EXPERIMENTS IN CONNECTION WITH DALY'S HYPOTHESIS ON THE FORMATION OF SUBMARINE CANYONS

BY

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CONTENTS.

I. INTRODUCTION.

For many years the existence of a deep gorge off the mouth of some larger rivers has been known. These descend the continental slopes to considerable depths. It remained to be shown by recent echo-sounding surveys, especially along the coasts of the United States, that they are of frequent occurrence. To SHEPARD[1]) we owe not only that attention has been centered on these most interesting and curious features of the submarine morphology, but also many original investigations into their nature and a number of excellent charts. STETSON[2]) published the results of dredging operations carried out in a few of these canyons. He kindly wrote me some recent results also. HESS made observations on similar formations in the Bahama's[3]).

The chief characteristics of the gorges are as follows: They are comparable in depth and steepness to the largest subareal canyons. They start at the edge of the continental shelf or even cut across it for some distance and slope outwards gradually and continuously to a

[1]) F. P. SHEPARD: Submarine valleys. Geogr. Rev., Vol. 23, 1933, pp. 77—89.
F. P. SHEPARD: Submarine canyons of the American coasts. Zeitschr. f. Geomorph., Bd. IX, 1936, pp. 99—105.

[2]) H. C. STETSON: Geology and palaeontology of the Georges Bank Canyons. Bull. Geol. Soc. Am. 1936, pp. 339—440.

[3]) H. H. HESS in: The Navy-Princeton gravity expedition to the West Indies in 1932. U. S. Hydrogr. Office, 1933.

depth of some 3000 meters. The lower reaches have not yet been examined in sufficient detail to know accurately how they end. In general they run comparatively straight, down the continental slope, but instances are also known in which the course is sinuous. Apart from a few exceptional cases there are comparatively few tributary gorges, but where tributary valleys occur they are neither hanging nor overdeepened. A few cases have been found where the valley system has a dentritic pattern. Although several canyons are opposite the mouth of a river, there are many more that occur where there are no river-courses and even where there is no hinterland of any extent.

As to the nature of the rocks out of which the gorges have been eroded opinion is devided. While SHEPARD found that the walls are generally composed of hard materials, STETSON observed slightly consolidated sediments cropping out in lightly dipping strata. The following quotation from STETSON is from a letter of February 1937:

> „We have only succeeded in finding hard rock on Georges Bank. „The canyons from Hudson Gorge south are full of a clay, probably „Pleistocene, which acts as a fill in an older cut and effectively „blankets the older formations beneath. In all, we have found three „hard sandstone formations which can be considered to be in place. „The other formations which make up the canyon walls are compact „clays and friable glauconitic sands. It is, of course, possible that „lithification took place after the canyons had been formed, but this „seems scarcely probable.''

Recent geophysical work points to weak materials in so far that the shelf appears to be built out from the coast in the manner of a huge delta at the investigated spot [1]).

Many attempts have been made to account for these submarine valleys. HESS considered the possibility of a sudden alteration in the speed of rotation of the earth. Apart from the obstacles to this picture from an astronomical point of view, SHEPARD draws attention to the fact, that no canyons could be formed by this mechanism at 35° latitude [2]).

DAVIS thought that the gorges might have been formed by undertow currents owing to the banking of water against the coast during storms. But in tropical regions the surface waters are too light on account of their high temperature, ever to flow out down the continental slope.

If a subareal gorge were submerged by subsidence of the coastal area, it would be gradually blotted out by sedimentation. This shows, as SHEPARD argued, either that they must have been formed recently or that they are kept open after subsidence by some mechanism that is still active. The geological history of the coasts off which gorges are found, in many cases precludes the possibility of recent subsidence. Moreover, there gradually appear to be so many area's with gorges, that a recent subsidence of all these is in itself an impossibility.

[1]) Personal communication from Professor VENING MEINESZ.

[2]) F. P. SHEPARD: Submerged valleys on the continental slopes and changes of sea level. Science, Vol. 83, 1936, pp. 620—621.

Fig. 1.
Model of three canyons on Georges Bank. After a chart prepared bij SHEPARD.
Horizontal and vertical scale the same.

For these reasons SHEPARD first inclined to the opinion, that they were generally of greater age, but were periodically cleaned out by submarine sliding of the mobile sediments collecting in them. This slumping of the mud in the gorges could be expected to take place especially during earthquakes.

Recently SHEPARD abandoned this explanation as being of general applicability and prefers to assume subareal erosion during the ice age [1]). He recalculated the eustatic shifts of sea level during the maximum glaciation. If the ice were thicker than is generally assumed and the area increased by adding several regions in which glaciation has not yet been proved but is not disproved either, then the level of the oceans may have sunk as much as 1000 meters. During these low stages of sea level the upper reaches of the gorges were eroded by running fresh water.

In the opinion of the writer an eustatic shift of 10 times the amount that has satisfied geologists for many years, both from glacial and many other data, does not seem probable. The chief objection to his mind, however, is that there is no indication in the submarine topography that the upper 1000 meters of the gorges was formed or greatly influenced by other agents than the deeper parts. Thus there is no break in the general outward slope of many canyons at this level. One would expect a certain amount of sag in the upper part of the nature found in practically all river beds between their source and their mouths at base level. One might also expect to find some indication of delta's at the low base-level, where the bulk of the material was deposited that the erosion carried away from the developing gorge. The fine bathymetrical charts that SHEPARD provided lend no support to the assumption that base level moved some way down the continental slope.

The hypothesis shortly brought forward by DALY [2]) is striking both in its simplicity and its originality. On the other hand it is exceptionally difficult, as DALY himself admits, to form an opinion as to its value for explaining the formation of the gorges. During the ice age, when the ocean level sank several dozens of meters, a large quantity of mud and silt was brought within the range of turbulance caused by storm waves. During exceptionally severe storms the waters on the shallowing flats must have churned up much more sediment than nowadays. In this manner a suspension was formed with a higher specific gravity than that of clear sea-water. This heavy liquid must have developed a tendency to flow out from the shelves and down the continental slope. These submarine currents, given sufficient speed of motion, must have eroded the bottom. If the slopes are actually composed of fine, unconsolidated detritus, deeper and deeper ruts would have been scraped out, resulting finally in the present submarine canyons. This mechanism would account for the recent formation of the gorges and their similarity to normal waterworn canyons as they are formed by a current flowing

[1]) F. P. SHEPARD: The underlying causes of submarine canyons. Proc. Nat. Acad. Sc., Vol. 22, 1936, pp. 496—502.

[2]) R. A. DALY: Origin of submarine „canyons''. Amer. Journ. Science, Vol. XXXI, 1936, pp. 401—420.

along the bottom. Another attractive feature of the theory is, that no appeal is made to highly improbable astronomical or glacial occurrences. No theory has yet been proposed by which one could explain a world-wide sinking of ocean level to the present position of the lower canyon ends and a recent sinking of all the area's where canyons are found is unlikely. SHEPARD's original theory of slumping is far from satisfactory on account of the general shape of the canyons and the slightness of the slopes. We are therefore forced to seek for a mode of formation, that could operate below sea level. Normal currents are not sufficiently restricted, properly directed or swift enough to form gorges down the slopes. The fine recent sediment now being deposited in the gorges, is also evidence against a mechanism that is in operation at the present time. We are left with the choice between admitting the probability of DALY's hypothesis or owning that we are completely baffled. This should make us specially careful not to reject the explanation offered on slight grounds or a mere feeling of improbability. Only objections that appear to be quite insurmountable can turn the scales against the suspension current theory. So far the only objection the writer is aware of, would result from conclusive proof that the continental slopes consist generally of firm, hard rock. As stated above this is still a moot question. In some cases, further, the area of the shelf appears to be too small to have supplied the necessary sediment. After performing experiments to illustrate the hypothesis several objections he entertained have been met. Several geologists who witnesed the experiments admitted being more favourably disposed towards the theory than at first.

