close to the land ice, it may be worth considering the effect of the muddiness on the photosynthesis.)

Certain statements earlier in this chapter were made under the assumption that even clay particles 2 or 1 μ, respectively, settle individually. Flocculation to faster clusters would have fundamentally changed the aspect. As ANTEVS (1925, p. 32) points out (referring also to data by ODÉN 1916 and 1918), from a high content of very fine material »it is evident that most of the small particles did not go down separately — — — but flocculation occurred».

The average composition of a series of varves will hardly give any clue to the rôle of coagulation, but the different strata within a varve may.

Before tackling that question, it may be of some interest to consider not only the sedimentation at a certain spot, but also the average amount of sediments annually carried further; past that locality.

Amount of material that annually passed the *Lucioperca* site. Microvarve measurement in Stockholm, performed by GERARD DE GEER (1940, p. 304, see also p. 256, and map, Pl. 77) make it possible to compare roughly the varves measured by several observers in Uppsala with the sedimentation in Stockholm in the corresponding years, G. D. G. −740 —−729, (provided of course, that the connections are indisputable; note how extremely difficult it is, in many cases, to be sure about some varves included in such micro series).

The mean thickness of a varve in that set was in Uppsala, at the locality of the fish, as previously stated, 13 mm, or, to be more accurate, 13.3 mm. DE GEER's mean from other localities in the Uppsala region would be 12.2 mm; the corresponding mean for Stockholm 1.9 mm, computed from his figures 1940, p. 304. As already pointed out, for a more exact comparison of the amount of sedimentation in different localities, a mere measurement of the thickness of varves is not enough; the porosity, or the degree of compression must also be taken into account; it is the actual amount of solid phase that counts.

If the thickness of the varves so far out in the sea could be assumed to decrease in the same proportion as the distance from the ice edge increases, the clay varve would thin out to nil some 80 km south of Uppsala (and at a distance of about 115 km from the ice border). In reality conditions have, of course, been much more complicated. The thinning out of the varves distally is more likely to approach zero asymptotically, clay thus being deposited at a still greater distance from the ice border. Already an extension of the clay varves over 100 km in distal directions seems to indicate freshwater conditions or almost so. As SAURAMO (1923) points out, symmict varves are narrow, in one case stated by SAURAMO, loc. cit., p. 108—109, only reaching 25—50 km, whereas diatactic varves are broad, from 100 to 150 km in this case given for comparison. In other regions even broader; very widely extending clay varves are exemplified by DE GEER 1940.

There were also most likely great irregularities in the distribution of the varves. Note how different the Post-Glacial sedimentation has been in different localities in the Baltic and adjoining seas; in many places, in rather different depths, Late-Glacial clay still practically forms the sea bottom, Post-Glacial deposition having been practically nil (see GRIPENBERG 1934, fig. 27, p. 146, also for instance characterization of sediment and locality F 54, 1924, loc. cit., p. 111), in many other localities the sea bottom consists of clearly Post-Glacial deposits. Though in many respects different from conditions in larger and deeper waters, even modest lakes and their sedimentary distribution may offer some analogies. G. LUNDQVIST, identifying and tracing synchronous levels in lake sediments, has strikingly proved (though for much shallower waters than ours) how far from even is the sedimentary cover of any period. (See LUNDQVIST 1924, 1925, and many subsequent works; for further reference see bibliography in LUNDQVIST 1942.)

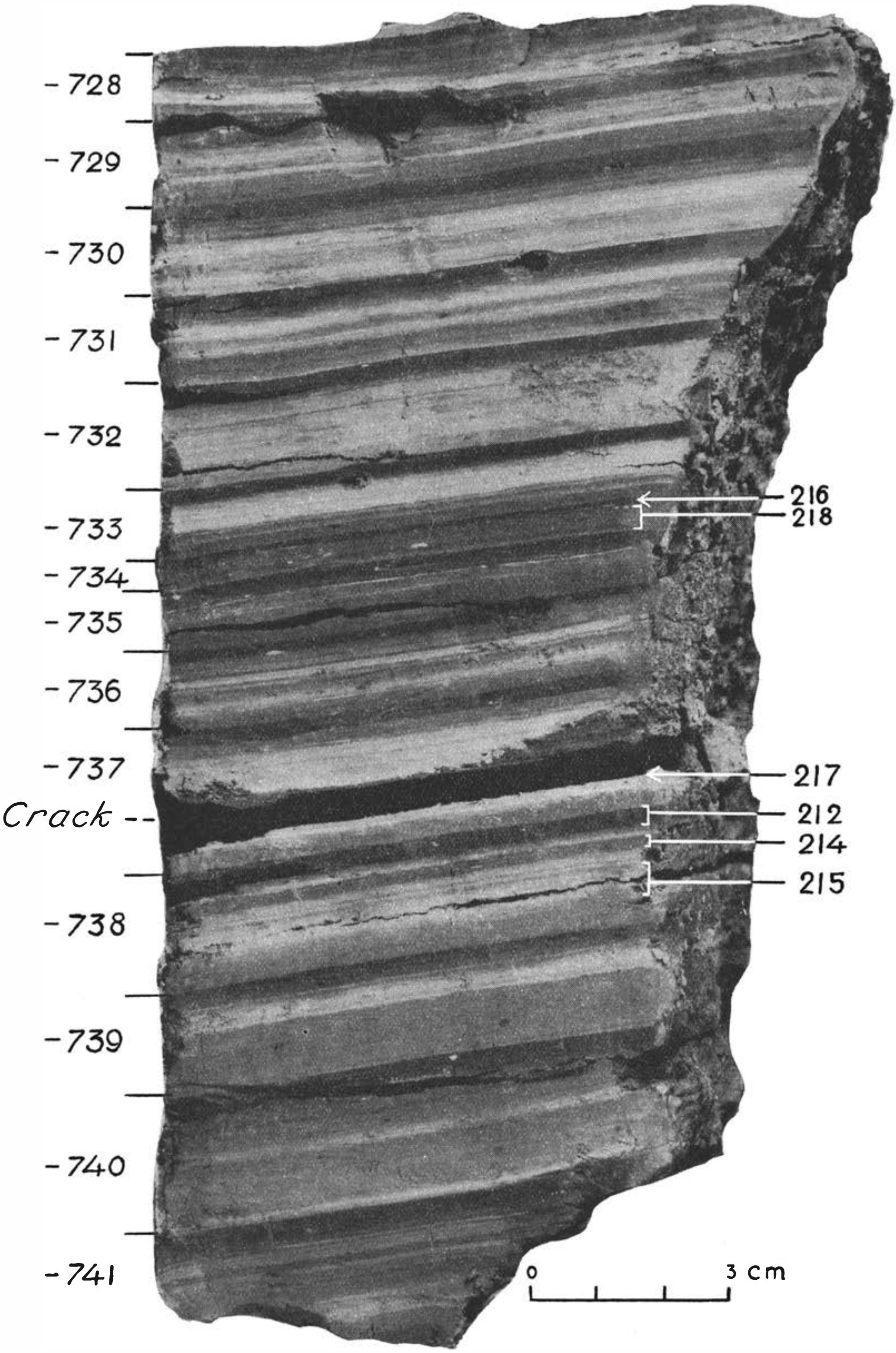
Our assumption here of a regular sedimentary cover of, in distal direction, evenly decreasing thickness is thus a theoretical generalization only, intended to give some idea about the order of magnitude of the amount of sediments that passed our *Lucioperca* locality at the time under discussion. In a vertical section distally from Uppsala through the Stockholm region, maintaining the above assumptions and with the clay layers complete, the mean sectional area through one varve in the group concerned would be some 500 square meters. Beginning at Uppsala, a strip in distal direction, one decimeter wide, would contain some 50 cubic meters of sediments, or some 50 metric tons, with the voids ratio as found in Vaksala. Over each decimeter perpendicular to the general direction of distribution of the sediments there would, where Uppsala now stands, have passed some 50 tons of solids in a year. If the sea was some 120 m deep, there should have passed, on the average, some 40 kg a year, or, say, approximately 100 g a day through each vertical square decimeter perpendicular to the general direction of transport, provided that the material had been evenly distributed through the entire vertical section. Thus through the space of a decimeter cube close above the bottom would have passed on, in distal direction, some 250 times as much solid material as the amount that had sunk to the bottom right there. If the sedimentary transport had taken place through bottom currents only, the amount passing the space stated would have been still larger.

Stratification within the varves and seasonal variation in sedimentation.

As structural details within the varves seem likely to throw some further light on conditions at the time of settlement, such details deserve attention in this connection, even if unimportant in chronological research (G. DE GEER 1940, p. 17). The individual varves in the series discussed differ considerably from one another, not only in thickness but also in structure, even though the most fundamental characteristics are similar. (For general principles of formation of glacio-aquatic varves, see handbooks or some of the references quoted in other connections such as DE GEER 1940, SAURAMO 1923, ANTEVS 1925, S. HANSEN 1940). Many details, hardly or not at all distinguishable when the clay is saturated with moisture (fig. 2), become much better visible during the process of drying (fig. 10), partly to tone down again as the drying out is nearly completed. Drying has by no means the destructive effect on our varves as on those of WALLACE (1927, p. 110).

The most striking and most easily observed features in the clay stratification here under discussion are the colours and colour contrasts. An adequate characterization of the colours is nevertheless difficult — they depend very much on the moisture content of the clay, and also, of course, on the light — not to speak of how different different observers see them and how indefinite and varying the very terminology of colours in nature still is. Statements in this paper about colours of clay should be taken with allowance for above imperfection — they only pretend to give a general idea.

As a rule, at least when the material is still wet or moist, the winter layer, dark gray, in some cases »brownish black», shows up as the most well-defined stratum of any varve of our set. It stands out against the much lighter larger part of the varve with its lighter brownish, in some cases almost reddish colours and light gray shades. The thickness of the winter layer of our particular varves (−740——729 at Uppsala) varies from about half a millimeter to three and a half millimeter. Already the most preliminary field observation reveals a »stiff», fine clay — in many cases strongly contrasting to siltier parts of the same varve. The upper surface of the winter layer is quite sharp and distinct — separating two different varves. In some cases it is directly overlayed by a light gray, somewhat silty layer, belonging to the coarsest parts of the following varve, see for instance in fig. 10 the bottom part of varve −740 or −730. In some other cases, the bottom part of a new varve is chocolate-brown or greyish brown »intermediate» clay, see for instance varve −739 in fig. 10. The intermediate gray colours of that picture mostly represent the brownish tints


-728
-729
-730
-731
-732
-733
-734
-735
-736
-737
Crack --
-738
-739
-740
-741
216
218
217
212
214
215
0
3 cm

of the original. In several of our varves, the bulk of the coarsest material, light gray, in extreme cases almost white, occupies a more central part, in some cases separated also from the overlying winter stratum by a brown »grading over» zone; see as an example −737 in fig. 10. In some varves, for instance in the exceptionally thin −734, practically no really light-coloured material shows up. In many varves, even in their otherwise brown parts (but, as far as my experience goes, never in winter deposits, see also HÖGBOM 1889, p. 264) there occur quite thin, gray silt layers, as a rule considerably less than a millimeter thick, in many cases only a small fraction of a millimeter. In some varves the drying brings out a very detailed lamination, see for instance the part immediately underneath the winter layer of varve −741. Very likely it would be possible, by close observation and suitable enlargement under ideally favourable conditions (just the right stage of drying), to make out dozens of diminutive lamina within certain varves (see, though for considerably thicker varves > 3 cm, also HÖGBOM 1889, p. 264, who counted more than 100 very thin layers), whereas some other varves show little or almost no tendency to similar miniature structures.

