that little, if any, alteration in design has been necessary since the work was commenced in 1948.

ACKNOWLEDGEMENTS

The Author wishes to thank Mr V. A. M. Robertson, C.B.E., M.C., M.I.C.E., formerly Chief Civil Engineer, Southern Region, British Railways, for the help and guidance given in the earlier stage of the work, and Mr F. E. Campion, M.I.C.E., the present Chief Civil Engineer, Southern Region, for the opportunity of presenting this Paper.

The Author acknowledges the co-operation and help of Messrs A. H. Toms, B.Sc.(Eng.), A.M.I.C.E., and A. M. Muir Wood, M.A., A.M.I.C.E. He would also like to express his appreciation for the help of Messrs E. J. C. Reed, M.B.E., A.M.I.C.E., and K. W. Adams, D.F.C., Resident Engineer on the site, and all members of the Folkestone Warren staff.

The Paper, which was received on the 1st December, 1954, is accompanied by five photographs and four sheets of drawings, from some of which the half-tone page plates, and folding Plates 3 and 4 have been prepared.

Discussion

Mr V. A. M. Robertson said he greatly appreciated both Papers, for several reasons. In the first place, his introduction to Folkestone Warren had taken place at the end of the last century, when he was a boy at Dover College and in 1902 he had been accustomed to cycle there from Dover. He had not dreamt at that time that, not many years afterwards, he would be a young engineer on the South Eastern & Chatham Railway. In 1912 however, he with others had been employed on a survey of the Martello and Abbotscliffe tunnels, and in 1916, a year after the 1915 landslip, when on leave from France in world war I he had been permitted to visit the site and see what had been shown that evening on the Authors' slides. In 1943 he became Chief Engineer of the Southern Railway and responsible for the safety of the railway through the Warren. It would be understood, therefore, that he had a particular interest in the two Papers.

The Papers would form an interesting record because they dealt with a subject which came only very infrequently before the Railway Engineering Division of the Institution. By 1948 it had become quite obvious that a geological investigation at the Warren was long overdue. When Mr Toms had had to relinquish his work at the outbreak of the war in 1939, Mr Muir Wood, the Author of the first Paper, had taken over where Mr

Toms left off, and would be the first to say how much he appreciated what Mr Toms had done and how much he had valued Mr Toms's help throughout that period.

Mr Muir Wood had had a very definite remit, and had given in his Paper an idea of what he had been able to do in a comparatively short time. He had been on the site night and day for 2 years. The results of his work and what he had learnt had been embodied in the first Paper. Mr Robertson would like to ask him two questions ; first, had he thought about what the future held in store for the Warren ; and secondly, were the slips at an end ? Was there going to be any necessity for further investigation of the type described in his Paper ? If Mr Muir Wood could not answer the second question, perhaps Mr Viner-Brady would be in a position to do so. What was being done now to find out what was still happening ? Was any movement known to be taking place ? It was stated that levels and measurements were still being taken, but could the engineers now in charge with any sense of security feel that there was nothing happening which could not be seen ? That was the crux of the matter.

Referring to Mr Viner-Brady's Paper, it was interesting that a job of the nature in question, which, as Mr Robertson had said earlier, was unusual in railway experience, should have been carried out entirely by direct labour. Mr Viner-Brady had talked to him about it on many occasions. When the question had arisen of whether or not to bring in contractors, Mr Viner-Brady had said that he would like to do the job himself if allowed to do so. He had been allowed to do it, and had made a success of it.

The alternative to what had been done would have been to have kept in existence the land powers to build a new railway, which would have cost as much as £3¼ million 3 or 4 years ago. The Southern Railway had decided to drop those powers in 1953 and must therefore have had complete confidence in what was being done to maintain the railway between Dover and Folkestone. Did that confidence still exist today in the engineer responsible for all those works ? That the works had been regarded as of extreme importance was shown by the fact that the Southern Railway had found it advisable to call in two Past Presidents of the Institution, Sir William Halcrow and the late Sir David Anderson, a Member of the Council of the Institution, Sir Claude Inglis, and a well-known Member in Belfast, Professor Naylor, to make sure that what was being done was right. Was it going to be all right ? Perhaps Mr Viner-Brady would say.

Professor A. H. Naylor described the model investigations which had been started after the 1948 sea wall had been designed and was under construction. The questions to be answered in order of urgency were :—

(1) The design of the covering slab protecting the chalk fill.
(2) The minimizing of erosion below the toe of the wall.
(3) The determination of an optimum design, suitable for future extensions.