II. EXPERIMENTS.

DALY remarks, when discussing his hypothesis, that it appears to be beyond experimental geology to prove or disprove whether the submarine gorges were formed by a suspension flowing down the continental slope. The writer is the first to admit this statement. On the other hand careful consideration led him to believe that experiments might help to clear the field of speculation. For there are several elements in the line of reasoning, that are irrespective of scale and that nevertheless form basic parts of the whole theory. The following questions can be answered or approached on the basis of experimental investigation:

1. Will a suspension flow down a slope under water?
2. Will this type of current continue to a considerable depth without loosing its motive force in consequence of mixing with clear water?
3. Has such a current any erosive power?
4. Will such a current follow a slight initial gorge? For if not the mechanism can never develop a gorge when starting to work on a comparatively smooth slope.

5. Is the rate of flow increased by enlarging the scale of the experiment? In other words can swifter currents be expected in nature than in the laboratory?

Professor ESCHER of Leyden university kindly gave me permission to carry out the experiments in his laboratory for experimental geology and gave me advice. It is a great pleasure to me to acknowledge his helpful encouragement. Professor BURGERS and Ir. THIJSSE both of Delft advised me on some hydrodynamical questions.

The procedure was as follows. A suitable slope was built of sand in a long tank with glass sides. Either the whole slope or the top part was covered with a layer of gypsum. The slope consisted of a steep coast, an almost horizontal shelf and a continental slope to the bottom of the tank. Clear water was then brought into the tank so as to cover the shelf with a few millimeters or centimeters of water. A suspension of clay was prepared, generally one liter, and poured onto the flat along the coast. In order to be sure, that the turbulance and initial rush off the steep coast did not influence the flow, a precaution had to be taken. The suspension was therefore poured behind a piece of wood with a rubber flange, that prevented the mud flow from moving across the shelf until the turbulance had subsided. This temporary dam was moved outwards about half way towards the edge of the shelf while the suspension was being poured behind it, at such a rate that some clear water trickled under to join the suspension. In this manner it was possible to reach a stage, at which the flat was half covered with clear water and half with an almost stagnant, milky suspension. In some experiments the suspension was substituted by a solution of rock salt, coloured with potassium permanganate. There were several reasons for this substitution. The suspension very quickly cleared, the clay sinking to the bottom, in consequence of electrolites in the water that had to be used. The specific gravity of the suspension could therefore not be kept in hand, while an experiment was in progress. The salt water had the advantage, that its specific gravity remained constant, that it could not deposit new sediment and that being clear, erosion could be followed during its flow.

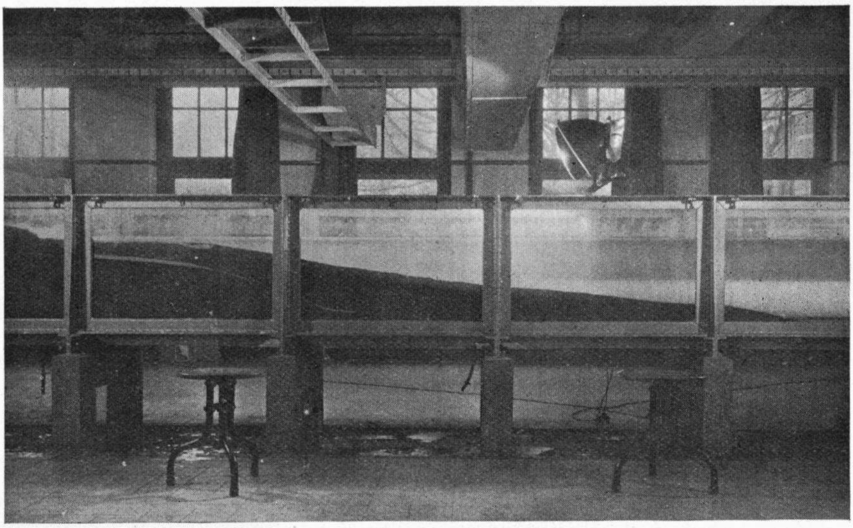
The suspension was also troublesome in that it was not sufficiently mobile if made too thick, and moved slowly if too dilute, giving ample time for the clay to settle before the current had properly started.

Fig. 2.

A. Part of the tank in the Leyden laboratory. Each pane of glass is one meter broad. On the left: shore, fronted by shelf, both with a hard layer of gypsum. The shelf is just covered by the water. The lower part of the water in the tank is opaque with clay, carried down by repeated flows (note the absence of mixing of clear and muddy water). The white line in the sand shows a surface of gypsum on which the first experiments (Fig. 4, A, B) were made.

B. Concentrated suspension of clay seen from above, flowing down the gorge of fig. 7 from left to right. Note slight turbulance at the head and the slower progress of the flow outside the gorge.

C. Dilute suspension of clay, almost without turbulance. The flow is entirely concentrated in the gorge of fig. 7.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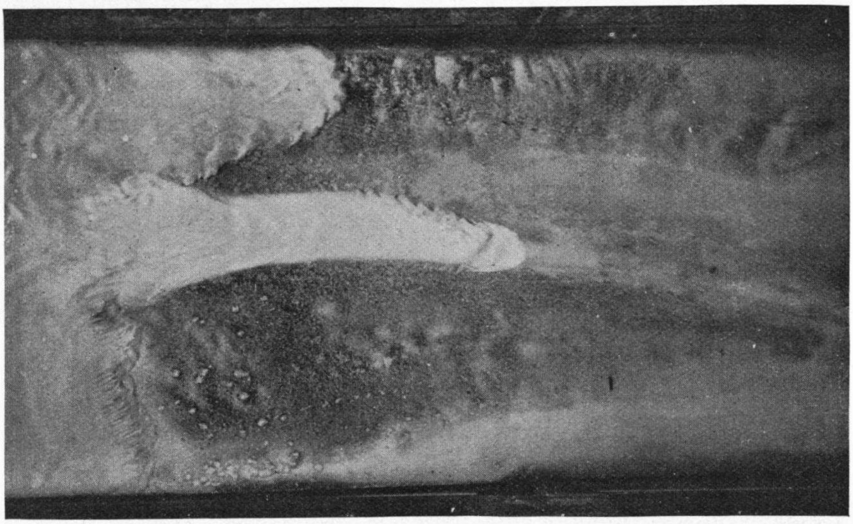
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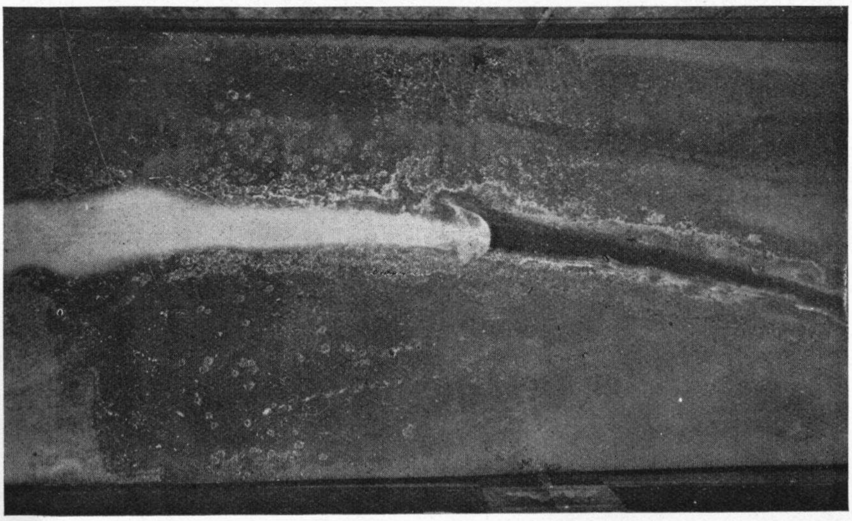

C

The experiments to test the first two questions consisted in bringing a suspension onto the flat in the manner described, and watching its flow down a smooth slope of gypsum. The suspension immediately started to creep out over the shelf when let go. It then poured over the edge and swiftly flowed right down the slope, spreading out along the bottom of the tank in a thin mist to a considerable distance. The frontal portions of the current were in cloudy turbulent motion [1]) and were followed by a calmly flowing current with laminary movement. The first impression is, that the head of the flow is quickly mixing with the clear water through which it is flowing, but closer inspection reveals, that this is not the case. The lower surface is nearly flat, floating a few mm over the bottom as shown in fig. 6, A. The upper surface is billowy like cumulus clouds, but in continuous intricate motion. But when the frontal part has passed the water above the tail is practically clear, and the cloud itself is not diluted noticeably. Evidently there is hardly any mixing between suspension and clear water. Although the dividing surface is highly warped and contorted it remains a sharp margin and soon flattens out when the billowy part has passed by. When the suspension is more dilute to begin with, it flows more slowly and the frontal part gradually flows down the slope like a flat tongue (fig. 6, B) (see also page 343).