Already immediate visual observation and some simple field tests of consistency (ATTERBERG tests, etc., see EKSTRÖM 1927, pp. 136—138) indicate a difference in particle size between the dark winter layers and the light gray parts of the varves, that seasonal difference stated and stressed by so many others for varves in different regions. The contrast between winter and summer deposits is not a question of particle size only, there are also chemical (and mineralogical) differences, exemplified by HÖGBOM's studies 1889, 1892 (cf. also BERGSTRAND 1859, p. 124, quoted by HÖGBOM, cf. also STOLPE 1869) on the $CaCO_3$ and $MgCO_3$ content in the clay. The comparatively quickly deposited summer layers contain much $CaCO_3$, whereas in the long suspended, slowly settling winter material $CaCO_3$ had time more or less to dissolve. The low temperature of the winter also increased the rate of solution (GRIPENBERG 1934, p. 180). As dolomite, $MgCa(CO_3)_2$, was much more resistant to dissolution, the ratio used by HÖGBOM for comparison: parts $MgCO_3$ per 100 parts $CaCO_3$, proved much higher in winter than in summer deposits. A more modest decrease of lime in winter deposits is shown in the Canadian varves of WALLACE 1927, p. 111. In some Danish varves, thoroughly studied by S. HANSEN, 1940, p. 369 ff., there are no corresponding differences between the content of lime in winter and summer layers, the

Fig. 10. Some varves in drying, showing some details of varve structures (though somewhat confused by drying cracks). Figures to the left: numbers (years) of the individual varves, as connected to DE GEER's chronology (1940). Varve −738 was pointed out as approximate position of the fish, varve −734 probable exact position. Figures to the right: numbers of special samples mechanically analyzed. Analyses 213 and 219 general samples (even, vertical samples) through 12 varves, −740—−729. Photography by N. HJORT.

reason for this is discussed by HANSEN. It should be added that the variations in $CaCO_3$ content do not explain the colour differences within the varves, they must be caused by other factors (HÖGBOM 1889, p. 273, who also suggests a possible explanation). Other chemical differences between summer and winter parts of varves are exemplified and discussed by WALLACE 1927, p. 111, though the influence on the colour pattern of the varve is not taken up by him.

Even with every allowance for chemical differences, and though it is quite clear that particle size distribution is only one of the characters in which the detail layers within a varve differ, that in itself is nevertheless important enough to warrant a study, especially when the underlying purpose is settling environment. Among the many kinds of differences within the varves, particle size seems especially likely to yield such information.

Some of the detail stratification within varves — especially some detail features depending on particle size — may perhaps be explained by local and occasional shifting of currents in the body of water outside the edge of the land ice. But the main reason, direct or indirect, for the structures, major and minor, of the varves must be climatic. Most important is the difference in melting, causing increase and decrease in the discharge and force of the glacial rivers. For formation of the winter layer, the freezing over of the surface of the sea, preventing any wave movement, must have highly contributed to the quiet settling conditions.

A rather detailed granulometrical investigation of some characteristic individual minor strata and groups of such strata within varves (cf. also ODÉN 1920, pp. 336 ff.) seemed, as already stated, the most promising beginning of a further study of the once settling environment. Thus some rather thin sections parallel to the stratification and varying in thickness between a millimeter or less (in the case of silt of flour consistency and of one extraordinarily thin winter layer of stiff clay) and some 3—4 mm, have been painstakingly cut or scraped off for examination. To secure material enough for that operation, as large samples as could conveniently be handled »undisturbed» (extention parallel to strata some 3 square dm and thick enough to well include all the 12 varves under discussion) were brought in boxes directly from the clay pit to the laboratory. There thin slices parallel to the stratification were taken out with considerable care so as to represent typical constituent layers within the varves. Even within individual detail sections as thin as those examined, variations in settling conditions may be represented. I have consistently tried to get these detail samples out of as uniform parts as possible of the varve fractions to be represented. See fig. 10, p. 232.

Layers of comparatively coarse material. In dealing with the different types of strata constituting a varve, it seems natural to begin with the coarsest material, suggesting the highest velocity in the transporting water. Such material of relatively large particles gives the general impression of being more »proximal» than the clay immediately below and above, though actually being deposited no nearer the mouth of the glacial river.

Coarsest appearing of any stratum within the 12 varves examined is a silt layer, hardly a mm thick, in the middle of varve −737. When the clay dries, it shows a marked tendency to crack and part along such layers; in fig. 10 the black band in varve −737 is due to the opening of such a crack along the layer concerned. The silt on the dried up parting surface then looks and feels like very fine flour. The little coherence there is — if any — between the particles, is by the slightest touch of a finger broken up, leaving a fine, loose whitish rock flour without cohesion, though the mechanical analysis shows some 20 % of clay, sensu ATTERBERG (particles $< 2\,\mu$) all of it fairly coarse clay, $> 0.5\,\mu$. In the silt calcium carbonate dominates.

Some of the irregularity of the size distribution diagram fig. 11 may be due to sampling imperfection in isolating the very thin silt layer from the nearest material above and below, and to imperfection in the analysis. Because of the thinness of the layer, too small a sample was available for sedimentation analysis to secure desirable detail accuracy, but the main features of the distribution are unmistakable. The material is comparatively uniform in size, shows a high degree of sorting. The modal particle diameter is about 5 microns or slightly less. There is a certain but not very marked tendency to skewness in the distribution, the frequency curve being somewhat drawn out towards the finer side, without however reaching the very small particle dimensions. In fact, the absence of really fine material seems rather remarkable for a layer in the midst of so distal and generally fine grained a facies of our varved clay. The sparsely represented finest constituents in the silt layer studied are between 1 μ and 0.5 μ; there is practically no trace of anything finer than 0.5 μ; whereas an average sample of our clay including several varves, to some 25 % is made up of still finer material. (It should be remembered that in all analyses for this paper the samples were thoroughly dispersed and the dispersion repeatedly checked microscopically.) That the water at the time of deposition of the silt layer under discussion should have lacked particles finer than 0.5 is hardly possible; those finest particles were probably present in the suspension in at least concentration similar to that suggested by the average distribution, already considered. The load of somewhat coarser material, silt fraction, was at the time much higher than the average. As a particle of the modal size of our silt layer, some 4 μ, settles roughly about 100 times faster than

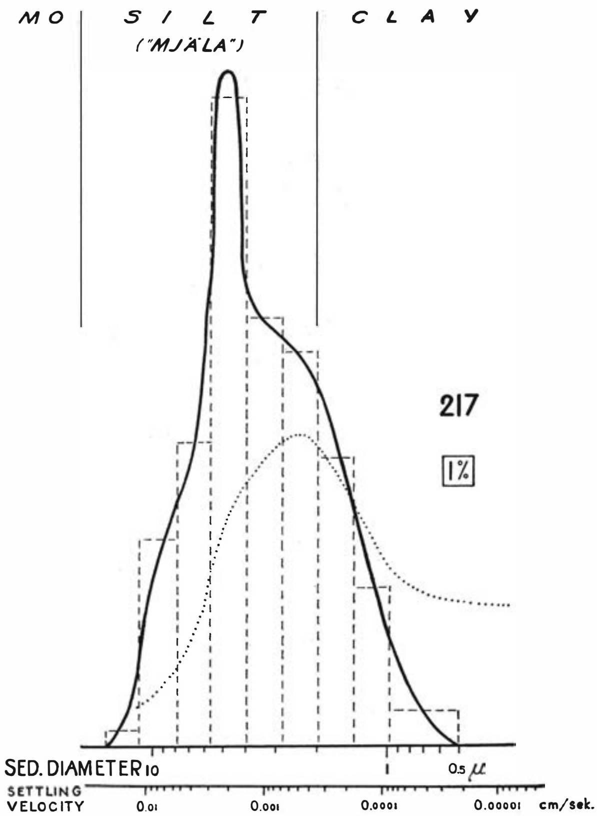


Fig. 11. Frequency diagram of exceptionally coarse material from thin layer in the middle of varve −737. To simplify comparison, a part of the average of the two general analyses 213 and 219, is given as a dotted line in this and other special analyses.

individual 0.5 μ particles (and many more times faster than still smaller particles, see velocity scale at bottom of fig. 11), it is understandable that the coarser fractions at periods when frequent in the suspension, might entirely dominate the sediment — even if the suspension carried masses of fine particles as well. Besides, if a current reaches the bottom »the smaller particles can accumulate only where the deposition is greater than the transportation» (SVERDRUP, JOHNSON, FLEMING 1942, p. 965). Thus, with particles settling individually, a size distribution of the sediment of the type of fig. 11 would be expected. Had there been a thorough coagulation at the time of sedimentation, giving the small particles, as included in clusters, a settling velocity of more or less the same order as the larger particles, such a distribution of the re-dispersed material would not have been possible. (Settling velocities of coagulated clay suspension are exemplified, for instance, by A. G. HÖGBOM 1892, p. 287 ff., GRIPENBERG 1934, p. 70 ff., NOMITSU and TAKEGAMI 1937, DREVESKRACHT and THIEL 1941.) The distribution revealed by analysis 217 and illustrated by fig. 11

thus, in the writers opinion, proves that the coagulation, if any, can only have been very slight just when the layer under discussion was deposited — probably a time when the water was more disturbed than when the finer material settled.

A less extreme and probably more typical example of a coarse part of a varve (—738) is presented by analysis 215, fig. 12. As the sample examined represents a fairly thick part, some 5 mm of the varve (and evidently a considerably longer time than 217), it may include rather different settling conditions. Though covering, in thickness, about one third of one »summer deposition» it clearly, as for time, represents much less than a third of a sedimentological »summer» season. No estimate can be given, in days or weeks, as to the length of the original deposition time, but that it was long enough to allow changes in the movement of the water seems likely, even though the layer seems rather uniform by ocular inspection on an intact surface.

The most striking feature in analysis 215, fig. 12, is the very marked skewness of the distribution. Whereas the modal diameter is about 5 microns, some $^2/_3$ of the sample is finer. The wave-like irregularities on the »downgrade» stretch of diagram 215 are probably of little or no significance, they may even be due to sampling or analysis imperfections. At least three different explanations suggest themselves when considering the pronounced skewness of distribution of number 215.

1) Different fractions may to a large extent belong to periods of rather different settling conditions, the relative regularity of the composite distribution might be partly accidental.

2) The actual distribution of the particles as they really settled, may because of coagulation have been much more regular, more approaching a probability distribution. Several types of sediments show a marked tendency towards a »normal distribution» when illustrated by a frequency curve on log scale or by a cumulative curve on a »semilogarithmic» probability graph paper (out of numerous possible references, see for instance GRIPENBERG 1934, p. 211, KRUMBEIN 1939, p. 584, BIETLOT 1941, cf. also opposing view by DOEGLAS 1946, 1947, and discussion by HÖRNER 1947 b). If the fine particles had settled as coagulates with clusters of higher velocity than the individual smaller particles of the redispersed sediment of analysis 215, the original settling in the sea may have corresponded to a regular, symmetrical distribution. If so, the skewness would in this case depend on (a probably modest) coagulation in the suspension in the Late-Glacial water. The coagulation question will be further considered in a later sub-chapter.