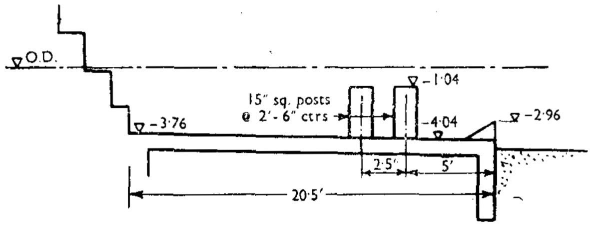

FIG. 35.—APRON TEST No. 17

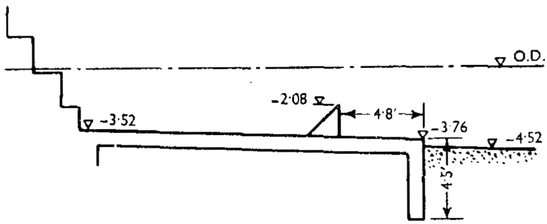

FIG. 36.—APRON TEST No. 6

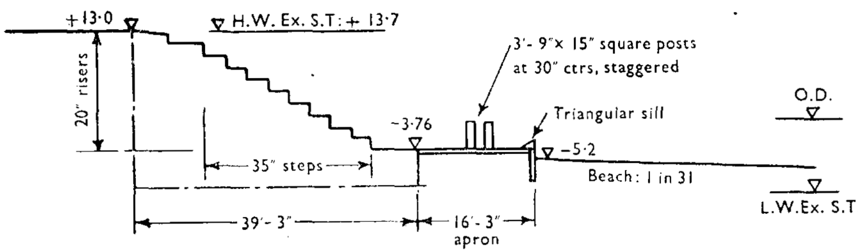

FIG. 37.—HALCROW WALL No. 2

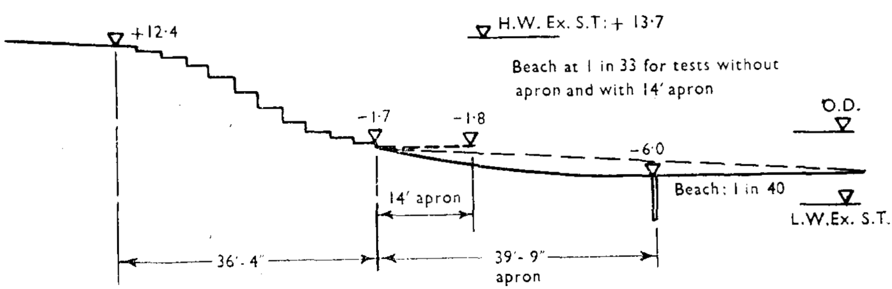

FIG. 38.—HALCROW WALL No. 1

The top covering was assumed to be terminated 50 feet back from the wall. Fig. 29* showed a plain apron with a 4-foot-high end wall and Fig. 30 showed it to be inadequate to prevent overspill. Figs 31, 32, and 33 showed the effect of three 2-foot-square baffle walls. There was but little overspill.

Fig. 34 showed a plain apron with a 5-foot wall. An improvement was to cut out the curved fillet at the base of the wall. Further improvements were effected by bringing the wall forward and by introducing baffles consisting of rows of cubes.

In all cases, however, it had been concluded that to provide the necessary robustness to withstand the high impact forces would be uneconomic and that it would be better to let the waves expend their energy on a plain apron covering the whole fill.

Toe protection was clearly necessary. The envelopes of erosion profiles had been obtained after tidal cycles without and with various forms of toe apron, using sand. The results were of relative value only. The actual foreshore material—fissured gault clay—could not be represented to scale. The sand results had, however, been checked against a specially prepared clay. A number of experiments were made with various toe aprons. The best was T 17 (Fig. 35) with a 20-foot apron, a double row of posts, and a triangular sill. However, T 6 (Fig. 36) with a 20-foot apron and a triangular sill set back from the edge had been adopted for it was almost as good and much simpler and cheaper.

Further experiments were made to discover the optimum wall design. The best proved to be that known as " Halcrow No. 2 " (Fig. 37) with a toe protection similar to the optimum of the previous tests. Why was that design not adopted for the subsequent work ?

Finally, Professor Naylor asked why the line of the 1951 wall was set back behind the 1948 wall.

Mr D. H. B. Reynolds observed that the Folkestone Waterworks Company had kept rain-gauges at the Cherry Garden, 2 miles north-west of the Warren, since 1868, and at Lower Standen, 2 miles north of the Warren, since 1898. The rainfall in October 1939, had been very nearly double the highest ever recorded for a month in those preceding years, and had been followed by rainfall in November which had also been nearly up to the record, so that the record for those 2 months had been entirely without precedent. There followed the slip of 1940. There had been no rainfall of such intensity to account for the slips of 1877 and 1915, although both had taken place in very wet periods.