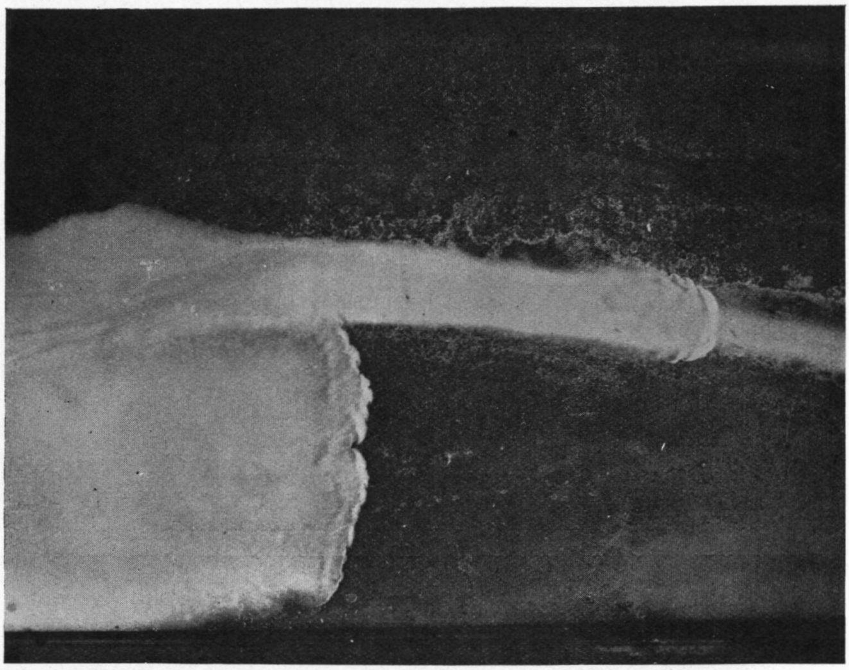
When a salt solution with or without a suspension is treated in the same manner the type of flow is identical, but as the specific gravity can be made greater the speed of motion can also be increased.

The first two questions can therefore be answered in the affirmative.

The third and fourth question were studied in the same manner, but a gorge was made in the continental slope. First two comparatively deep gorges were modelled out of gypsum, the one cutting well into the flat, the other starting on the slope, and meeting the first some way down. Both had a slightly sinuous course. It was found that a suspension flowed out over the shelf, but as soon as the edge reached the upper end of the gorge it poured into this depression, sucking in the milky fluid from both sides (see fig. 2 B, 3 A). Only when a large amount of suspension was brought onto the shelf, did it succeed in reaching the edge and flowing down the slope beside the gorge for a short distance. The current in the gorge, however, was much swifter and more powerful. Even a short way down the slope it had sucked in the suspension from the sides. In the lower reaches no suspension ever succeeded in flowing anywhere else than along the gorge.

After the clay had settled, a very fine smokey suspension remained and this continued to move off the shelf and down the gorge for almost an hour. By throwing crystals of potassium permanganate onto the slope or shelf the movements of the water could be followed with the greatest ease. On the slopes beside the gorge the purple tongues, dissolving off the crystals, moved downwards only very slowly in consequence of their greater specific gravity. In the gorge, however, the movement was many times as swift in consequence of the movement of

[1]) One is strongly tempted to draw a parallel with the glowing clouds photographed on the slopes of the Merapi volcano on Java, or with snow lawines.



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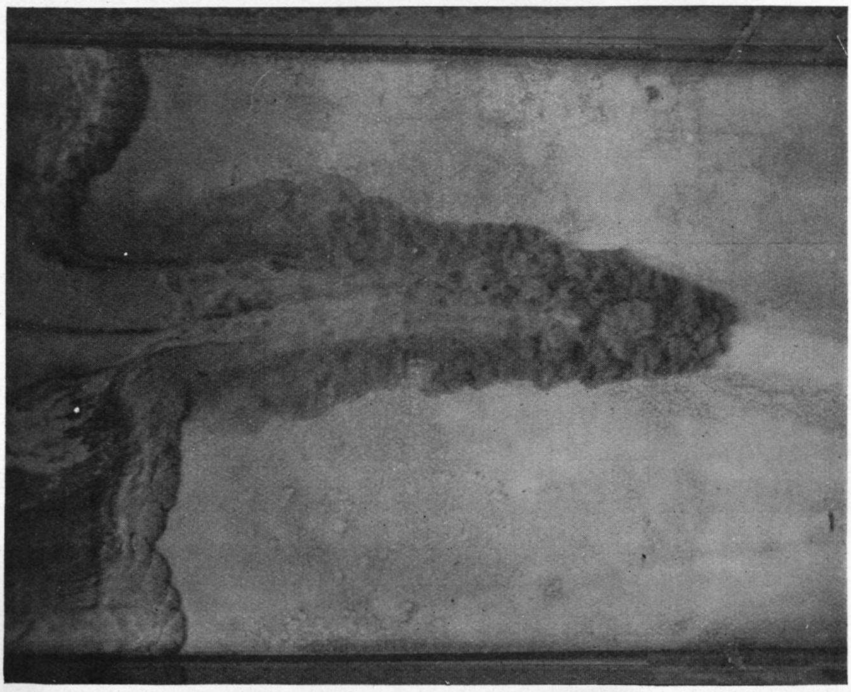
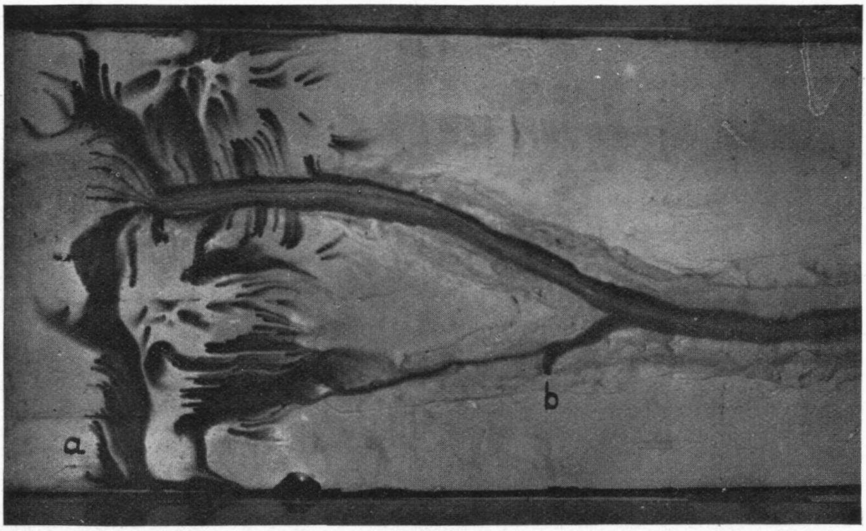


B

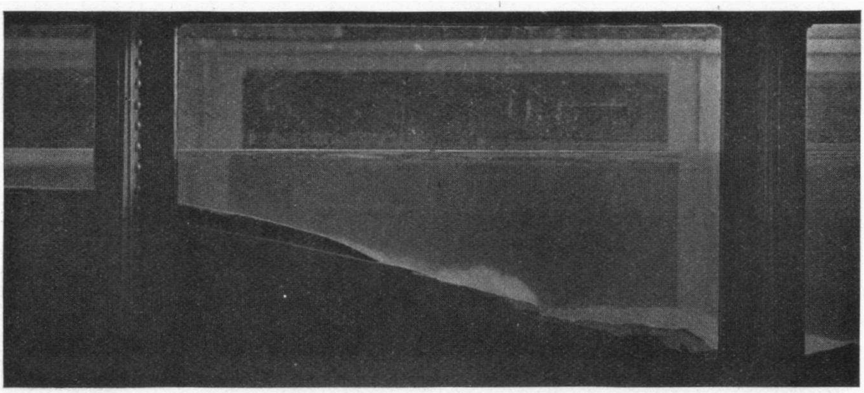
Fig. 3.