3) As GRIPENBERG 1934, pp. 203—205 has theoretically demonstrated, a sedimentation in a current would give a skew distribution, under certain

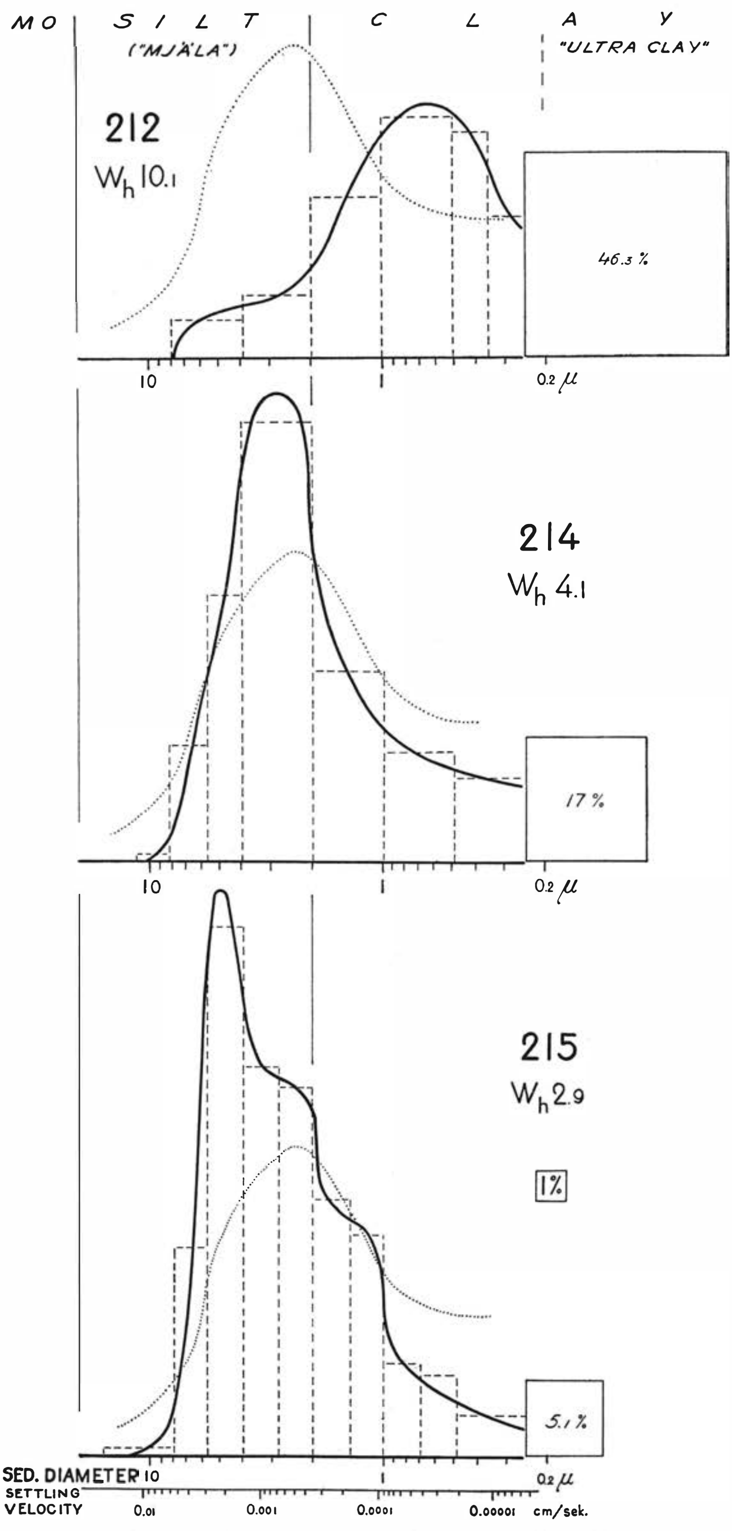


Fig. 12. Frequency diagrams of different layers within a rather typical varve, −738. For location within varve, compare figure 10. Dotted line average composition of varves −740 — −729.

ideal circumstances the »fine» side of the distribution curve would tend to a parabola shape. Disregarding some probably accidental wave-like irregularities, the distribution 215 shows a certain resemblance to the type theoretically deduced by GRIPENBERG.

Even with allowance for above possibility 1), it seems almost certain that the original sedimentation of 215 was also influenced by factors 3) and 2). Cf. p. 242, the discussion of distribution 214. In a somewhat distorted form the »current type» of distribution is shown in the already discussed distribution. See also comparative discussion pp. 245 f., analysis 214. The movement of the water, demonstrated by the type of skewness of the distribution curves, is in good agreement with the comparative coarseness of the silt layers, requiring a certain transport ability of the water.

Inorganic precipitation of calcium carbonate?

In oral discussions of his own results and mine of our simultaneous, independent studies, G. ARRHENIUS suggested that the strongly calcareous silt layers probably were due mainly to inorganic precipitation of calcium carbonate; he found that the physico-chemical conditions should have been favourable, and that the good sorting shown by analysis 217 supported the idea. ARRHENIUS's interesting explanation of the calcareous silt layers (later published; 1947, p. 29) is worth careful consideration; it *may* be of further consequence for the interpretation of settling conditions in our Late-Glacial »sea» and it touches a question of considerable importance in general geology.

As far as the calcium carbonate particles in the varved clay of Uppsala are concerned, there seem to be three possibilities; they may be either 1) disintegration products of limestone, 2) products of inorganic precipitation, or 3) of organic precipitation. Considering the already stated presence of organisms, the last explanation would a priori seem rather probable — as it is for the calcareous zone of certain Proterozoic Tien-shan varves (E. NORIN 1937, p. 118—123 and Plate XXXV) that show such surprising analogies to our Quaternary ones. However, as a microscopic study of our silt revealed nothing suggesting an organic precipitation, the explanation of our calcareous silt ought to be either 1) or 2), or a combination of both.

Our silt is not made up of $CaCO_3$ exclusively, it also contains minerals that prove the presence of fragments of igneous or metamorphic rock. Considering the rôle of limestone in the source material of the Uppsala varve sediment, fragmental lime particles should also be expected. The particles, as far as their tiny size permitted a study, actually looked more like corroded fragments than like a precipitate. They were, in fact, very different indeed from a fresh, artificial test tube precipitate of $CaCO_3$ of about the same average particle size. The artificial precipitate was far more uniform not only in shape (crystallized), but also in size. The sorting in analysis 217 does not seem too extreme for a normal mechanical sediment, settled under fairly uniform conditions. For sand, equally good or better sorting is illustrated for instance by HÖRNER 1947 a, fig. 7, p. 171, diagram C—F, compared with the silt diagram G.

Nothing so far found excludes the possibility that the bulk of the material of the silt layers under discussion might be made up of rock fragments. But neither is a contributing inorganic precipitation disproved.

The conditions in the rather deep water in front of our waning land ice were just about as different as possible from those at that much-studied classical locality for inorganic precipitation of calcium carbonate, the Great Bahama Bank, where Ca^{++}-saturated water, moving in over the tropical shallows, becomes over-saturated through heating (and evaporation), small aragonite needles, stirred up by the waves, serving as the necessary nuclei of precipitation. Yet, great as the differences were, even the modest temporary warming up of our Late-Glacial surface water, already discussed in connection with the *Lucioperca*, may have resulted in some precipitation of calcium carbonate if the critical concentration was reached and suitable nuclei were present, as they must have been, with all those miniature fragments in suspension. It should be remembered how much less soluble $CaCO_3$ is in warm water than in cold (see for instance GRIPENBERG 1934, table 23, p. 173), also its marked tendency, nuclei lacking, to remain in over-saturated, metastabile state. It is very difficult to form any opinion on the probability of Late-Glacial precipitation of calcium carbonate — not to speak of its amount. Outstanding authorities have declared it impossible even to state, for a given set of known conditions, whether a water is saturated with calcium carbonate or whether precipitation or solution may take place (SVERDRUP, JOHNSON and FLEMING 1942, p. 998). True enough, other experts have shown that, at least under certain conditions, data on influencing factors may permit an accurate determination of the saturation point (BUCH 1945, p. 165 and works quoted there). Our data on the Late-Glacial waters are at any rate far too inadequate for any such conclusions about $CaCO_3$ conditions. Instead we may hope, by finding out from the sediments themselves whether any precipitation has taken place — and, if so, to what extent —, to be able to draw some conclusions about other characteristics of the sedimentation environment as well.

In a more preliminary consideration of the probability of any fairly large-scale Late-Glacial precipitation of $CaCO_3$ it may be noticed that WATTENBERG and TIMMERMANN (1936, p. 29) do not think that there is any considerable precipitation of calcium carbonate even in temperate shallow seas to-day, in spite of wide-spread over-saturation in surface water down to considerable depths. The negative conclusion of WATTENBERG and TIMMERMANN as to precipitation is founded on studies of the sediments themselves. In the present day Baltic there is no precipitation of calcium carbonate (BUCH 1945, p. 184). In the southern part of the Baltic there occurs occasional over-saturation in surface layers, not in the Bothnian gulf. But even when and where surface layers may be over-saturated, underlying water is, as a rule, under-saturated (GRIPENBERG 1934, p. 172, see also BUCH 1945, pp. 170—174). It should, however, be remembered that whereas most of the present day tributaries of the Baltic are comparatively poor in lime (GRIPENBERG 1934, p. 173, BUCH 1945, pp. 184—185, also J. V. ERIKSSON 1929, pp. 50 ff., especially Fig. 5, p. 51) some Late-Glacial affluents must have been very rich in lime, as for instance the main glacial river of our region, as is suggested by its once suspensionary load, now forming varved sediments. The material came from the south Bothnian Silurian area, the importance of which has been stressed by many geologists; see the thorough treatment of the subject by GRIPENBERG 1934, pp. 117 ff. Though not sufficient for a quantitative estimate of the calcium content of the Late-Glacial »sea», the studies by A. G. HÖGBOM (1889, 1892) on the varved clays show such an extensive dissolving of $CaCO_3$ from the particles in suspension that it must have substantially contributed to the calcium concentration in the water. Conditions for temporary over-saturation in surface layers may occasionally have been at hand, and if so, with suitable nuclei in abundance, precipitation should have taken place. Even then the chances for such a precipitate to reach the bottom (at our locality at the depth of some 120 to 150 m) seem uncertain. If the water below the surface layer was under-saturated, calcium carbonate was dissolved again to an extent depending on, among other things, the degree of under-saturation and the size of the particles. Their corroded appearance now proves that a dissolving did take place, probably mainly in the suspension. The possible importance of post-sedimentation solution should, however, not be overlooked, cf. TARR 1935. In spite of varvity and stratification, the movements and exchange of water must have been rather limited in the distal parts of our Late-Glacial clays, once settled — but examples of considerable secondary migration of calcium carbonate within the sediments are plentiful nevertheless.

Not only for the local inquiry concerning the sedimentation environment at a certain time, but also as a link in the comprehensive studies by different experts in different regions and varying conditions on the geologically important general question of inorganic precipitation in the carbonate cycle (SVERDRUP a. o., loc. cit.), the origin of our calcareous silt layers deserves further investigation (as stressed by HÖRNER 1947 c, p. 266).

Chemical analyses of calcareous particles of our silt layers (provided it is possible to isolate them in sufficient quantity) and of the limestones from which fragments could have been derived, would probably disclose whether the silt is mainly fragmental or mainly a precipitate — possibly even the comparative abundance of either component. The distribution and differences horizontally of individual silt layers could probably, if studied in sufficient detail, give an answer to the question as to whether the sedimentation was mechanical or chemical.

Examples of minute stratification within individual varves are plentiful also from regions where there is no limestone among the mother rocks of the clay. Whether there also occur silt layers similar to those of the Uppsala varves cannot be stated without further investigations outside our area. Such a study would help to solve the question of the modes of formation of the silt.

Intermediate layers.

Certain brown parts of the varves, parts evidently intermediate in coarseness of material, are represented by analyses 214 from varve −738 and 218 from varve −734 (figs. 12 and 13, respectively).

The sample for analysis 214 is taken from the upper brown layer of varve −738, probably formed in the early autumn. The modal diameter is about 3 μ, slightly more than in the total average of the whole series, but the individual brown layer naturally has reached a higher degree of sorting than the general sample; a much greater part of the individual sample is concentrated around the modal diameter. The most noticeable feature of the distribution 214 is, however, its skewness. (Note that this skewness is opposite to that of GRIPENBERG's 1934, pp. 191, 206, type 3, non-varved common type of fine-grained Late-Glacial clay.) Care has been taken to get a uniform sample for analysis 214. Considerable changes during the original settling of the layer represented by that sample seem unlikely. Possible explanations of skewness have already been discussed in connection with analysis 215. For very fine sand (»mo»), and even for silt, movement of the water is likely to give a skew distribution as shown and explained by GRIPENBERG 1934, pp. 203 ff. (The examples given by GRIPENBERG have, as a rule, a modal diameter somewhere between 20 and 50 μ, see especially GRIPENBERG, loc. cit., p. 192, fig. 31, nr. 1, 2 and 5.) The finer and more slowly settling the clay components are, the more unlikely it is that the skewness could be caused exclusively by the sorting effect of a more or less constant current on the settling of individual particles.