Mr Muir Wood had stated (p. 421) that " whether the bulk of the water percolates through fissures in the base of the chalk cliffs has proved a debatable issue." All Mr Reynolds's researches had indicated that no water percolated through the lowest 130 feet or so of the Lower Chalk,

* Figs 29–34 are between pp. 450 and 451.

which appeared to be completely impervious; no water seemed to move in it, except where there were fissures associated with faults.

Mr Muir Wood had also referred (p. 422) to the evaporation which took place and to the rainfall of 1947 and 1948, and had said that no more than 4 to 5 inches could have penetrated to the drainage headings. Mr Reynolds thought that that was an under-estimate. They had an evaporation gauge, but evaporation took place from the gauge even though there had been no rainfall, so that if the summer was a very dry one and the year's rainfall occurred in the winter, the evaporation taking place mainly in summer, there would be a larger proportion of the rainfall percolating.

On p. 422 there was a reference to the marked disparity between the rainfall in 1935, 1936, and 1937 on the one hand and 1947 and 1948 on the other, the first being a very wet period and the second a very dry one, and it was pointed out that the standing level in a borehole had been much the same. That, Mr Reynolds thought, showed that the water had been getting away over some barrier.

The mention of the springs near Steady Hole (p. 424) was of interest. The Author gave the level as 173 O.D. That corresponded almost exactly to the bands of impervious clay or marl which occurred just below the middle of the Lower Chalk formation and threw out all the springs which were seen on the southern escarpment, both on the cliffs of the Warren to the east and also on the downs westward from Folkestone. Those bands seemed to be the lowest level at which any water was found in the chalk.

Mr Reynolds was also interested in the suggestion that the ideal drainage gallery might be a heading set at a suitable elevation along the face of the cliff. Such a heading would undoubtedly intercept some water and take it away, and it would be all to the good, because he was sure that that water which seeped out of the cliff served to lubricate the Gault of the slip planes. He thought that the water in the Warren did two quite different things. It acted as a lubricant to keep the sliding surface ready to slide, and it also acted as a powerful hydraulic jack to set the mass in motion. Mr Reynolds had already referred to that subject[8] and suggested that there was probably a large fissure bringing water into the Warren from the west-north-west. It was probably the water in the fissure or group of fissures which had the jacking effect which set the slips in motion. That could not be proved, but he would give a few of the reasons which led him to take that view.

Before doing so, however, he commented on the recent heading which had been driven and which intercepted only 2,000 to 8,000 gallons per hour. He felt sure that there was more water to be found. The heading

[8] A. H. Toms, "Folkestone Warren Landslips: Researches carried out in 1939 by the Southern Railway Company." Railway Paper No. 19. Instn Civ. Engrs, 1946. *Discussion by* D. H. B. Reynolds, p. 37.