A. Concentrated suspension running down the gorge of fig. 7. Note how the part outside the gorge is being drawn into it.

B. Concentrated suspension, weighted by rock solt, with strong, turbulant motion, in the gorge of fig. 7.



A



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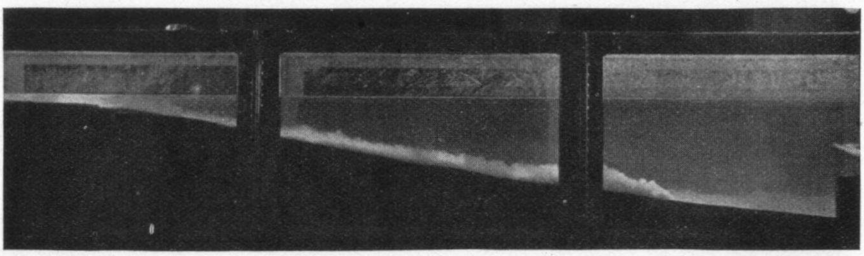

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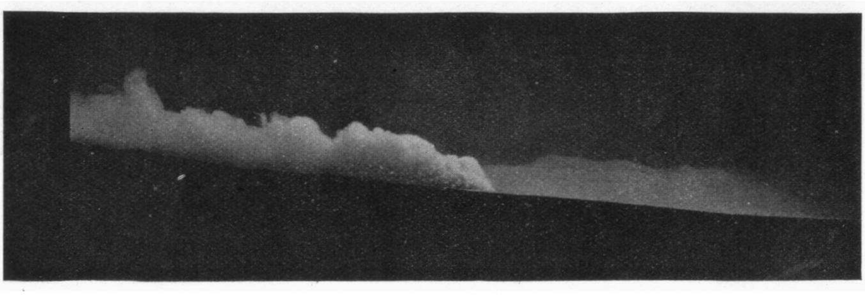

D

the fine suspension. On fig. 4, A the dark streaks clearly show how the currents into and down the gorge were directed.

After the suspension had settled it was also possible to churn up a new cloud on the shelf with the fingers. The result was the same, but eddies were made and these sometimes spoilt the elegance of the results.

By repeating this churning up practically all the clay was carried away from the shelf and deposited in the gorge and on the bottom of the tank.

To decide the question whether the current had any erosive power, or moved over the bottom without disturbing the sediments, a flow of clear salt water was sent down the gorge after all the clay had settled. The solution started on the flat in the normal manner and then dashed down the gorge raising dense clouds of clay on its way. On fig. 4, B the white cloud may be seen caused by the churning up of sediment by a current that started as a clear flow at the top of the gorge.

As the surface of the clay was very smooth, the current flows over it without being able to stir it up, except in the turbulent frontal parts. If little chips of gypsum, minute balls of clay or fine pumice sand were dropped into the gorge these were frequently rolled along the bottom a considerable distance, long after the clay had ceased to be moved, or by a current that was too calm to disturb the clay at all.

In a following experiment a more gradual slope was built up with only a slight gorge running down the middle. Four successive layers of clay were then laid down over the entire surface by mixing a suspension with the water of the tank; first a white layer, then brown, white and finally again brown. Each layer was about $\frac{3}{4}$ mm thick. On this model a series of observations was made to compare the rate of flow of various concentrations and amounts of salt water. After these were finished the gorge was reexamined and considerable erosion was found to have taken place. The upper two layers had disappeared over almost the entire length, the third layer had started to flake off here and there. Moreover it became apparent that the current had eroded more strongly along the outer bank in curved parts than along the inner bank.

In fig. 7 the gorge is drawn to scale to show its shape, dimensions and slope and fig. 2, A gives a picture of the tank with lamps and slope.

It will be seen, that the upper end of the gorge forms only a very

Fig. 4.

A. Branching gorge with very dilute suspension (not visible on the photograph) slowly flowing down. The movement is shown by the dark streaks coming off crystals of potassium permanganate. (At „a'' the flow dives into a cavity and emerges again at „b'').

B. Cloud stirred up on the way down (in the gorge of fig. 4 A) by a clear solution of salt, proving erosion.

C. Suspension (two liters) weighted with salt, flowing down a gorge along the front of the tank. Note the turbulant head and thinner part, where the motion has become constant.

D. Detail of the flow shown in fig. 4, C a few moments later.

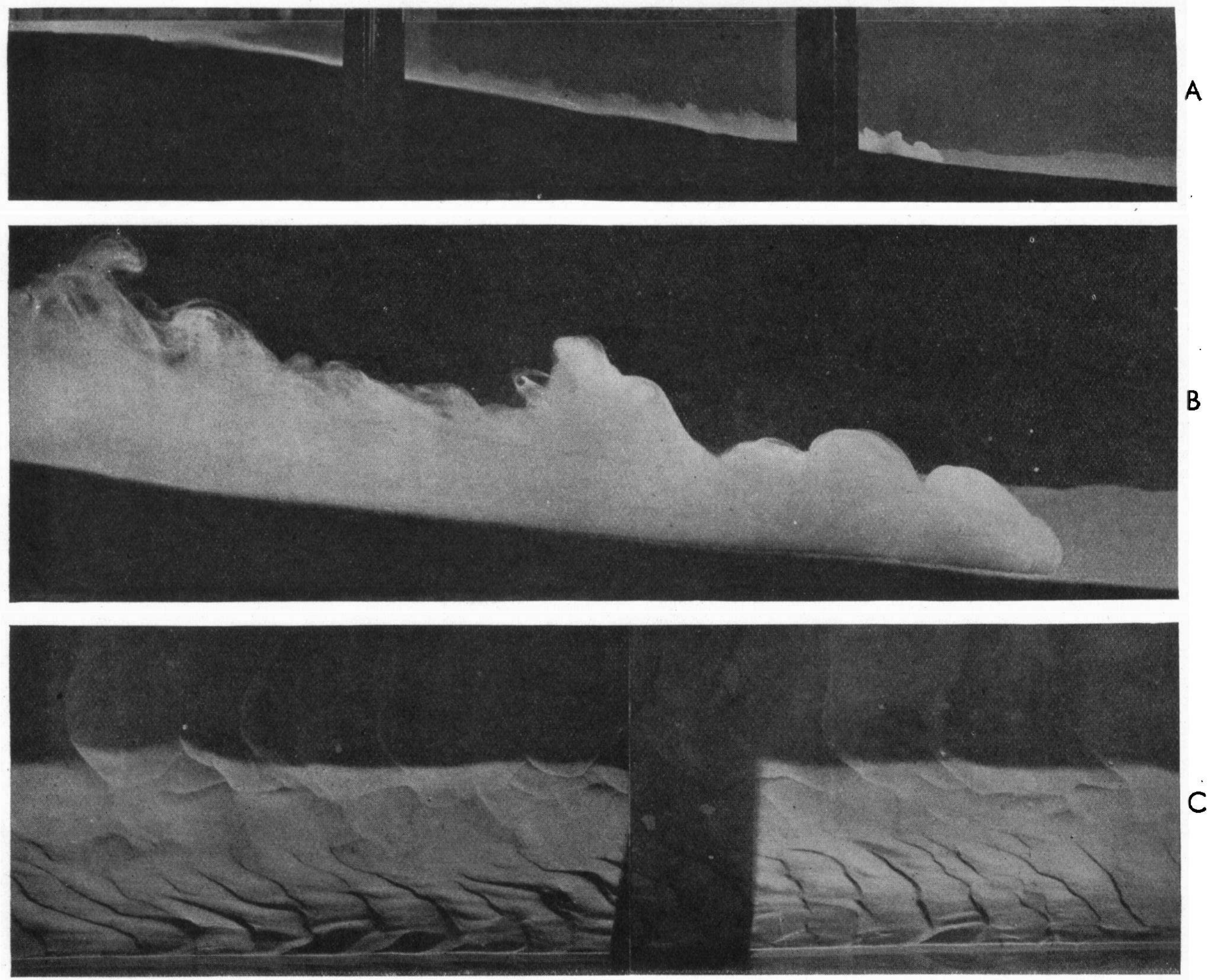

Fig. 5.