Such an extention of the distribution towards the »fine» side as in sample 214, involving a strong representation of particle sizes even much smaller than a micron, cannot be explained with a sedimentation of each particle individually; note the consequences of the slowness of the settling. To figure out just how such a sedimentation would proceed under natural conditions would hardly be possible, several factors are too insufficiently known. Yet, even in considering some very simplified, rather theoretical examples one may form some idea about the probability or improbability of very small clay particles accumulating by individual settling to such an amount as in our varves.

It seems worth considering, for instance, how particles of any certain settling velocity, originally evenly suspended in water of a given depth, would become distributed in the direction of an even and constant horizontal current. Or how large a part of the material passing any certain area of a section perpendicular to that same constant current would settle within a given horizontal bottom area. Or what the suspensional concentration of any certain completely dispersed »fine» fraction would have had to be to give the amount of deposit actually found to have settled in any given time. A somewhat similar computing has already been tried on pp. 227—228, but there under the assumption that nothing settled more slowly than 2 μ or 1 μ particles, respectively. An individual particle sedimentation throughout, necessitates a concentration high enough to be utterly unlikely under natural conditions.

Here only the question of sedimentary distribution will be illustrated by a highly simplified example, in which several complications, unavoidable in reality, are temporarily disregarded. As already stated, the clays here studied were deposited some 35 km off the mouth of the glacial river. Allowance should be made for turbulence at and somewhat outside the point of discharge. Suppose that from 5 km from the ice edge and on, the current was even and horizontal in the entire height of the water, taken to be 120 m and that turbulence could here be disregarded. The settling velocity of the coarsest particles of »sediment 214» was some 0.01 cm/sec or 8.6 m in 24 hours (see fig. 12). Suppose that such (and smaller) particles were evenly distributed throughout that imaginary section 5 km off the glacier front. The longest time required for any of those particles to sink to the bottom at a depth of 120 m would be $120 : 8.6 = 14$ days. To reach our locality at all, 30 km away, particles of that settling velocity must have covered a vertical distance of 30 000 m in 14 days; that is 2140 m a day or about 2.5 cm/sec. To keep well within the sizes actually studied in the analyses, consider now particles settling with a velocity of 10^{-5} cm/sec $= 8.6 \cdot 10^{-3}$ m in 24 hours. It would take those particles nearly 14 000 days or some 38 years to settle individually to the bottom at 120 m. In that time the current just mentioned, 2.5 cm/sec, if unchecked, would have covered some 30 000 km — about 3/4 of the circumference of the globe. Even if the current had slowed down considerably, particles settling 10^{-5} cm/sec would have spread over an enormous distance. Had the current had only 1/100 of the velocity required to get the largest particles to our locality, thus $2.5 : 100 = 0.025$ cm/sec, the finer material under discussion would yet have reached 400 km. As the distribution in the starting section was assumed to be uniform, particles sinking 10^{-5} cm/sec or slower would — under the assumptions already made, have been evenly distributed all over that distance.

If, as in the averages for our 12 varves, some 20 % of a varve some 13 mm thick consists of such fine material, then, under the above assumptions and with a horizontal velocity of the water of only 0.025 cm/sec, the yearly accumulation would still 400 km from the mouth of the glacial river be 2.6 mm. Experience shows that the sediments are not that widely distributed with such a thickness. Not to speak of a similar thickness at a distance of 30 000 km.

It is evident that even with exceedingly slow currents and conditions as regular as they can only be in theoretical speculation, the assumption of consistent individual settling of particles as fine as here discussed leads to consequences quite out of proportion to observed facts.

Further, there must have been more irregular and more complicated movements in the fairly large and unsettled body of water where the sedimentation took place (note, among other things, the movement in the water caused by changes in thermic stratification). As for particles of colloidal size, not even the quietest lake with the most constant temperature

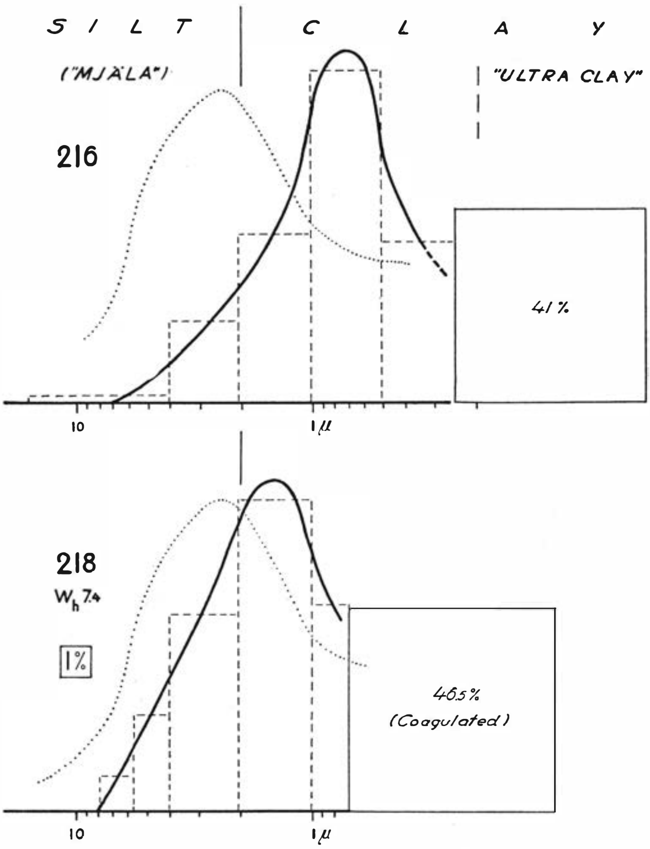


Fig. 13. Frequency diagrams of different layers within a somewhat dwarfed varve, −734. Compare fig. 10 p. 232.

would enable them to sink to the bottom individually in anything like the amount in which they occur in our Late-Glacial deposits. Whereas the movement of the water may be a main reason for the modest skewness of the previously discussed material 217, characterized by the absence of the very finest particles, the skewness of number 214, unless caused by changes in settling conditions, is probably due mainly to the clay having settled in partly coagulated form. If the original, composite settling velocity distribution of individual particles and agglomerates was really more or less symmetrical and regular, which is rather uncertain, a considerable part of the agglomerates should have reached at least the sedimentation diameter groups 1—2 μ — otherwise our distribution 214 could never have been »regular». Concerning that type of distribution cf. p. 237 and references given there.

A coagulation mainly in the very fine fractions, and seemingly leaving particles of a few microns untouched seems contrary to a well-known, in certain respects empirically verified theory of sedimentation (as summarized by WIEGNER 1932). Normally, in a disperse system, somewhat larger particles in their settling should serve as coagulation nuclei for smaller particles which they overtake. (Cf. also ODÉN's, 1916, p. 192, statement that »der Schwellenwert für kleinere Teilchen grösser ist als für gröbere». Schwellenwert = threshold value.) Furthermore, according to several observers, the very small particles should have a tendency towards stability. I can offer no adequate explanation here for the preference of smaller clay particles in the coagulation of our clay, but call attention to the important observation and similar statement by GRIPENBERG already 1934, p. 74, that coagulation begins with the smallest grains, gradually progressing to the larger ones. The coagulation will be considered again on pp. 251 ff.

The other example of a brown layer examined, that of the exceptionally thin varve —734, is illustrated by analysis 218, fig. 13. Gray, silty layers are almost entirely lacking in this varve. The sample examined in analysis 218 represents the entire material between the two enclosing winter deposits, except half a millimeter or less closest to each, left out so as to avoid possible transition zones. Even so, analysis 218 is supposed to cover all the summer and most of the spring and autumn of the year —734. For a spring and summer deposit, the material is surprisingly fine — considerably finer than the collective general samples, representing an average of both summer and winter deposits of 12 years. The modal diameter is only some 1.5 μ — not much more than half of that of analysis 214. At first sight, the diagram 218 may give little impression of skewness, so characteristic of analysis 214, discussed above. The skewness of the distribution 218, in reality probably very pronounced, has been some what camouflaged by the early coagulation of the sample during the analysis. Some time after the pipette analysis had passed the 0.7 μ size, the suspension began to coagulate and was not re-dispersed. Thus, of the total material, the 46.5 % finer than 0.7 μ was not granulometrically differentiated, and the size distribution within that finer group is not known. That distribution does not, however, seem to promise much detail information on the sedimentation environment at any certain time, as analysis 218 represents particles, the original, natural settling of which in the Fini-Glacial »sea» has taken a long time, probably more than half a year, during which sedimentation conditions must have changed considerably. That sample certainly does not represent sedimentation under even nearly uniform conditions. Analysis 214, on the other hand, represents a much shorter time and, most likely, also much more uniform conditions.

Winter layers.

Winter layers have been examined from varves −738 (analysis 212) and −734 (analysis 216).

In spite of the fineness of the material, the mechanical analysis was not carried out beyond the diameter size of 0.24 μ, as it seemed uncertain, even with the precaution taken (quite constant temperature, protection against light, etc.), whether a regular sedimentation analysis, without resorting to centrifugation, would give reliable results for still smaller particles. Possible complication through Brownian movement, etc. Note also that even the faintest convection currents would disturb the settling of such small particles. As such a great part of the winter clay is finer than 0.24 μ, and thus left undifferentiated, the analyses give a rather incomplete idea about the size distribution in the winter layers.

Some authors using pipette analysis or other standard sedimentation methods have, it is true, published distribution diagrams including much smaller particle sizes than 0.2 μ, but in some cases the plotting of those finer fractions involves a minimum size arrived at in another way that seems considerably less exact. Thus for CORRENS' fine and complete curves (1940, p. 537), the actual granulometrical analysis was not extended beyond 0.1 μ radius (= 0.2 μ sedimentation diameter). The ultimate limit of the finest fraction is chosen as when in an earlier work the entire clay content was left undifferentiated granulometrically: »Für die Darstellung der feinsten Korngrössen wurde als untere Grenze die Porenweite des Ultrafilters genommen. Sie liegt, wie aus den Darlegungen von v. HAHN zu entnehmen ist, bei etwa 0.01 μ.»

Only some 8 % of our winter material is coarser than 2 μ, ATTERBERG's upper limit for clay, and a slight extrapolation indicates that approximately 40 % is finer than 0.2 μ, thus being what has sometimes been characterized as »ultra clay» or »colloidal clay».

The samples examined are probably fairly typical of the winter sediments of our region at the time concerned. Conditions may have changed somewhat even during a winter. The presence of silt particles, with settling velocities up to perhaps two or three decimeters an hour, together with the very slow particles of fine clay, does not in itself permit detailed conclusions as to the degree of coagulation.

Comparison of size distributions.

To facilitate comparison, all 8 analyses included in this study have been plotted as cumulative diagrams in fig. 14, and some data presented in tables I and II.

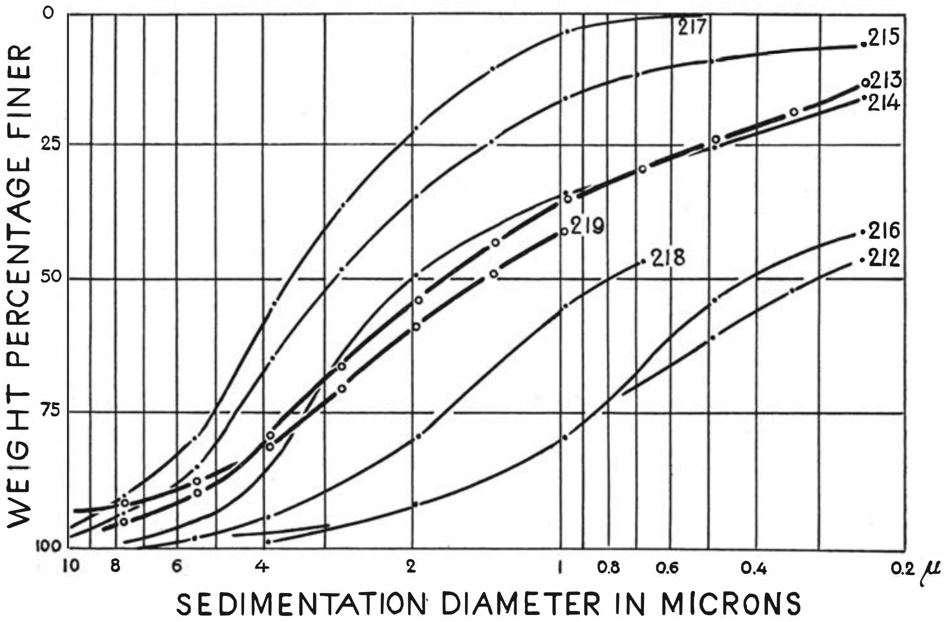


Fig. 14. Cumultative diagrams of the clay sample examined.