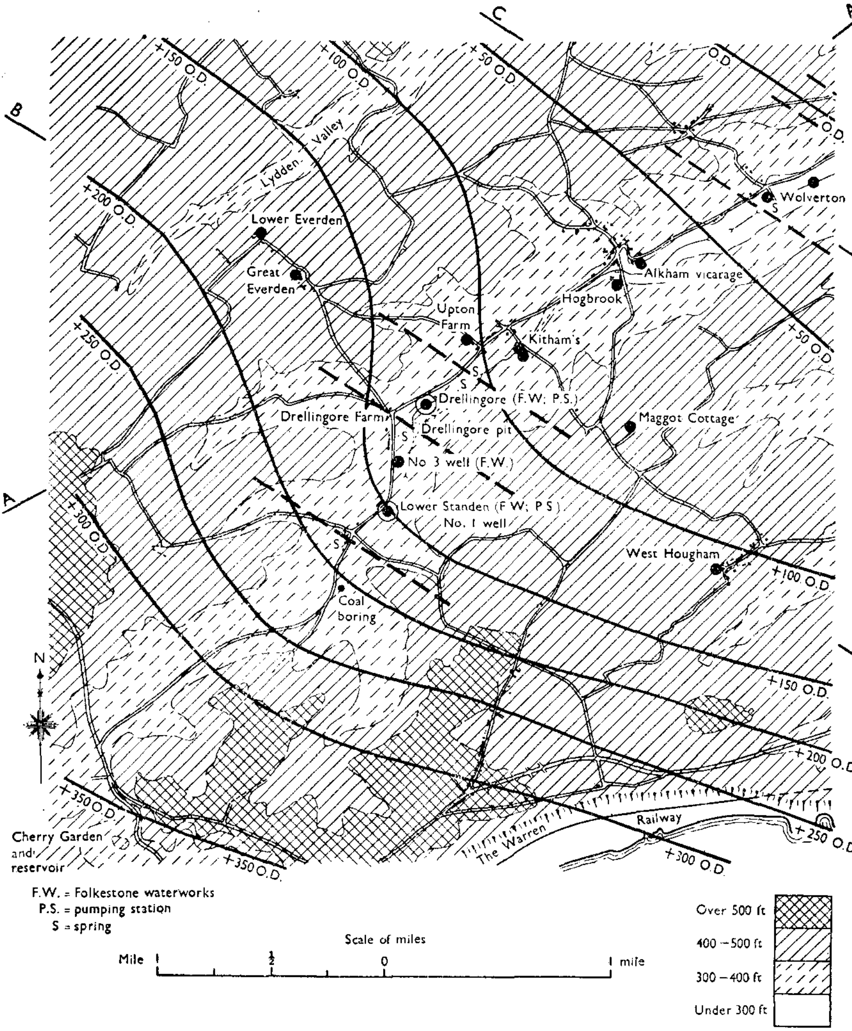

FIG. 39.—PORTION OF A MAP DEALING WITH UNDERGROUND WATER IN THE DOUR WATERSHED

had been driven in about the right place, but from its level it was presumably down in the Gault; and, since he had already said that the water was held up at a higher level, it now remained to find the water and conduct it to the heading.

Fig. 39 showed part of a map of the River Dour catchment (situated immediately to the north-north-east of the Warren) which had been prepared in connexion with a waterworks survey. The full heavy lines running in the general direction north-west to south-east were contours of the base of the Middle Chalk formation (the top of the Plenus Marl). They showed a general dip to the north-east, but nearer the Warren it was to the north-north-east, and in dry conditions when water was not plentiful that was the direction in which water tended to move ; but it did not travel at the low levels, because the Chalk was almost completely impervious in its lower parts. Geologists had divided the Chalk into Upper, Middle, and Lower, but from the waterworks point of view the effective division was half-way down the Middle Chalk. Above that there was a great deal of percolation, but below half-way down the middle chalk it was almost completely impervious and the movement of water was confined to the fissures.

The fissures were of two kinds : those associated with faults and those which followed the bedding planes. In the absence of faulting, the fissures at the bedding plane did not seem to be very wide open. The heavy broken lines on the map were the supposed positions of important faults. There was insufficient evidence to plot them very far, but they had been identified in the adits at Drellingore and at wells Nos 1 and 3 at Lower Standen, and Mr Forster Brown had identified them in the coalfields as having the same general direction. There seemed good reason to believe that the water which got into the Chalk followed the dip until it reached one or other of the faults, at which the fissures were of greater size than anywhere else, and they conducted the water in a south-easterly direction towards the coast-line. In wet seasons the fissures were saturated and the water then headed up to the upper part of the Middle Chalk and higher, and could then travel north-eastwards with the dip.

In an adit at Drellingore the Lower Chalk not far below the Plenus Marl was absolutely firm and solid, with hardly a crack and quite impervious. Mr Reynolds had been along such adits many times towards the end of a dry period, when heavy rain was causing the yield of the well to rise above the capacity of the pumps, but the adit roofs had always been dry. The chalk was quite non-porous and the water moved entirely through the fissures associated with the faults and the bedding planes near them.

Fig. 40a was a section of the area shown in Fig. 39, running from south-west to north-east, and the sections shown in Figs 40b and 40c were at right angles to it. Of the stratum lines shown in Figs 40a, b, and c, the lowest represented the series of clay bands which formed the cast bed half way down the Lower Chalk and below which no water was found. They were responsible for all the springs along the southern escarpment. and they formed the effective floor of the open reservoirs at the Cherry Garden, which were not watertight by reason of their concrete linings. Fig. 40a showed a pronounced dip to the north-east, and the water tended to travel away from the Warren ; it was only when there was intense rainfall that there was enough of it to make it pond up and go uphill against the dip of the strata and come out of the escarpment either at Steady Hole, where it