A. Suspension (two liters) following a gorge along the front of the tank.
B. Detail of the head, taken a few moments later. Note the slightly floating, turbulant head.
C. Ripples formed in the (half) gorge of fig. 5 A, seen from above.

slight dent in the shelf. Even when there is 3 cm of water on the shelf a hollow of ¾ cm depth is sufficient to tap nearly all the suspension from a distance about twice the width of the hollow on each side. In comparison the model gorge was much slighter than the finished canyons on the continental slopes.

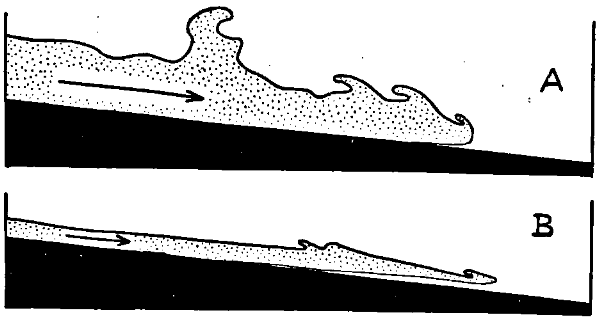

Fig. 6.

A. Head of a more concentrated and swifter flow.
B. Slower and more dilute.

So far our experiments have answered the first 4 questions quite definitively in the affirmative.

In order to gain an insight into the last question a series of experiments mentioned above was carried out. In the first run half a liter of solution was taken in each case with increasing amounts of rock salt. The average depth of the water on the flat was 6½ mm. In fig. 8 the results are seen. The stronger the solution the greater the speed of motion, shown by measuring the speed developed by the head of the flow. The velocity of the current following, after the head has passed, could not be measured. It is somewhat greater. In the second run the depth of the water was 13 mm and 1 liter was taken of each of the solutions. It will be seen from fig. 8 that the speed of motion is considerably larger. By taking 2 liters again with a depth of 13 mm the speed is not much increased and the same is the case when the depth is 23 mm. As in the experiments with 1 liter the gorge was filled to the brim by the tongue of salt water, the increase to 2 liters had little influence because the extra amount ran down the slope at both sides and was not drawn into the gorge till near its lower end. The gorge would have had to be deepensed before the increase of speed could have become appreciable. Taking this into account it is obvious on studying fig. 8 that by enlarging the scale of the experiments the speed of the flow is increased considerably. This answer to our last question is perhaps not sufficiently definite.

A further set of experiments was therefore made in a new gorge running along the side of the tank. The rate of flow was measured on increasing amounts of a solution with the specific gravity of 1.06. The

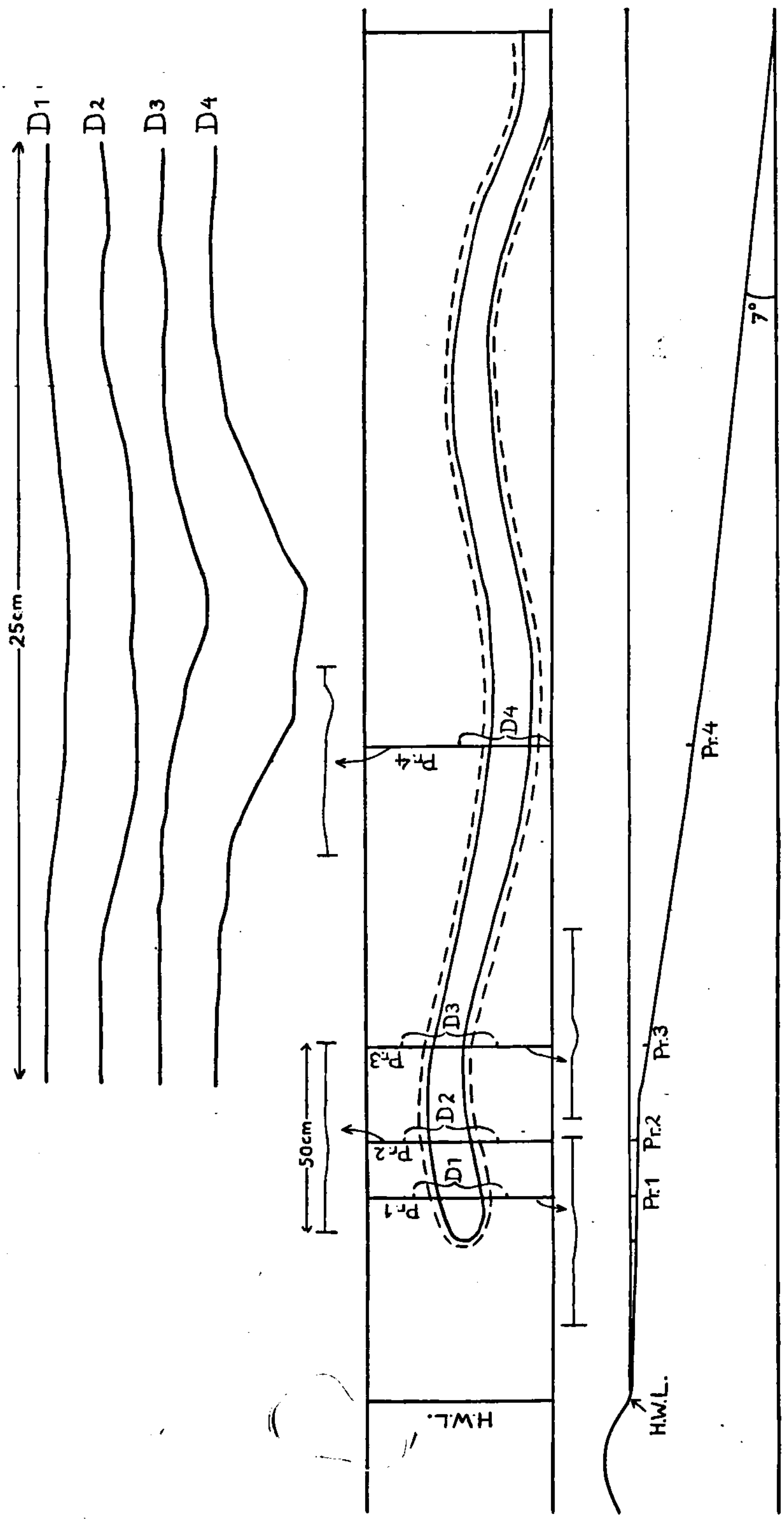

Fig. 7.

The gorge in which the experiments of fig. 8 were made.

depth of the water on the flat was also gradually increased and the gorge was made larger from time to time by heaping sand along its side. Thus the scale of the experiment was gradually increased in all respects except in broadening the shelf. Unluckily this proved to be a serious defect,

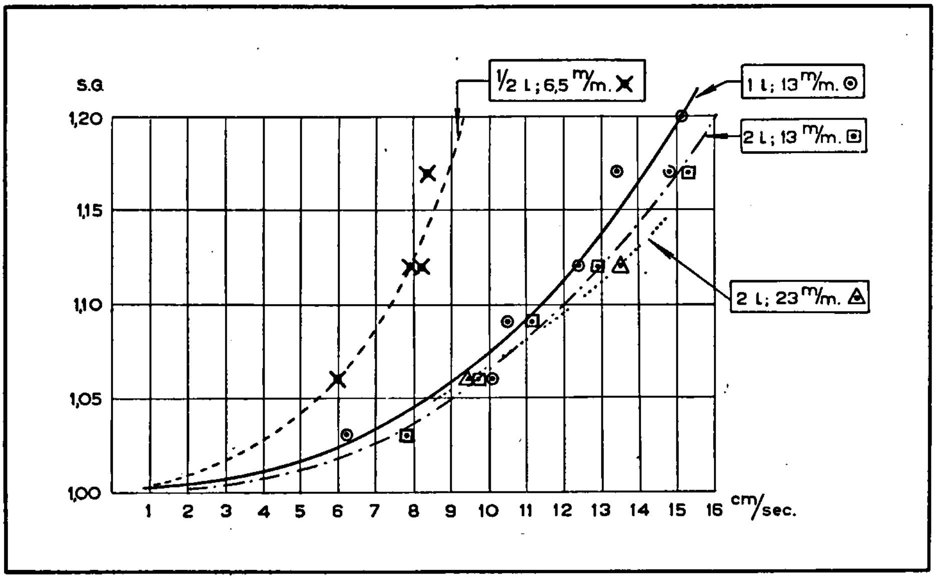

Fig. 8.