There is a marked difference in particle size between the layers formed during different seasons, the spring and summer deposits being as a rule considerably coarser than the fine, stiff winter clay. As the layers differ not only in particle size, but also in chemical composition (see for instance the studies by HÖGBOM, already referred to p. 221 and 241), it is doubtful whether the hygroscopicity values really stand in constant relation to the total surface, but the general tendency seems unmistakable, confirming the granulometrical results. Our varves thus follow the classical pattern.

Table I. Mechanical composition of the samples examined, expressed as ATTERBERG classes. (By interpolation. Compare diagram. ATTERBERG values rounded off, decimals omitted. ATTERBERG values are given for easy comparison with other publications, for further details see diagrams. *T* stands for traces, here meaning less than 0.5 %.) Hygroscopicity values as defined p. 221.

	Varve number	Analysis number	Mo[1]		Silt (»mjäla») 0.02—0.002 mm		Clay, finer than 0.002 mm	Hygro-scopi-city
			coarse 0.2—0.06	fine 0.06—0.02	coarse 0.02—0.006	fine 0.006—0.002		
General samples	-740—-729	219	*T*	5	5	31	59	5
	-740—-729	213			8	37	55	
Winter layers	-738	212				8	92	10.1
	-734	216				8	92	
Intermediate layers	-738	214			3	47	50	4.1
	-734	218				20	80	7.4
Silty layers	-737	217			16	62	22	
	-738	215			12	53	35	2.9

[1] The Swedish term mo which has previously been to some extent internationally used, is adopted here too for the size group 0.2—0.02 mm inducting the finest of WENTWORTH's »fine sand», his »very fine sand», and the coarsest of his wide silt group.

Whereas the two winter layers studied show considerable similarity, there are, as might be expected, great variations among deposits referable to the warmer seasons.

The distinction here made between coarse and intermediate types within those deposits (in some varves intensely interstratified) is admittedly somewhat arbitrary. Our analyses represent extremes (217, 218) as well as grading-over types. Probably every gradation could be found between »coarse» and »intermediate» within the »summer» series, whereas the contrast to the »winter» deposits is, as a rule, more distinct.

Even the finest summer material found in our series, sample 218, is distinctly coarser than the winter layers, though the difference is not great enough to account for the marked contrast in appearance. The difference in shade between a summer material like that of varve 734 (fig. 10) and any winter layer within our series can not be due to particle size only.

Table II. Some characteristics of the samples examined.

	Varve number	Analysis number	Modal diam. (μ)	Median (μ)	First quartile (μ)	Third quartile (μ)	Sorting coefficient So	$\log_{10}$ So	Skewness (TRASK)
General samples	-740—-729	219	2.3	1.4		3.2			
	-740—-729	213	2.7	1.7	0.54	3.5	2.54	0.405	0.65
Winter layers	-738	212	0.65	0.31		0.85			
	-734	216	0.70	0.42		0.85			
»Inter-mediate» layers	-738	214	2.8	2.0	0.49	3.3	2.60	0.415	0.40
	-734	218	1.5	0.80		1.7			
Silty layers	-737	217	4.8	3.6	2.1	5.0	1.54	0.188	0.81
	-738	215	4.9	2.9	1.4	4.5	1.79	0.253	0.75

The dimensions given refer to weight frequency. Modal diameter: the most frequent sedimentation diameter (weight frequency) as found in the frequency curves, figs. 10—13. Median and quartiles correspond to the cumulative diagrams, fig. 14, sorting and skewness as defined by TRASK except that quartiles are here numbered in the manner otherwise customary; thus (quartile) sorting $= \sqrt{Q_3/Q_1}$; quartile skewness $= Q_1 \times Q_3/(\text{Median})^2$. For further explanation see KRUMBEIN and PETTIJOHN 1938, pp. 229—239.

First quartile values (and consequently all data, the computing of which cannot be done without Q_1) are missing for 4 samples: In 212 and 216 Q_3 falls on a smaller particle size than can be measured by the method used. Samples 218 and 219 coagulated during the sedimentation analysis before Q_1 was reached.

The difference between the winter material and the coarsest layer in the summer deposits is very considerable indeed, as exemplified already by the medians 0.31 μ and 3.6 μ. Expressed in settling velocities the contrast becomes even more striking: the median of the silt layer corresponds to a settling velocity more than a hundred times higher than that of the winter median. In interpreting the meaning of those great differences, the origin of the silt already discussed, plays an important part.

A remarkable feature in most of the distributions studied (and presented here) is a certain tendency to similarity in the coarsest fractions even when the rest of the curves thoroughly differ.

To the synopsis of average distributions and of type layers of the varves under discussion may be added comparisons with a few other Late-Glacial clays.

As already stated, our varves are located fairly near the top of the clearly distinguishable varve series of the Uppsala region. On top of those well-developed varves follow micro varves and »spotted zone» (see p. 202), which in its turn is covered by a stiff, fine dark clay, sometimes called »Ancylus clay», a terminology the justification of which may be questioned (GRIPENBERG 1934, p. 207). That clay was evidently formed when the mouths of the glacial rivers had, with the retreat of the ice edge, withdrawn so far that the varve development no longer reached our region. The very fine particles slowly accumulating, formed the unvarved »ultradistal» clay. The rise of the land had not yet advanced sufficiently to furnish considerable redeposition products. The particle size distribution, together with other characteristics of that clay, was studied by ODÉN and REUTERSKIÖLD 1919 (see also ODÉN 1920) and GRIPENBERG (loc. cit., p. 208) has re-computed their results in a way easier to visualize. The median of that clay, 0.66 μ, is finer than that of our finest »intermediate» material studied (0.80 μ), but coarser than our coarsest winter layer (median 0.42 μ). Judging from our analyses 216 and 218, the median for a sample representing the whole varve −734 would probably come fairly near the ODÉN—REUTERSKIÖLD »Ancylus» clay. The »Ancylus» clay contains more of the coarsest material and less of the finest than those special samples of ours. As the sample of ODÉN and REUTERSKIÖLD must represent a much longer time than our »season types», the lesser degree of sorting was to be expected. Not only clay of the extreme »winter» type, but also somewhat coarser material reached the »ultradistal» region. (In comparing distributions derived by ODÉN and pipette analyses, note the possibility of methodological differences influencing the results).

Several of the Glacial varve clays from the Baltic studied by GRIPENBERG 1924 show a median smaller than our 12-year-average but larger than any average median that our finest varve, number −734, could possibly give (see for instance GRIPENBERG's samples F 28 1925 c, F 20 1925, F 33 1925 c, F 54 1924, loc. cit., p. 194, and pp. 103—111).

The finest of all the Baltic glacial clays of GRIPENBERG F 76 A 1930 II (taken at a depth of 99 meters far out from the easternmost skerries E.S.E. Stockholm) — evidently a varveless »ultradistal» clay — has a median closely corresponding to our winter layer; the coarse quartile of the GRIPENBERG sample is identical with corresponding quartile of our winter layers. Though not holding as much »ultra clay» as our winter layers, GRIPENBERG's clay is quite remarkably fine for a general sample probably representing many years or rather decades of deposition. Such an advanced concentration of very fine material seems somewhat surprising in a location where the bulk of the Late-Glacial clay would be expected to date from the decidedly brackish — and thus coagulation creating period of the Yoldia sea — even though the bottom layers at the locality concerned may be older than the salt water invasion of DE GEER's year —1073. The ultradistal surface layers exemplified by GRIPENBERG's sample F 76 A 1930 II may, however, be so much younger that the brackish stage had already changed into fresh — or almost fresh — water again. May GRIPENBERG's material here referred to perhaps be an ultradistal phase of the clays discussed in this paper, or perhaps even still somewhat younger? Note, however, the absence of calcium carbonate in this particular GRIPENBERG sample.

It may be added that the fineness of our winter clays — and also that of some samples of ODÉN's and some of GRIPENBERG's in the publications quoted — seem remarkable enough to call for further attention. That sediments of oceanic shelfs and the upper part of their slopes are much coarser (see for instance STETSON 1938) may be expected, but that whole extensive series of deep sea sediments (CORRENS 1937, cf. also 1940) fail to include a sediment as fine as our winter clay, seems surprising — even with allowance for the difference in time required for accumulating.

Coagulation and salinity.

One of the reasons for the granulometrical study of different detail layers within our varves was to secure some information, if possible, about the state of coagulation or dispersion in which the sediments settled and, indirectly, on the salinity of the water. The particle size distribution suggests, in the writers opinion, the following conclusions:

1. The impression of ocular examination and field tests is confirmed: the diatactic, distinct varvity of our sediment involves a considerable difference in particle size between deposits of different seasons, the winter deposits being the finest. The particle size distribution of the different layers, and the relations between those distributions excludes settling under »total» coagulation.

2. To settle at all, to the extent that it has taken place, the finest portions must nevertheless have been more or less coagulated. That holds true especially for the winter deposits with their very high content of »colloidal clay». The degree of coagulation may have changed with the seasons. The skewness of sample 214, the distribution being extended towards the »fine» side, shows however that the coagulation was not limited to winter season only.

3. Provided fine particles were also present in spring and summer, as they must have been[1] (BRENNER 1925, p. 16), it is evident that larger

[1] Provided the glacial rivers carried also fine particles, which seems safe to conclude that they did (BRENNER, loc. cit.). The decomposition and dissolving of minerals in the glacial waters and the possible consequences, also in decreasing (or increasing!)

and smaller particles did not cluster together sufficiently to do away with the differences in settling velocity. As it is hardly possible that comparatively large particles should have to any considerable extent joined each other without attracting smaller ones, it seems safe to conclude that a great part of the larger particles were not included in agglomerates but settled individually. The agglomerates of smaller particles were not large and compact enough to settle with a velocity equal to that of the larger idividual particles. The size distribution of sample 217 (the silt probably dating from a time when the water was unusually disturbed) suggests that the clusters did not include any considerable amount of particles larger than 1 μ, and that few of the clay agglomerates reached a settling velocity equal to that of individual 1 μ particles. That conclusion would, it seems, hold true even if the silt consisted mainly of a physico-chemical precipitate (see above p. 239). The silt particles must at any event have settled through a suspension rich in small particles that would have gone down with the silt »grains» if attached to them.

4. Though what is thus known about the state of coagulation in which our sediment settled does not furnish quantitative data on the electrolyte content of the water, it can safely be concluded that the salinity must have been very low, the water almost fresh or perhaps quite fresh.

As a background to those partly tentative conclusions, and for further considerations, some points about clay coagulation require additional attention, especially as certain of our conclusions seem somewhat difficult to bring into agreement with established conceptions. Only a few references and some scattered reflexions will be offered here on these very complicated questions. The laws controlling coagulation of clay — and thus to a considerable extent its sedimentation — are not yet sufficiently known to predict the effect of different combinations of factors, and still less to deduce, from the state of coagulation, exact data on the factors involved. In spite of much experimental and theoretical work — part of it very successful and important — by many experts, there still seem to be fundamental differences even concerning the basic principles. For a general orientation over one main line of theory and its experimental background see WIEGNER 1932 and several references quoted by him, especially VON SMOLUCHOWSKI, MÜLLER and TUORILA. Different kinds of movements (Brownian molecular movement, gravity, etc.) bring small suspended particles close to each other (in a way that can, in part at least, be mathe-

particle size, should not be overlooked. For weathering of felspar see TAMM 1925, 1928, 1929; the action of the water etc. on calcium carbonate must, under favourable conditions, have had a much greater effect on the granulometrical make up. But weathering and dissolving could hardly explain an absence of fine particles in the summer.

matically treated), and unless a suitable charge, exceeding a critical potential, provides sufficient repulsion to keep the particles apart, they will adhere (provided differences in movement impulses are not in themselves strong enough to overcome attraction between particles, cf. upper size limit of coagulating particles and their settling velocity). For a short and clear summary of MATTSON's in certain respects rather different explanation of clay coagulation as an ionic exchange phenomenon, and rejecting high potential as a direct cause of stability, see GRIPENBERG, loc. cit., p. 76 ff., and for further penetration MATTSON's own works quoted there.