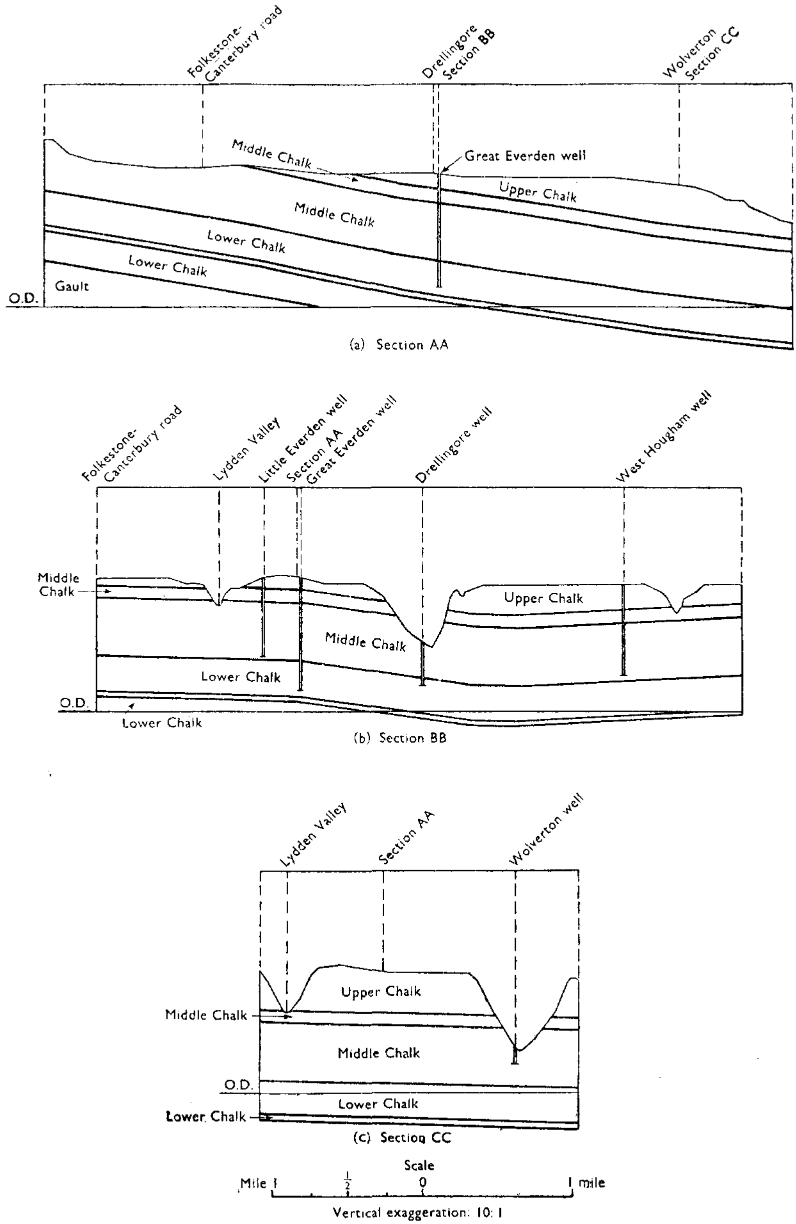

Fig. 40.—Sections from Fig. 39

could be seen, or farther west where it was hidden by the talus. No doubt some came out there after heavy rain, but not normally.

In the course of his survey of the wells in that area Mr Reynolds had been able to differentiate between wells of different depths and also wells near the main fissures and faults and he found that in the wells near the main fissures and faults the water might rise in a few weeks by more than 100 feet. In the wells which were not associated with large fissures the rise was less than half of that amount in similar conditions. He felt sure that the fault which approached the Warren from the west-north-west, and about which Mr Muir Wood might be able to give some more information, was bringing water towards the Warren. Mr Reynolds wished he could say how much, but at any rate he was sure that the water which it brought was liable to increase to an unpleasant extent in a very wet period in the future.

A tunnel was now being driven for the Folkestone Waterworks Company through the line of that fault near their reservoirs at the Cherry Garden. That tunnel had gone in 300 feet already. It was in the upper part of the Lower Chalk, not far below the Plenus Marl, and was in exactly the same sort of chalk as that at Drellingore which was non-porous and was normally firm, but in the tunnel it fell away in 6-inch lumps and had to be close-supported. He felt sure that that was a very important line of faulting, probably as big as the one farther north-east which was responsible for the valley of the River Dour and had made a great line of weakness there. He wished it were possible to locate the water in those fissures for the mutual benefit of the railway and the waterworks so that the water could be kept away from the Warren and turned to a useful purpose.

Mr A. H. Toms observed that Mr Wood's Paper confirmed the fact that problems of such a character could not be resolved by the application of soil mechanics methods alone. A study of the fossil distribution in the Gault had proved essential and, in that connexion, the collaboration of the Geological Survey, and in particular of Mr Casey, had been invaluable. It was certain that both Mr Casey and Mr Wood had profited greatly by their discussions, to the benefit of both the civil engineering and geological professions.

Mr Toms showed lantern slides which illustrated the good shingle beach which had been maintained artificially before the 1939–45 war, throughout the Warren, by constant tipping of shingle off the western end of the sea wall. About 30,000 cubic yards per year were imported from Dungeness for the purpose. Further lantern slides indicated the very serious denudation of beach and erosion of the foreshore during and since the war, arising from a combination of causes. Among them were a reduction of groyne maintenance during the war, the cessation of shingle importation from Dungeness, damage and severe erosion by storms, and the constant eastward drift of the "beach."