Graph of comparative experiments to show the influence of increasing specific gravity on the speed of the flow's head.

but it was not realised until afterwards. I shall return to this point in the discussion. The maximum amount that could be conveniently handled in this manner was 10 liters of solution.

The speed was measured separately for one meter in the middle of the slope, with an average of 9°, and for the next meter with an average of 6°. The curves of fig. 9 were thus obtained, showing more conclusively than fig. 8 the increasing rate of flow when the scale of the experiment is enlarged.

Another interesting fact was observed. As soon as the speed of flow exceeded some 10 cm/sec the sand in the gorge began to move and formed fine ripple marks (fig. 5, C). This is clear proof of the erosive power of the flow. The shape also shows that the maximum erosion takes place at the bottom of the (half) gorge.

Finally some experiments were made on the influence of wind-formed waves. In the Leyden laboratory a current of air can be drawn over the water in the tank. The influence was studied on a shelf with a thin layer of clay sediment, fronted by a slope. As soon as the speed of the wind was sufficient to form waves at the shore, clay was stirred up on the shelf. Before the suspension, thus formed, could flow down the slope it was mixed with clear water by the incoming waves. The water in the tank became more and more muddy without a distinct flow occurring down the slope.

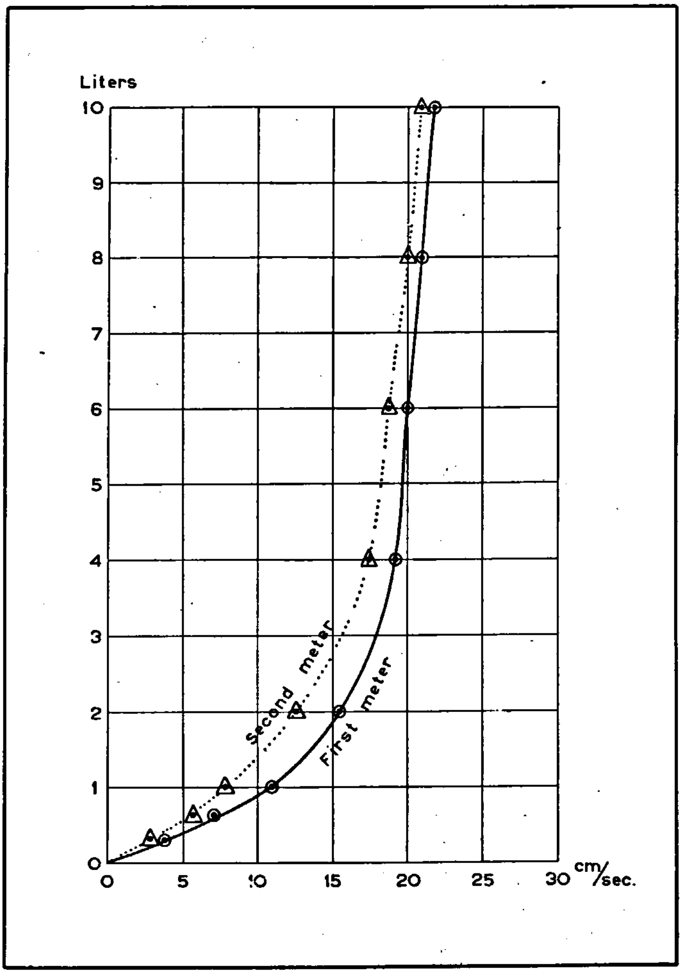

Fig. 9.

Graph of comparative experiments to show the influence of increasing scale (see discussion on page 345).

III. DISCUSSION.

An attempt can now be made to view DALY's theory while bearing in mind the answer given by the experiments to our five questions. There appears to be no objection to the assumption of a lower level of the sea during the glacial epochs. Neither can there be much doubt, that in consequence a great mass of fine sediments must have been churned up by storm waves. What cannot be answered offhand is: what the concentration of the suspension was, nor how long it took to settle when the whirling of the water died down. A storm directed towards the coast would raise larger waves than one blowing out to sea and the consequent undertow would help to sweep the suspension towards and over the edge of the flat, as DALY points out. Without studying the exact nature of the turbulance and the size etc. of the waves we cannot say if the principle noticed in the last experiment would also be active in nature. In the meantime a weak spot in the theory must be admitted as long as the possibility remains that the suspension will be mixed with clear water and diluted more and more as it works out to deeper water at the edge of the shelf. As the nature of the model waves is quite different to that of large ones in nature in consequence of the capilary forces playing a dominant part in minute ripples, the experiments cannot be used as proof that in nature the suspension cannot flown down the continental slope before it is lost by dilution.

There does not appear to be any reason for doubting the next stage in the theory. If ½ a liter of suspension will flow down a slope in the model over a distance of at least 3½ meters, the enormous amount of suspension in nature must be able to flow down the continental slope of the same order of steepness as far as the actual deep sea.

Mr. THIJSSE tells me that extensive experiments have been performed in his laboratory on the mixing of currents, but that no simple rules could be established. Mixing doubtless increases with the velocity and my experiments are on too small a scale to prove the extent of mixing for the natural dimensions. However, he is of opinion that a current of the nature here considered will be able to flow for 10 kilometers before it is too much diluted by mixing. We will see presently why the greater distance, needed to explain the canyons, may also be safely postulated.

It depends principally on the amount of silt that remains in suspension during the long hours of descent, whether the suspension of warm surface water in the tropics is heavier than the cold bottom water. If a suspension is dilute to begin with or if the turbulance during the flow is insufficient to keep a larger quantity from dropping on the way, the mixture may soon meet a level below which the cold clear water is heavier. It will then spread out in a thin tongue at this level. This case will probably not occur frequently. In the first place the temperature has a comparatively small influence on the specific gravity of sea water. In the second place, as we saw in the experiments, the flow stirs up sediment on its day down if it is not too slow and by this process may even be expected to become more and more heavily laden with sediment, thus not only regenerating but even increasing its own motive force..

As the suspension spreads out over a wide area on reaching the bottom of the slope, it will deposit its load on a much larger surface than a river arriving at base level. If the submarine gorges have been formed by subareal rivers there should be delta-like projections at the lower ends. But if on the other hand they were generated by submarine flow of a suspension, there need be only thin wide flanges of erosion products, if fine materials prodominated over coarser particles rolled along the bottom. The ration between the two cannot be estimated. If therefore future investigation of the lower ends of the canyons proves the absence of delta's, a strong point is gained in favour of the suspension theory. If they are found they can be explained both by subareal erosion and by the flow of a suspension that carried a large amount of pebbles and sand along the bottom; or finally by slumping.

The experiments leave no doubt, that a current flowing down the continental slope will concentrate in slight initial depressions. The more these depressions are eroded, the more effectively will they draw in the current and thus tend to develop into the submarine gorges we now find. The upper ends that cut across the edge of the shelf draw in the suspension at their tip, but also from the sides. They should therefore tend to develop broad, cirque-like heads and tributary gorges should be more frequent in this part. The best known examples, those of the middle part of Georges Bank, show just such a shape (see fig. 1). There are hardly any tributary gorges, starting lower down on the slope. The only clear example in the lower middle part of SHEPARD's chart possesses a kind of delta and strongly suggests an origin due to land-sliding. Stream piracy may also be responsible.

DALY pointed out, that the valleys formed above sea level during the first ice age would easily develop into deeper gorges by the flow of suspension in a later glaciation. Once started in a definite position they would extend down the slope until they reached the floor of the ocean or met an older rut, using this as their channel further down.

There is no difficulty in explaining the winding course that is sometimes encountered because the suspension can follow and erode along a sinuous rut in the same manner as a river. Irregularities in the continental slope before the mechanism started to operate, or inequalities in the strength of the country rocks may be responsible for such exceptional shapes.

It is less obvious how a dentritic pattern could be brought about because many of the ruts then begin a long way down the slope. Possibly we then encounter an old, drowned river system that was masserated out of the recent covering of soft materials. It is of importance to note that even in the example given by SHEPARD there are relatively few branches that do not begin close inshore.