The present writer makes no attempt here to venture into the theory of coagulation — fundamentally a physico-chemical problem, but will call attention to certain factors which seem possible, to some extent at least, to consider without involved theory, some of the factors influencing coagulation of a clay suspension:

Particle size. As already shown, all particle sizes from some 10 µ down to colloidal sizes have been present in the suspension. In such a polydisperse system one might perhaps a priori expect orthokinetic coagulation, already referred to p. 245, to play a dominating part: larger particles, in their faster settling overtaking, attracting, and attaching smaller ones. It needs hardly to be pointed out that our above conclusion 3) is not in agreement with a predominantly orthokinetic coagulation.

If, on the other hand, perikinetic coagulation, with Brownian movement as the cause of collisions in all directions, dominates certain stages of the flocculation process, great number and faster movement of smaller colloidal particles than of units of more than a micron or two in size, the beginning of the coagulation in the smaller fractions (see conclusion 3, p. 252, also p. 245) seems understandable. Note that the theory of an ideal orthokinetic coagulation was developed and experimentally checked under elimination (through sufficient dilution) of perikinetic coagulation (WIEGNER 1932, p. 162—163). In nature, both types of flocculation may well occur together. The relation between orthokinetic and perikinetic coagulation has been discussed in another connection by VUORINEN, 1939, p. 59.

Why, in some of our deduced cases, the agglomerates did not grow to reach higher velocity than that of individual particles of about a micron or so, is not clear. It might be supposed that as the agglomerates grew, they became less subject to the Brownian movement, and that, with a settling velocity more like that of the presumptive nuclei of orthokinetic coagulation, the chances for the coagulation to go on as perikinetic decreased.

Kind of particles. After a study, much concerned with flocculation, DREVESKRACHT and THIEL (1941, p. 700) concluded that »chemical and mineralogical variations in fine sediments have very little influence on the rate of sedimentation». The conclusion may seem somewhat surprising, considering certain marked differences in behaviour, not explainable by particle size only, among the ten different types of sediments used in their experiments. See also the differences in flocculation tendencies of different kinds of material pointed out by ANTEVS (1925, p. 33—34). Some allowance for difference in material should be made in trying to apply to our Late-Glacial sedimentation the results of for instance NOMITSU and TAKEGAMI (1937) from their comprehensive systematic study of the coagulation of some Japanese and Korean »muds».

Flocculation studies should preferably be made on the very material under discussion in any certain case. Even then, it would by no means be sure that the present behaviour of a sediment would exactly correspond to that under identical conditions at the time of sedimentation. See ANTEVS's (1925, p. 33) notion that the glacially new-ground material in this respect differs from »the unconsolidated materials that largely supply land rivers».

Yet, even if not »very small» and by no means negligible, the effects of the difference of material on the settling keep within limits that make some generalizations possible, as seen from the exceedingly varied material of DREVESKRACHT and THIEL, loc. cit. As no coagulation studies have been made on material quite identical with that of our varves at the time of their deposition it is reassuring to know that even conclusions drawn from other material is likely to give the right order of magnitude.

The concentration of particles in the suspension naturally plays a very important part in the process of coagulation; the more particles there are in a unit volume of water, the more chances for collisions. A very diluted suspension may show no signs of coagulation — at least not for a long time, whereas a rather concentrated suspension may coagulate quickly and almost completely — as probably experienced by most sedimentation analysts working with material, difficult in this respect.

The influence of suspensional concentration figures in coagulation theory and has been practically studied by several investigators, but so far the relations have not, to the writer's knowledge, been fixed in numbers or formulae in a way that could be adapted to our present problem. VUORINEN's (1939, p. 59) explanation of certain observations of his may have a considerable practical bearing: dilution favours the orthokinetic type of coagulation; in a concentrated suspension perikinetic coagulation may occur so quickly that the small particles are more or less withdrawn from the schematic procedure of orthokinetic coagulation (see also WIEGNER 1932, p. 162—163, already quoted above, or TUORILA's and his eliminating perikinetic coagulation through dilution of suspension).

Movement in the water, at least violent turbulent movement, tends to prevent or counteract coagulation, or to partly deflocculate when it has already occurred (see for instance ODÉN 1916, p. 192). Note how some elutriators have a stirring device to break up flocculae by movement. In the glacial rivers the intense turbulence prevented or restricted coagulation, and also outside the mouth it was more or less counteracted as far as the turbulence extended. The seasonal differences in movements within our Late-Glacial lakes and brackish »seas» are likely to have considerably influenced the coagulation, as has been pointed out by several authors (Example: ANTEVS 1925, p. 32).

Temperature influence on coagulation seems to be insufficiently known, yet it must be active in different ways, first of all on the velocity of particles, either in Brownian or gravital movement, and thus on the chances of collision of particles. A low temperature is by some authors considered an essential condition for varve formation. (ANTEVS 1925, p. 33; FRASER 1929.)

Electrolyte content. The great influence of salinity (or, more generally expressed, electrolyte content) on coagulation and sedimentation has long been recognized by geologists and experimentally verified (A. G. HÖGBOM 1892, p. 287—289). SAURAMO (1923), in his classical study of Finnish varves, stressed and specified the rôle and effect of salinity in the development of varves, more brackish water giving more symmict varves, fresher water more diatactic (as defined by SAURAMO 1923, p. 82). According to FRASER's experimental study (1929), the maximal salt concentration at which varve formation takes place, is about 1/50 of that of the ocean. The statement, valid for FRASER's experiment, may involve too unreserved a generalization; there were other factors, also, and field evidence seems to indicate that in some cases varves must have been formed at a higher salinity than FRASER's maximum. GRIPENBERG's (1934, pp. 70 ff.) experimental investigation into the effect of different degrees of salinity on coagulation has given much information. Even as low a salinity as 0.25 ‰ (corresponding to 4 mekv./l) caused considerable coagulation, especially among the smaller particles, the modal sedimentation diameter of the coagulate being between 2 and 5 μ, and particles with sedimentation diameters smaller than 1 μ having from one day to another been reduced from 44.9 to 3.5 weight percent. Coagulation in GRIPENBERG's experiment had thus reached considerably larger particle sizes than, according to our conclusions p. 252, in the »Yoldia sea» when certain layers in our varves were deposited. That does not necessarily mean that our Late-Glacial »sea» at that time had a still lower salinity than 0.25 ‰; it was far less muddy than the experimental water and had a lower temperature; also other factors may have been different enough to make coagulation conditions different. The comparison nevertheless indicates that the Late-Glacial waters of our region can have had at best a very low salinity.

Important also are the results of NOMITSU and TAKEGAMI (1937), VUORINEN (1939) and DREVESKRACHT and THIEL (1941) on the coagulational effect of different well-defined concentrations of specified ions. VUORINEN's clay, and numbers 3 and 7 of the ten samples of DREVESKRACHT and THIEL seem to be those most like our material, as far as components go. The bivalent cations Ca^{++} and Mg^{++} have generally a stronger coagulating effect than the monovalent Na^{+} and K^{+} in the corresponding concentration. In the proportions between different cations characteristic to normal sea water, the coagulating effect of Ca^{++} (and Mg^{++}) would be a little — but not much — stronger than that of Na^{+} (NOMITSU and TAKEGAMI 1937, pp. 22—23). Whereas at sufficient concentration of cations, sufficiently fine and frequent particles in suspension coagulate in a very short time, the coagulation slows down as ion-concentration decreases. At a certain critical concentration the effect of the ions decreases abruptly (loc. cit., p. 24). NOMITSU and TAKEGAMI state that concentration to be c. 1/100 normality, but their results show that it varies considerably with the kind of ion and concentration of the suspension. At a suspensional load (of the Japanese mud) of about 3.1 g/l the critical Na^{+}-concentration seems to be somewhat lower than N/100, for Ca somewhere around N/500. VUORINEN found that in a clay suspension of 50 g/l coagulation in 24 hours had begun at about the following concentrations: some 0.7—0.8 mgekv./l $CaCl_2$ or some 2.5 mgekv./l NaCl. Note the very concentrated suspension of 50 g clay a liter. A more diluted suspension would probably have stood a somewhat higher electrolyte concentration without coagulating.

The results of DREVESKRACHT and THIEL rather suggest a gradual effect of decreasing electrolyte content than any special critical concentration. To exemplify effect: concerning their samples 7 (a varved glacio-lacustrine clay from Minnesota, composed of felspar, quartz, and carbonate) and 3 (a silicious and felspatic clayey Minnesota silt of Ordovician? age), in suspensions corresponding 5 g/l, the following conclusions, among others, can be drawn from published data (especially those of table 1, p. 693, diagrams fig. 1, p. 694 in the paper quoted): If the »flocculator» is Na^{+}, a settling velocity of the slowest (finest) parts of the coagulate equal to that of the largest individual clay particles, 2 μ (lowest »sedimentation diameter» of coagulate 2 μ), corresponds to a NaCl concentration = M/12, a lowest sedimentation diameter of coagulate = 1 μ corresponds to a NaCl-concentration of about M/20. (Ca^{++} is not included in the study under discussion.) At a standard composition of sea salts, above Na^{+}-concentrations would correspond to salinities of about 6 ‰ and between 3 and 4 ‰, respectively (computed from the data on sea water composition by SVERD-

RUP, JOHNSON and FLEMING 1942, p. 171 as well as those of NOMITSU and TAKEGAMI 1937, p. 2; as for definition of salinity see SVERDRUP a. o., p. 50, 55).

Now, Na^+ is not the only ion in sea water to cause coagulation, nor is it the most effective, but according to NOMITSU and TAKEGAMI (1937, pp. 22—23, especially table 11 and diagram fig. 14) the effect of Na^+ alone seems to come so near the combined effect of the sea water that the values above may be used for a first approximation. The results of NOMITSU and TAKEGAMI concerning the relative effect of each cation in sea water, and the combined total effect of the sea water, seem difficult to understand and explain. Three of the electrolytes investigated, Mg^{++}, Ca^{++}, and Na^+, have each by themselves, in a concentration equal to that in which they are represented in sea water, a coagulating effect rather like that of the complete sea water. In several cases a single electrolyte gives a stronger effect than when, still in its same concentration, combined with other electrolytes in sea water. The agreement between Table 11 and the certain contents of the previous tables and graphs in the paper quoted hardly seems as good as might be expected, even with allowances for experimental difficulties.

The results of different investigators concerning clay coagulation seem partly somewhat contradictory and, as already stated, it is not yet possible to tell anywhere nearly exactly the salinity of the sedimentation environment from the degree of coagulation. Thus it would not yet be possible, even if our tentative conclusions as to coagulation were in every respect definite and detailed. The salinity in our case should, however, have kept between a few per mille and some tenths of a per mille. If corrections for certain other factors (temperature, concentration of mud, etc.) are omitted, we might conclude that salinity, in the summer at least, was less than 4 ‰. (The conclusion derived from a comparison between the size distribution of our sample 217, discussed p. 252, and the observations by DREVESKRACHT and THIEL just quoted.) A salinity of about 1 ‰ or less seems perhaps more likely — compare for instance the Na^+ concentrations in the laboratory studies of GRIPENBERG 1934, NOMITSU and TAKEGAMI 1937, and VUORINEN 1939, referred to above.