In the vicinity of one buttress to the old sea wall nearer the western end of the Warren there had been a loss of more than 10 feet depth of shingle and Gault or chalk, whilst at Horsehead point a vast accumulation of beach and chalk from the big 1915 fall had disappeared entirely, the loss in depth being about 20 feet.

That rapid erosion in the neighbourhood of Horsehead was in itself more than ample justification for the protection works which had been put in hand in that region. Weighting and protection of the foreshore was probably a more positive means of providing extra stability to the slips than drainage tunnels, the precise effects of which could not be forecast in such a heterogeneous soil mass as the Warren ; Mr Toms would like to see foreshore weighting extended from the 1948 work right to the western end of the Warren. That end had always been the worst for slips and, apart from completion of the western end of the sea walls after the 1937 slip, the local lowering of the water in the back of the Warren by the new tunnel heading, some groyne repairs and wall and buttress underpinning and some excavation of the sea side of the railway cutting nothing had been done to improve the stability of that zone. The above-mentioned excavation would benefit a potential slip up through the railway but would not improve the overall stability of that end of the slips as a whole.

However, in spite of those remarks, Mr Toms was strongly in support of a policy of drainage of the back of the Warren, even though it might have limitations, as had been proved by the rather localized water-lowering achieved by the new tunnel heading (20 feet lowering over a restricted area near the landward end of the heading).

Lantern slides were shown by Mr Toms of the very large fall of high cliff which occurred near the western end of the Warren in 1940 and which was accompanied by cracks and slight settlement of a further slice of the high cliff at the rear of the fall. Mr Toms thought that those slight settlements, which had not developed since, might well be associated with the small " throws " discovered, by the 1948–50 research, in the Gault at depths below and behind the main slip surface.

Fig. 41 was a distorted longitudinal section of the new tunnel heading, showing the Gault barrier passed through before entering the Chalk. Relatively little water had been encountered until the loam pocket was struck, when there had been an uncontrollable " run-in " which delayed the work for about 3 weeks. Thereafter the heading passed through broken chalk until it struck the 2-to-3-inch thick remoulded slip surface at the Chalk Gault contact face. Fig. 42 showed the strength of undisturbed samples prepared from large pieces of the Gault taken from the slip surface and at various distances behind it up to the termination of the shield chamber. The samples had been cut out by hand, not with a sampler tube, and carefully waxed up to prevent any loss of moisture before preparation for testing.