Our next concern is with the rate of the flow. There appear to be two lines of investigation for arriving at an estimate of the speed in nature. DALY took the flow of rivers as point of comparison and arrived at a speed of 2 to 3 km per hour (DALY l. c. p. 418) or 70 cm/sec. We can also use the results of the experiments as basis of a calculation.

DALY says (p. 410):

„Arbitrarily assuming uniform motion in all parts of the stream's „cross-section, and also assuming the motion to have reached the „steady state, the equation relating to velocity (v) with the density „of the liquid (d), axial slope (s) taken to be the same for both „surface and bottom, and the hydraulic mean depth (m, equal to „the area of the cross-section divided by the wetted perimeter) is

„ $$v = c \sqrt{m.s.d},$$

„where c is a quantity depending on the roughness of the stream „bed and the slope of the channel.''

We have already seen that in our experiments v (= the velocity of the head of the flow) is proportionate to $\sqrt{d}$, and as s is about the same in nature and the experiments, the part that remains to be tested is the influence of the hydraulic mean depth.

Our second set of measurements must now be examined (fig. 9). The amount of solution was gradually increased, so that the depth of the heavy liquid when spread out over the shelf is also enlarged. The depth of the flow as it begins to rush down the gorge is thus in direct proportion to the amount of liquid used. But as soon as the gorge thus starts to tap the solution off the shelf, a difference arises between a smaller and a larger scale of the experiment. As the area of the shelf was not enlarged in proportion to the depth of the solution, the larger the scale the smaller the amount of solution that can be drawn into the gorge. For the smaller experiments this influence can be neglected, for the area is still large enough to provide solution as quickly as it disappears down the gorge. The ration of velocity to hydraulic mean depth therefore conforms fairly well to the formula. But as the scale is enlarged the supply of liquid to the gorge, onwards from the moment that flow starts, is increasingly encumbered.

When the breadth of the gorge begins to approach ¼ of the width of the tank the influence begins to be noticeable. The gorge receives less solution than it could accommodate and the hydraulic mean depth in the gorge, the factor determining the velocity, is not increased in proportion to the increase of solution poured onto the flat. The velocity measured should increase slower than $\sqrt{q}$ if q is the mean depth of the solution on the shelf.

Our graph clearly shows this to be the case. Bearing in mind this simple explanation of the divergence between the graph and the formula for the larger scale and the good agreement for the smaller scales, it appears fairly safe to use the formula for calculating the velocity in nature of the head of the flow. The speed of the current is greater, as pointed out above (p. 339). In the following I will not make a difference between these two values.

In nature the area of the shelf is large compared to the size of the gorge. The smaller scale experiments therefore give the most trustworthy figures.

To calculate the linear scale of the experiments we must use the depth of the solution at the moment it reaches the edge of the shelf. When using 1 liter of solution this depth is ¼ cm. The depth of the current in the gorge was not measured and could not have been estimated accuratley on account of the billowy surface [1]). In nature the thickness of the layer with suspended sediment can be estimated at 10 to 25 meters out of 20—50 meters of water on the shelf. There was of course also matter suspended in the upper layers of water. This was probably less and although it would help to increase the velocity, it is left out of account so as to arrive at a conservative estimate. As s in the experiment (1:7.4) is about twice that in nature (when the process of erosion began; namely the slope between the gorges of Georges Bank), we get the following result, for an effective density of 0.004, a figure used by DALY:

$$v_{nature} : v_{exper.} = c\sqrt{4000 \times \tfrac{1}{2} \times 0.004} : c\sqrt{1 \times 0.06}$$
$$= c\sqrt{10.000 \times \tfrac{1}{2} \times 0.004} : c\sqrt{1 \times 0.06}$$

as $v_{exper.} = 10$ cm/sec $v_{nature} = 115$ cm/sec or 183 cm/sec that is 4 km/hour or 6.5 km/hour for the head of the flow. Assuming the depth as 25 meters, an effective density of 0.01 is sufficient to cause a flow of 3 m/sec. These velocities are considerably greater than those calculated by DALY.

Professor BURGERS kindly pointed out to me, that extrapolation from millimeters to several meters brings in complications, because the frictional forces follow different laws than the forces of inertia. He believes, however, that we do obtain a value indicating the right order of magnitude. Afterall that is what we are aiming at.

He also drew my attention to similar experiments carried out by W. SCHMIDT [2]) for meteorological purposes, in which extrapolation to the scale of nature gave very satisfactory results. In these experiments the propagation of the head of the flow was found to be only half the velocity of the current following. Our estimates for the speed in nature must therefore be on the conservative side and probably come near the value for the layers close to the bottom.

When in consequence of less exceptional storms, the density of the water is increased less, the velocity of the flow may sink below the minimum for stirring up sediment in the gorge. No erosion will then take place and the current will soon stop in consequence of the sediment settling. Estimating this lower limit to effective velocity of the head at 40 cm/sec. (= two to three times the velocity that produced ripple marks in the experiments and a value considered to be reasonable by Mr. THIJSSE on the strength of his extensive experience with currents in estuaries, etc.) we find that the minimum concentration is 0.00028 by weight, that is 0.00014 or 1:7000 by volume. For each square centimeter of the shelf one quarter of a cm³ of sediment is required for this minimum velocity. It makes no difference whether this amount is

[1]) See page 348.
[2]) W. SCHMIDT: Zur Mechanik der Böen. Meteorol. Zeitschr. 1911, p. 355—362. Weitere Bemerkungen zum Böenvorgang, Meteorol. Zeitschr. 1912, p. 103—110.

suspended in a layer of 1 or 100 meters thickness; the velocity remains the same.

Returning to the question of dilution of the suspension by the waves at the edge of the shelf, it appears that if only a small fraction of the materials remains in suspension after the waves have gradually died down, there is still ample motive force for the currents down the slope to start running and to increase in velocity. One would even expect the mechanism to work at the present day, were it not that the shelf sediments have been washed clean during the glacial period. The now remaining sand settles too quickly to start the movement and the process will not become of importance again until more fine matter has settled on the shelves [1]).

Now that we have arrived at an estimate of the velocities in nature, a further test of the theory can be made to see whether it will work on the basis of a few measurements and estimates as to quantitative elements. The most easterly of the gorges of fig. 1 will be used for obtaining data.

The amount of rock eroded from the most easterly of the three large Georges bank canyons is roughly 100 km^3. That part of the shelf which fed the current into this gorge has an area of some 1000 km^2. It is not probable, that the shelf was aggraded as much as the lowering of sea level, for the coarser constituents sank to the bottom before they were carried off the shelf. Thus there was gradually formed a protective covering of pebbles and sand. The amount of sediment washed off this surface during the ice age, can be estimated at 36 km^3, assuming a degradation of three dozen meters. Half this amount may have been lost by running down the smaller furrows on the slope and during storms that were too ineffective to set up a current along the bottom. There remains an amount of 18 km^3. We arrive at the conclusion that out of the gorge 6 times as much has to be eroded as was available for setting up the current.

It may well be that an even larger percentage has to be eroded out of the gorge. On the other hand part of this load is rolled along the bottom.

The carrying power of a stream is greatly strengthened by increase of the velocity. There is nothing against assuming that a flow once gaining a speed great enough to become turbulent and to stir up sediment, will increase its velocity and ultimately take up 6 times the amount of sediment it started with. Its velocity would have risen to more than twice the original by this process.

The process would take the following course. At first the sand is only lifted a few dm from the bottom, but the mud will rise higher and have a greater influence in increasing the velocity. Not until the flow is running at 1 m/sec will the sand be lifted high enough to effect the density of the flow as a whole. It will now also be obvious why the dilution by turbulance at the surface of the flow is not a drawback to the theory. The sediment lifted on the way down will be ample to

[1]) For the case of Georges Bank see: SHEPARD, TREFETHEN and COHEE: Origin of Georges Bank, fig. 5, Bull. Geol. Soc. America, Vol. 45, 1934, pp. 281—302.

restock the current and the flow will be able to continue for the dozens of kilometers it has to traverse before reaching the end of the gorge.