Dr. S. LANDERGREN of the Geochemical Laboratory of the Geological Survey of Sweden kindly determined spectrographically the boron content of the clay under discussion (precision method of LANDERGREN 1945 a) and put the result at my disposal. The sediment contains 0.003 % B. As LANDERGREN has shown (1945 b, especially pp. 12—13) there is a regular relationship between the boron content of a sediment and the salinity of the water in which it settled. The 0.003 % of the Uppsala varve clay corresponds fairly well to the 0.0025 % in the sediments of Sundsvall today, deposited in water with a salinity of about 0.59 % (5.9 ‰) (LANDER-

GREN 1945 b, p. 13). The adsorbtion of boron to a sediment depends, however, also on the size of the particles, the relative surface of the sediment. Though no definite data are known to me about the size in the Sundsvall sediments, it does not seem unlikely that it might be less fine than the Uppsala clay, and that the boron content of 0.003 in Uppsala may correspond to a still lower salinity of the water than the 5.9 ‰ at Sundsvall.

That there are many complications in the salinity/coagulation relationships has repeatedly been stressed (pH conditions have been unduly neglected for lack of data). One more complication should perhaps be pointed out; it may also concern the boron/salinity relationship. In sea water the proportions between the different ions do not, as a rule, vary much. But when sea water becomes as diluted as in our case (if there still was any sea water) and so strongly influenced by local conditions, it is by no means a priori certain that the proportions between the ions remained even nearly the same. No simple and general relationship has been found between Ca^{++} content and chlorinity of the water of different parts of the present-day Baltic (BUCH 1945, pp. 163—164, referring to investigations by GRIPENBERG and WITTIG). The Ca^{++} content in relation to salinity is however generally higher in present Baltic waters than in the ocean. (In addition to above references also personal communication by Miss GRIPENBERG.) Certain ions, for instance Ca^{++}, may have played a greater relative part when and where our clay settled than in real sea water. (Cf. however how some lake waters with comparatively high Ca content remain turbid in a way that disproves total coagulation; example Lake Hjälstaviken, LOHAMMAR 1938, p. 193.) To try to express coagulation conclusions, as in above attempts, as probable salinity may therefore not be strictly justifiable, but as long as we cannot express them as concentration of any particular ion, salinity seems to offer a provisional approach, provided complications are kept in mind.

Possible more or less regular ionic relationships in fresh water are not yet sufficiently known; preliminary announcements by Dr. W. RODHE, Uppsala limnologist, indicate such more or less fixed ionic relationships even in extensive groups of fresh water with low electrolyte content.

In any case, higher salinities than the maximum alternatives arrived at through the above way of reasoning seem hardly to be expected in the settling environment of the sediments concerned. Salinity conclusions thus attempted naturally refer to the layers of water that decided final settling and degree of coagulation. In natural conditions, hydrographical stratification frequently involved different salinity in different layers.

That the salt content must have been very low in the water in which our clay was deposited is — even all complications considered — evident from the character of the deposits. They thus confirm the conclusion arrived at on the evidence of the fish. The fish alone may only have testified low salinity in the upper layers of the water and still left the possibility open for rather high salinity near the bottom. The sediments show that low or no salinity prevailed also in the deeper parts.

Miss GRIPENBERG, in discussing a fine-grained Late-Glacial clay from the Åland sea, and a corresponding clay from Uppsala (that so called Ancylus clay of ODÉN and REUTERSKIÖLD already discussed here p. 250, which she considers a marine Yoldia deposit) stresses the great similarity to certain fresh water clays. She points out that those Åland and Uppsala clays do not agree with what might be expected from salt water deposits. For geological reasons she finds that the water must have been salt and concludes that evidently, contrary to what could be expected, sedimentation of that type under certain conditions must be possible even in saline water. My explanation is the opposite: the water was, as the sediments suggest, practically fresh. At the time when those clays were formed, the Baltic region, because of probable continued rise of land, could be expected to be even considerably more shut off from the sea than at the time, perhaps a century or more earlier, when the sediments here under discussion were formed. If the late Goti-Glacial to early Fini-Glacial crustal subsidence stated by VON POST (1947 a, p. 211, and earlier works) for S.W. Sweden, reached our region, it must have been reversed into a resumed uplift before the time of those sediments (cf. also following references).[1]

Already for the stage of the Yoldia »sea» when our varves were formed, the years G. D. G. –740––729 and thereabouts, as far as salinity goes, the term »sea» should not be stressed.

In the still very acute debate on the development of the Baltic, with so much uncertainty left about such factors as relative elevation of land and sea at different stages, the testimony of the *Lucioperca* and the surrounding clays alike seem to be of value. The Baltic's connection with the sea can, at best, have been a rather modest one, shallow and probably not wide either. In the discussion on Late-Glacial changes of level (cf. for instance for certain parts of the debate in Sweden CLEVE-EULER 1943, 1946, 1947; STEN FLORIN 1944; MAJ-BRITT FLORIN 1944, 1946; CALDENIUS 1944 a, b; HÖRNER 1942, 1944; VON POST 1947; SANDEGREN 1946 b; see also papers on the subject by different authors already quoted) a further study of the structure of the varved clays would give additional information.

When the manuscript of this paper, preliminarily completed in August 1946, was undergoing its final trimming for publication (summer of 1947),

[1] Concerning those Late-Glacial crustal movements, see further VON POST's (1947 b) important paper, published after this manuscript was completed.

an important paper on varvity, and particularly on Uppsala varves, appeared: G. ARRHENIUS: The Varvity of the Glacial Clay. A Study of the Varved Marl in the Uppsala Region (Swedish with English summary). ARRHENIUS tackles the problem from a modern physico-chemical angle and furnishes much new information of principal importance on the chemistry of varvity, particularly the colour differences.

ARRHENIUS' varves, −815 to −769, are slightly older than those treated in this paper, −740 to −729. Together ARRHENIUS' varves and mine do not even span a century, yet they seem to indicate considerable changes in sedimentation environment even within half that time.

As a detail in his paper ARRHENIUS further develops his theory of certain silt layers as anorganic precipitate of calcium carbonate, a theory already referred to here, p. 239. The conclusive proofs are still missing.

ARRHENIUS' valuable contribution to the geochemistry of our Late-Glacial clays will not be discussed here. He opens up again an important line of study that has, with minor exceptions, been rather neglected here since the time of A. G. HÖGBOM's pioneer work.

As for mechanical sedimentation, ARRHENIUS arrives at conclusions in part directly opposed to mine. He considers the entire sedimentation of his varves to have taken place in a highly coagulated form (»totally coagulated» as ARRHENIUS expressed it in our geological society).

Some of ARRHENIUS' varves differ from ours in a way that necessitates a difference in explanation. His oldest varves (from somewhere between G. D. G. −815 and −800) have a winter material considerably coarser than the summer material (against winter medians of 1.6 and 1.8 μ stand summer medians of 0.8 and 0.9 μ, respectively; note however that the samples examined do not represent the entire seasons). The greater coarseness of the winter material ARRHENIUS explains as a residuum, the finer particles having been dissolved. A material as fine as that of the summer layers could not without coagulation have settled so quickly — and left coarser material in the suspension.

The mechanical analyses of different layers within a varve not quite as old, varve X, around −790, shows, on an average, less difference in coarseness between summer and winter, though sorting and distributional details differ. The summer median is somewhat higher (larger particle size). In varve −783 both quartiles as well as median are considerably higher for summer than for winter. That is: on the whole, the winter material is considerably finer than the summer material. Up to a particle size of about 10 μ, varve −783 seems, in general make up, to rather resemble ours, in spite of numerical differences. (In comparing results, note that ARRHENIUS' Q_1 corresponds to our third quartile, his Q_3 to our first.)

ARRHENIUS explains the coarseness of the summer material of −783 and X by the theory that it contains calcareous precipitate, foreign to the mechanical sediment proper, which, without that precipitate, would be much finer. ARRHENIUS' chemical argument for coagulation: the difference in calculated and observed Mg/Ca-relation, will not be discussed here, see ISING 1947.

I should like to point out how the character of ARRHENIUS' varves changes from the older to the younger. The particle size distribution in the older varves (from around −800 down) seems to indicate a very considerable coagulation, his younger varves seem to be more and more like our still younger ones, in my opinion proved to have settled with very limited coagulation.

The most likely explanation seems to be that in the years between about −800 and, say around −740, the isolation of the Baltic from the open sea, through evidently rather intense changes of level, had progressed far enough to render the water almost fresh.

7. The Fini-Glacial climate, one of the main factors in the living conditions of our fish.

The testimony of our *Lucioperca* specimen, that the salinity, if any, was slight in the sea off the Baltic ice front, is in good agreement with what the sediments themselves indicate, as has already been pointed out. The climatic evidences given by the fish confirm and support earlier observations and conclusions by many experts: our Fini-Glacial climate was far from arctic.

The very rapid retreat of the ice edge in our region, some 300 m a year[1], seems understandable only with climatic conditions highly favourable to melting. Note also thickness of varves, by G. DE GEER held as general indicator of warmth (though not exemplified by him from this region). On thickness of varves as climatic indicator see also SAURAMO, for example 1938, p. 221, cf. however below. Ablation must have been very intense, its effect highly surpassing the amount of material carried to the border zone by ice movement from the region of nourishment (compare for principles AHLMANN 1942 a). Though the relations between the thinning out of an ice sheet or glacier and the retreat of its border, on land and in water, have been much elucidated in important studies[2] it is not yet pos-

[1] The varve of the fish corresponds approximately to the transit between Divisions *P* and *Q* of the Stockholm—Uppsala—Gävle line of G. DE GEER and collaborators. 300 is the figure given for the annual recession in Division *P* (up to the varve of the fish), for the following Division *Q* the recession was only c. 150 m per year. (G. DE GEER 1940, p. 150 and Plate 72). The average for Divisions *J*—*P* of the Uppsala region was 300 m (loc. cit., pp. 148—150).

[2] For example SAURAMO 1924, p. 158 ff., and later papers; G. DE GEER 1940, p. 158 a. o., ANTEVS 1939, and especially AHLMANN 1928, p. 336 ff., cf. 1939, and many

sible to draw quantitative conclusions on the annual amount of thinning out of our waning land ice, even less do we have data enough for computing it's whole regime (»economy»). The comprehensive and exact measurements and calculations by AHLMANN and associates on the regime of modern glaciers[1] throw indeed much light on our Late-Glacial conditions, but also clearly show the complexity of the interaction of factors. The warning against drawing direct climatic conclusions from the speed of recession of the edge of the ice (AHLMANN 1942 a) is clearly well-founded. But when the ice front on fairly level ground (here sea bottom) was retreating as rapidly as around Uppsala, in spite of the forward movement of the ice mass itself (as proved by annual(?) moraines, rather common in our region) it must mean comparatively high summer temperature (even with full allowance for other possible factors). It has naturally been long recognized by glacial geologists that the extension of the ice in Yoldia time was not a direct function of the then climate only; the ice was, at least in the border regions, a relict from earlier, more severe climatic conditions (AHLMANN 1942 a, CAILLEUX 1942, quoted later, compare also VON POST 1918, p. 22, 1933, table pp. 58—59).

Botanical evidences play an important part in the reconstruction even of Late-Glacial climate. The considerable frequency of *Pinus silvestris* in pollen spectra from Middle Sweden corresponding to the Fini-Glacial Yoldia time (see for instance M.-B. FLORIN 1944, p. 430. S. FLORIN 1944, pp. 560—562, figs. 2—4) may indicate a comparatively favourable climate, even though certain possible complications should not be overlooked. The susceptibility of pine pollen to long distance transport tends to give high *Pinus* percentages in arboreal pollen spectra of certain non-forested regions (AARIO 1940, p. 65, see also VON POST 1947 a, p. 208, on *Pinus* pollen in arctic tundra deposits). For sediments the possibility of secondary pollen (IVERSEN 1936, and later papers) should be considered.

Pollen spectra of supposedly Yoldia age in greater distance from the then land ice strengthen the impression of fairly favourable climatic conditions, but the chronological combinations are not quite definite (T. NILSSON 1935, p. 484—485). Even for regions comparatively near the Late-Glacial ice border, time relation between the old phases of development of the vegetation and the retreat of the ice front is difficult to establish with reasonable accuracy and reliability. That some features in Late-Glacial pollen diagrams and climatic development can be recognized over

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other publications by AHLMANN and co-workers. Other factors worth considering in the retreat of an ice margin are discussed by BRENNER 1944, p. 32.