It would be observed that whereas, in the thin slip surface, the Gault

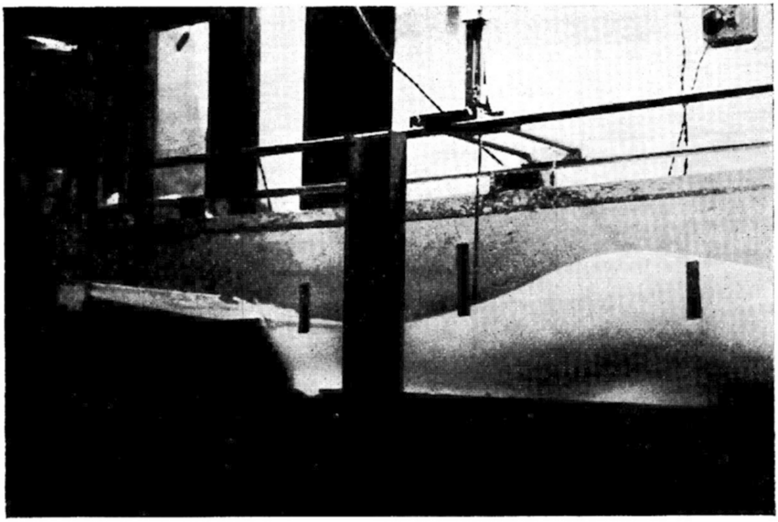

Fig. 29

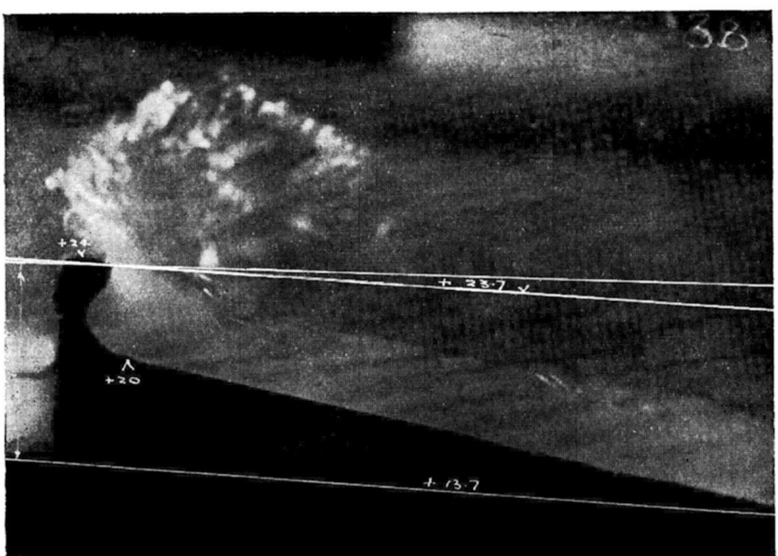

Fig. 30

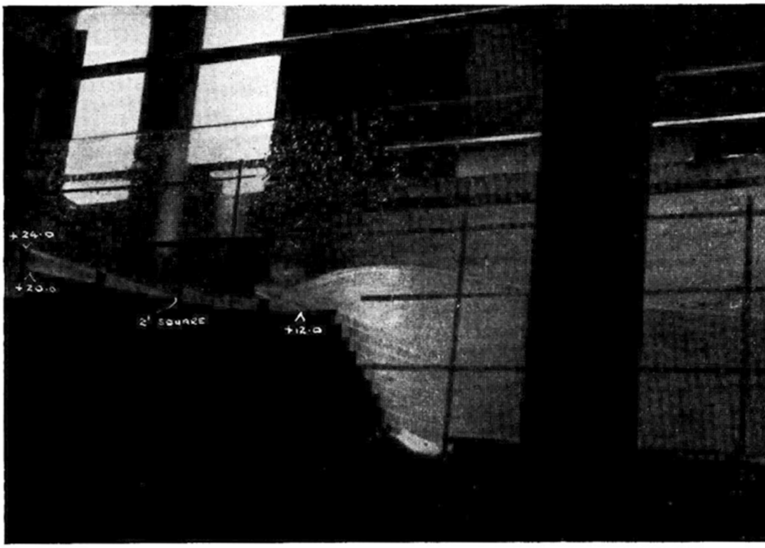

FIG. 31

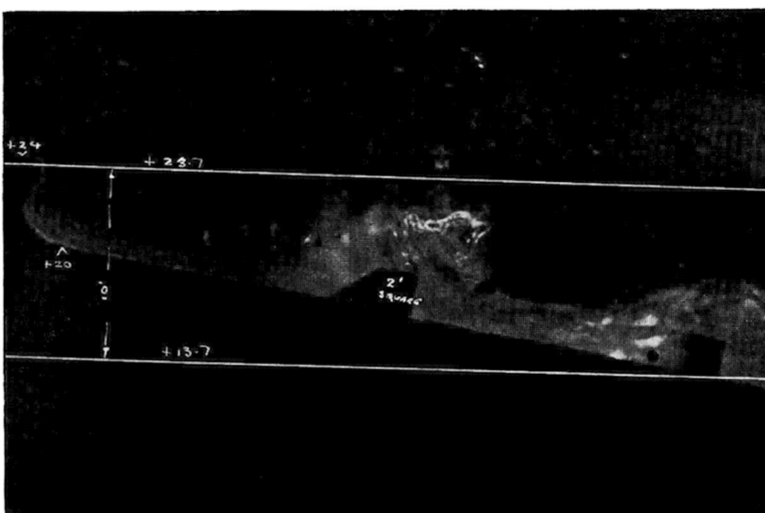

FIG. 32

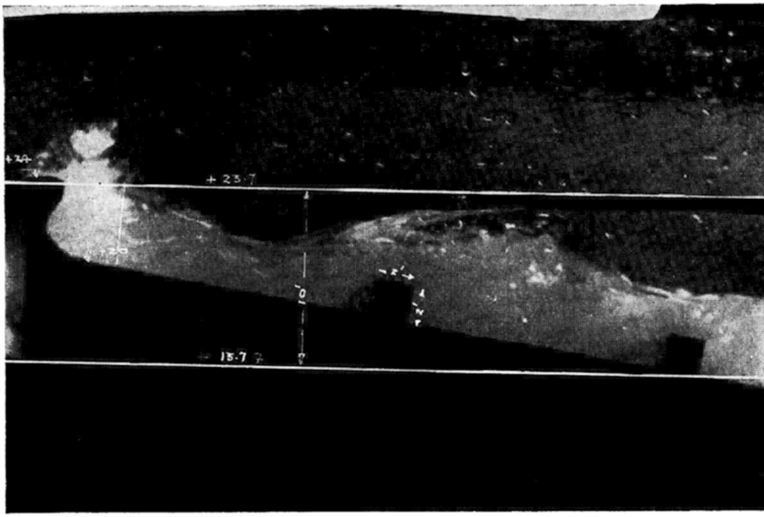

Fig. 33