Each time as much as 2½ millimeters of sediment was stirred up on the shelf the flow began and ran down the gorge. As according to our estimate 18 meters were available, there could have been 7000 flows in all. In consequence of very exceptional storms a much larger quantity of sediment was drawn down the gorge, so that there would have been less flows, say 5.000.

On the other hand some of the sediment settled again, when the waves had died down and before it had reached the edge of the shelf. Let us assume therefore that there were 10.000 flows in all.

It is obvious that each successive ice age found less fine sediments to stir up than its predecessor. If there were 200.000 years of lowered ocean level, there cannot have been more than 50.000 years during which there were occasional suspension flows.

Combining our two results we find that once in every 5 years sufficient mud was suspended to cause a flow that eroded the gorge. Probably during the second quarter of the first ice age, when there was still abundant sediment and the waters were sinking rapidly (about 1 cm per year) and already the base level had decended 40 meters, there was an effective flow almost every year, or possibly even oftener.

The erosional work may next be compared to that performed by rivers. Each of our 10.000 flows had to carry away $^1/_{100}$ km^3 of sediment from the gorge, the swifter ones more, the slower ones less. This amount is reasonable, considering the Mississippi carries annually $^1/_5$ km^3 of sediment to the Gulf of Mexico.

Finally one can compare the amount of sediment held in suspension by rivers and by the density current. The ratio of sediment to water by weight in larger rivers is on an average 1:2000, the average velocity 1.50 m/sec. Some rivers, however, contain as much as 1 part of sediment in 300 parts of water (Po, Rio Grande), the Durance even 1 in 30. In the supposed density flows the ratio was 1:3500 at the start and 1:600 towards the end, the velocities being from 0.40 m/sec to 1 m/sec. The relation is probably such, that during severe storms, when the initial velocity was greater, there was relatively more and coarser sediment stirred up on the way down and only fine sediment and less in the cases on which our calculation was based. In the case of the submarine gorges the transportation is aided by the much steeper slope down which the materials were to be carried.

It is hard to estimate the depth of the flow in the gorge. In the experiments the depth was not measured, but from photographs it is believed to be of the order of 1 to 2 cm when 1 liter of suspension was used. On the same ratio the depth in nature would be 100 m. The cross-section of the flow is then 60.000 m^2. As 17 km^3 of suspension is to flow off the shelf, the duration of the current would be more than a week.

It is obvious that after the storm dropped the mud would have settled again long before the suspension had all flowed off the shelf. In fact the flow did not last much longer than the agitation of the water. If this lasted a few days, half the sediment settled again on the shelf. For this reason 10.000 flows in stead of 5000 were assumed above.

The amount of sediment assumed to disappear at each flow was $^{1}/_{100}$ km^3, spread out over the area „wetted'' by the density flow (50 km^2) this represents a thickness of $^{1}/_{5}$ m. In other words a rush of water during a few days at a speed of one or two meters per second has to erode the bottom 20 cm.

The size and shape of the gorges shows that just as in a normal valley only the very bottom part was touched by the current (in our calculation about $^{1}/_{10}$ of the cross-section). If there were only erosion to cut out the gorges they would show almost vertical sides. As this is hardly ever the case, some process must have opened them out continuously while the current cut downwards. Weathering and creep, the agents fullfilling this task in a river valley are absent on the sea floor. It must therefore have been the process of slumping. This consideration is of the utmost importance. It shows that at least 90 % of the materials carried away by the current were not worn off the bottom, but had previously slid down the slopes and lay waiting for the current in a loose heap along the bottom. This would enormously facilitate the transportation and explains the high ratio of sediment in the flow.

Each flow had only to scrape a few cm of solid rock off the bottom. It also shows that the solid rock is so weak as to slump when the slope exceeds 1 in 6 (the slope of the walls) although supported by the water. SHEPARD has brought evidence to show, that slumping has been repeated since the currents ceased to operate and clean out the gorges. In the chart of Georges bank some scars in the slopes may be seen that are best explained as old land slides. By dredging STETSON found a few harder layers. These would crack off when undercut or „underslumped'' and be broken up. They could not have shielded the slopes from erosion except where occurring in very massive layers.

SHEPARD's own hypothesis holds that the gorges were made during enormously lowered glacial sea levels, in other words in a few ten thousands of years. Weathering and creep could not have performed much in so short a period. In this theory also, direct crumbling under the influence of gravity must be assumed to explain the wide-open shape of the canyons. Both theories can therefore only be applied if the materials are very weak. But if this assumption is once made, surely the suspension current hypothesis is to be preferred.

The foregoing calculations are in part fairly accurate, but several elements had to be introduced of which the reader may well feel that the estimates are arbitrary. It is by no means intended to claim any degree of accuracy for the resulting figures, even for the canyon considered. On the other hand the theory would loose much of its appeal if the quantitative element had been left entirely unconsidered. The fact that the estimates lead to quite reasonable results and no flat contradiction is encountered between available energy and work to be performed, may certainly help to allay fear that the density current hypothesis is unable to account for the size of the submarine canyons.

It has to be admitted, however, that some of the Californian canyons arise where the shelf is narrow and where the supply of suspension can hardly have been adequate for eroding large gorges. Are these canyons older and were they merely cleaned out by the flows? This question must be left unanswered for the time being although its importance is not denied.

If further proof is forthcoming to show that continental slopes are formed of loosly scattered or only slightly consolidated materials, there appears to be every reason for crediting glacial density currents with the formation of most submarine canyons, especially those of the Georges Bank-type, and the reopening of older, smothered ones.

During the last glacial stage there were probably no flows with eroding power, because ocean-level sank less and most of the finer material had already been carried away. It is therefore not surprising that clay was deposited in some canyons already during the ice-age. This may explain the occurrence of pleistocene clay in the Hudson gorge, found by STETSON.

In conclusion another sentence may be quoted from STETSON's letter, as it is of great importance to oceanographical geology:

> „Even if we cannot use these currents for cutting gorges, it „seems to me that we have a very important mechanism which can „be used for distributing sediment down the Continental Slope and „out to the ocean basins."

IV. SUMMARY.

SHEPARD has made us familiar with the shape and various properties of submarine gorges and in a number of publications he has shown the great importance to earth-science of these wonderful features of the sea floor.

Various hypotheses have been brought forward to explain the formation of submarine canyons. The only hypothesis to which there are no grave objections, is that suggested by DALY. During the low sea levels of the ice age mud was stirred up on the shallowing shelves and the dense water ensuing, flowed down the continental slopes, gradually eroding the gorges. This theory can be substantiated by experiments. These proved that a suspension will flow down a slope without being much diluted by mixing, that such a flow will concentrate in slight depressions in the flat and follow a rut down the slope, that the current will begin to form ripple marks in sand at a velocity of 10—20 cm/sec and that the speed varies with the root of the density and probably also roughly with the root of the depth.

Rough estimations, based on the data of one of the Georges Bank canyons, show that the velocity in nature should be somewhat less than that of larger rivers, but encreasing as it proceeds and catches up more sediment. At the lower end the suspension has gained 6 times the (effective) density and two to three times the velocity it started with. At this stage the amount of sediment has caught up the average of most larger rivers and reached that of the most muddy major rivers. Some 10.000 flows must have occurred, or one every 5 years, each eroding as much sediment as the Mississippi carries to sea in two or three weeks. The current lasted many hours and its discharge was several times the average of the Mississippi. The relatively large percentage of materials carried by the currents is a consequence of its having slumped down the sides of the gorge before the current picked it up.

However rough these figures may be, they show that no preposterous assumptions have to be made to arrive at reasonable relations between velocities, amounts of water and sediment and size of the gorges for Georges Bank.

Where the shelf is narrow, as at some of the Californian canyons, there appears to have been hardly enough sediment to set up the necessary currents.

The chief remaining uncertainty lies in the necessity of assuming small strength of the rocks forming the continental slopes. But all evidence available except some of SHEPARD's dredging results, points to the same conclusion. Older, smothered gorges may have been cleaned out by this mechanism.

In any case, as STETSON points out, the mechanism must have played an important part in the sedimentation on and beyond the continental slope.

Groningen, March 1937.

An 8-mm film illustrating the experiments, of about 20 meters length, can be obtained at the price of 8 guilders from the Rijksmuseum van Geologie at Leyden.