[1] See numerous glaciological papers in Geografiska Annaler from 1929 on, such as AHLMANN 1933, 1940, 1942 b, AHLMANN and THORARINSSON 1938. Also other publications by AHLMANN (1939, 1941, etc.)

considerable distances has however been proved by VON POST 1947 a, p. 209, as mentioned below.

Even from as distant a region as the foot of the Alps certain deposits indicating improved climatic conditions can, with reasonable approximation, be synchronized with our *Lucioperca.* WELTEN's (1944) Faulenseemoos studies, including an independent Swiss varve chronology, registers from the former Faulensee near Interlaken, »Einsetzen der Wasserbesiedlung (Seekreidebildung) um 7550 v. Chr.», and at the same time »scheinen sich alpine Trockenrasen in der Gegend auszubreiten».

For some time, the discussion on Late-Glacial climate of the Baltic region has been especially lively in Finland; part of that discussion evidently also concerns the place and time now under our consideration.

Modifying his earlier (HYYPPÄ 1936, pp. 455 ff. and plate VIII)[1], much discussed view on the Late-Glacial climate (»Spätglaciale Wärmezeit» with July temperatures 12—16 during the formation of the great Fennoscandian moraines, in Finland Salpausselkä, thereafter renewed »subarktische Zeit» during Yoldia stage, cf. however 1937, p. 216 about chronological uncertainty) HYYPPÄ 1941 (diagram Fig. 1, p. 604 ff., especially 606—609) holds that in Yoldia time[2] climate grew warmer than in the preceding period. Summer temperature, at the latest in the beginning of the Rha time (for its dating see SAURAMO, for instance 1942, p. 238: some 6800 B.C.) was at least as favourable as in our present time. (HYYPPÄ 1941, p. 609). The border of the land ice was, to be sure, followed by a tundra — or almost a tundra — zone (AARIO 1943, p. 103 ff., 1944, p. 696 ff.) and corresponding climate, which, however, did not reach far out from the edge of the ice.

That the woods in Fini-Glacial time did not, as has sometimes been suggested, reach practically clear up to the Fennoscandian ice front, when on land, is shown by certain considerable Late-Glacial inland dunes in Sweden, but field evidences also indicate that it did not take long before dune formation ceased; the region evidently soon became covered by vegetation. (IVAR HÖGBOM, 1923, p. 248. HÖRNER 1927, p. 165—167. Concerning I. HÖGBOM's time limit for the Mora dunes cf. however the consequences of V. POST's, 1934, pp. 43, 57, dating of the Bonäs shore line.)

More definite morphological indications of Late-Glacial climate have recently been discussed by VON POST (1945, p. 40).

A glance at the pollen content of the Fini-Glacial varve clay confirms

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[1] Cf. also HYYPPÄ 1933, LUNDQUIST 1928, p. 144, THOMASSON 1935, p. 607, SAURAMO 1938, p. 223.

[2] When the usual classical terminology is used here, it should be remembered that HYYPPÄ later, 1943 a, b (preliminary reports) has arrived at a very different conclusion about the Late Quaternary history of the Baltic and consequently introduced other terms.

the conclusion that even at so late a stage of recession, the land-ending ice-front must have been bordered by a forestless belt. Microscopical examination of the clay concerned, without special preparation, as a rule reveals no pollen, at least no arboreal pollen; numerous slides may be carefully searched without even a sign of a tree pollen. Treatment with hydroflouric acid according to the method of ASSARSSON and GRANLUND 1924, and subsequent chlorination etc. (ERDTMAN 1943, p. 34 ff.) shows however that the clay is not quite without pollen, though in the little there is, arboreal pollen is by far outnumbered by non-arboreal pollen and spores, only partly identified. Though looking especially for tree pollen, I found only six: four *Pinus* and two *Betula*, altogether, in 3 slides. Pollen of *Graminae* type are comparatively plentiful, there are also some *Ericaceae* tetrades; a *Lycopodium* spore or two were noticed. No attempt at a regular NAP-analysis was made. That far out from land (remember map, fig. 8, p. 211), different distribution factors are likely to have highly distorted the pollen spectrum that should have been characteristic of the flora of nearest land area (FAEGRI 1945, pp. 78, 82 etc.).

More attention than before to NAP in Late-Glacial clays of middle Sweden may help to throw some further light on the rapidly changing geographic conditions of that time.

AARIO, who himself once presented what was then considered conclusive evidence[1] of the forest's following closely the retreating ice, later, on the strength of his important and comprehensive studies of actual vegetation and contemporary pollen frequencies (1940, 1943, 1944) with full attention paid also to other pollen than that of trees[2], strongly opposes the idea of a Late-Glacial »Wärmeperiode». AARIO (1944, p. 702) summarizes: »Die Yoldia—Rho—Periode ist noch ziemlich kühl gewesen, wird aber allmählich günstiger. Im Laufe derselben wird Finnland eisfrei. Die Baumgruppen werden immer zahlreicher.» (Pine and birch in eastern, birch in southern Finland.) — — — »Gegen Ende der Rho—Periode (Yoldia IV) war das

[1] Cf. VON POST 1924, p. 104 concerning difficulties in detecting whether a region was, in a time passed, devoid of forest or not, and in deciphering passed changes in the extent of timbered areas, difficulties that can hardly be satisfactorily overcome in pollen studies of the original type, concentrating almost exclusively on tree pollen, as many of the available pollen diagrams from middle Sweden even in the parts reaching farthest back in time. See also several papers quoted in next note, for instance IVERSEN 1947, p. 72.

[2] Data on NAP (Non-arboreal pollen), NTP (Non-tree pollen), or NBP (Nicht-Baum-pollen) are also given and discussed in HYYPPÄ's later works (see for instance 1941) for the general importance and effect of the NAP (and absolute pollen frequencies) in reconstructing the vegetation of Quaternary (inclusive Post-Glacial) periods, see also papers by FIRBAS, FAEGRI, GAMS, and IVERSEN, quoted by AARIO, also IVERSEN 1942, 1947, ERDTMAN 1943 b, FAEGRI 1943, 1945.

Klima schon so günstig, dass die Birkenwälder und Birken–Kiefern–Mischwälder fast überall in Finnland einheitlich wurden.»

VON POST (1947 a, pp. 208—210) states that tundra conditions in S.W. Sweden came to a definite end and the development of closed forests began at the time of the *Betula* maximum BM, only 100—200 years before G. DE GEER's zero year (= about 6800 B.C.), thus half a millenium after the time of our fish. To be sure, tundra[1] conditions had grown less pronounced between VON POST's stages P/B, in Late Goti-Glacial and BM, but there had been at least two relapses marked by minor *Pinus* maxima, caused mainly by *Pinus* pollen heaving »flown» from greater distances into what was fundamentally a tundra setting. Of those relapses, Pm[1] corresponds chronologically to Y IV, the time of the deposits discussed in this paper.

Above climatic statements primarily refer to the Viskan region in S.W. Sweden, but certain results of HYYPPÄ 1936 prove, according to VON POST, fundamental analogies as far away as in the southern part of the Fenno-Russian border region.

From different parts of southern and central Sweden traces of Late-Glacial nival solifluction have been reported, indicating temporary sharpenings of the then climate (VON POST 1945, pp. 39—40).

An interesting parallel to VON POST's above statement about the definite change from tundra to closed forest occurring as late as 6900—7000 B.C. is WELTEN's (1944) conclusion concerning the Faulensee region in Switzerland. »Eine starke Klimaverbesserung» around 6750 B.C. ended the *Salix* time (Weidenzeit) and favoured Hippophaë, soon (around 6630) to be overwhelmed by arboreal birches: *Betula pubescens* and *B. verrucosa* (loc. cit., pp. 94 ff., 140, 192). That the improvement of the climate, though well on its way at the time of the fish, still had centuries left to anything like a first climax thus holds true not only for Fennoscandia. In the Alps as well as in Fennoscandia, the cooling effect of considerable ice remains, melting overdue, may have meant a local »relict element» in a climate that would otherwise already then have been more favourable.

HESSLAND's (1943) comprehensive studies on the shell deposits of northern Bohuslän (on the North Sea coast of Sweden) have shown that a close parallelism between temperature conditions on land and in the sea cannot be taken for granted. That there must be some relationship seems evident, especially for such a closed-in basin as the Baltic. The time of our Uppsala fish fits into the first millenium during which in northern Bohuslän the immigration of boreal-lusitanian species dominated over that of arctic, arctic-boreal, and high boreal species. The dominance is very marked, even though the total immigration to the Bohuslän waters had a

[1] The term tundra, used for Late-Glacial conditions of southern and central Sweden, should not unconditionally be interpreted as fully identical with present-day arctic tundra. Cf. IVERSEN, in DEGERBØL and IVERSEN 1945, pp. 56—57 concerning Denmark.

minimum, not yet fully explained, around the time approximately corresponding to the Baltic Yoldia period[1] (HESSLAND 1943, p. 288).

AARIO (1944, p. 703), for his region and line of approach, stresses the necessity of further investigation, but states that, whereas there may be minor changes especially concerning the Yoldia period, the main results of the unfavourableness of the Late-Glacial climate, but also of its general improvement during Late-Glacial time, are not likely to change.

Our *Lucioperca* specimen from, at least approximately, the year −734 in the DE GEER chronology (nearly 350 years after the beginning of the Yoldia time according to G. DE GEER) may offer some additional information to our insufficient material on the climate of the Yoldia time. So near to the ice front as our fish died, there couldn't very well have been satisfactory living conditions for that species, the water must necessarily have been rather cold with all those drifting icebergs, that carried and dropped boulders and finer débris found here and there in the varved clays — in some layers regularly and abundantly.

But our *Lucioperca* proves that somewhere in no greater distance than could be covered by the fish there were conditions favourable enough for thriving and reproduction of that species with its rather high temperature requirements.

The newly found fish thus is another confirmation that the Late-Glacial »Arctic Sea» of our region was neither a sea proper, nor properly arctic. Our fish fits well into the characterization in which ANDRÉ CAILLEUX has recently (1943, p. 78) summarized the conclusions of many scientists, including his own, concerning the Fini-Glacial climatic conditions of Fenno-scandia: »Au total, ce sont des conditions non plus périglaciaire, mais tempérés».

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Credit is due to Mr. V. ERIKSSON and to the Vaksala Brick Works for noticing and taking care of the fish remains and turning them over for study.

[1] The linking up of the »sub-fossil» Bohuslän shell fauna with definite heights of the sea level in relation to land (one of the factors on which the dating rests) may not throughout be quite conclusive (HÖRNER 1945). HESSLAND's attempts in dating the shore levels, around Yoldia time rest however on firm foundation, information provided by SANDEGREN (see 1943 b, p. 14) for the years around 7000 B.C. and 6350 B.C. Even with allowance for considerable uncertainty in some connections, the dominance of boreal-lusitanian immigration over arctic-boreal in the millenium 8000—7000 B.C. seems established.

Help and advice was generously rendered by several zoologists: Professor HJ. RENDAHL and Miss GRETA VESTERGREN, M.Sc., of the Stockholm Museum of Natural History who from the incomplete remains identified the species, Professor SVEN EKMAN of Uppsala who put at my disposal his wide knowledge and comprehensive notes on the distribution and living conditions of Scandinavian fishes, Dr. BENGT LUNDHOLM whose keen and inspiring interest and valuable suggestions were always at hand, and other members of the staff of the Zoological Institute in Uppsala for dissecting a newly caught *Lucioperca* for comparison. The staff of the Swedish Government Board of Fishery rendered information and also some material for comparison. Professor JOHN VAN OOSTEN of Ann Arbor, Mich. contributed information and reference on American pikeperches.

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DGU stands for Danmarks Geologiske Undersøgelse.
GFF » » Geologiska Föreningens i Stockholm Förhandlingar.
KVA » » Kungl. Svenska Vetenskapsakademien.
NGU » » Norges Geologiske Undersökelse.
SGU » » Sveriges Geologiska Undersökning.

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