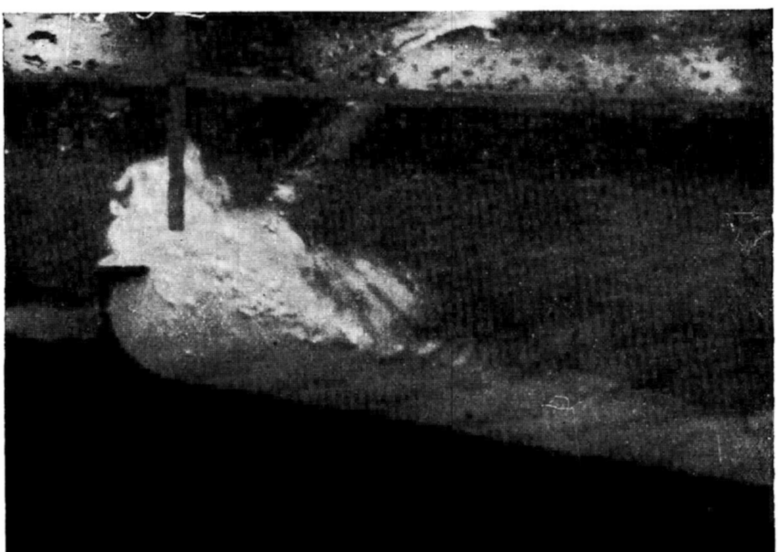

Fig. 34

FIG. 43.—PHOTOGRAPH TAKEN IN 1896 SHOWING GAULT EXTRUDED THROUGH WESTERN FORESHORE

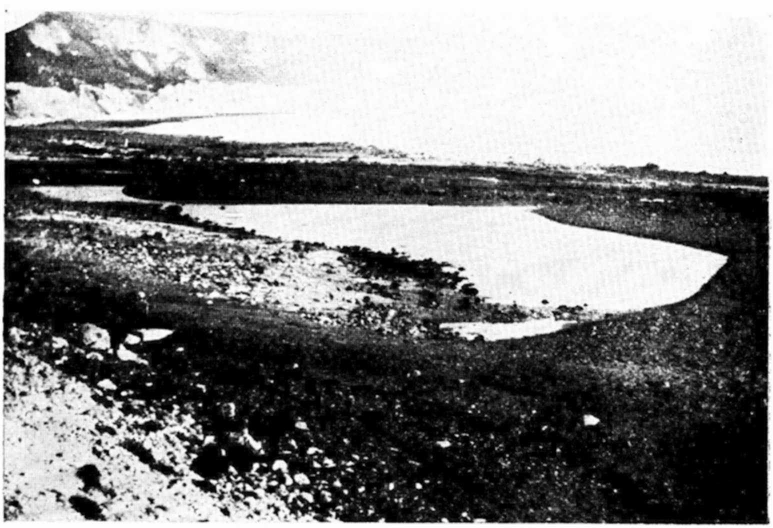

FIG. 44.—LAGOONS FORMED BY EXTRUDED GAULT RIDGES, 1896

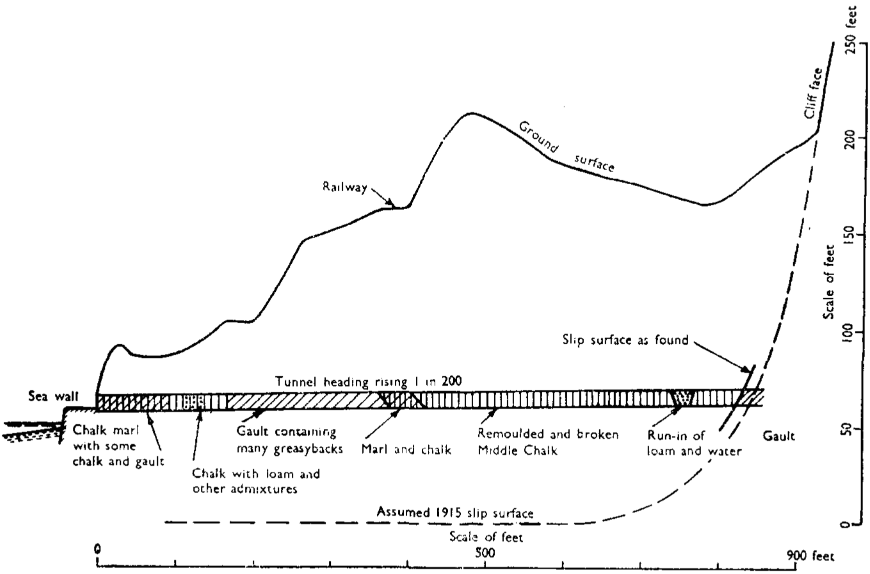

FIG. 41.—DISTORTED SECTION ON LINE OF CIRCULAR TUNNEL HEADING

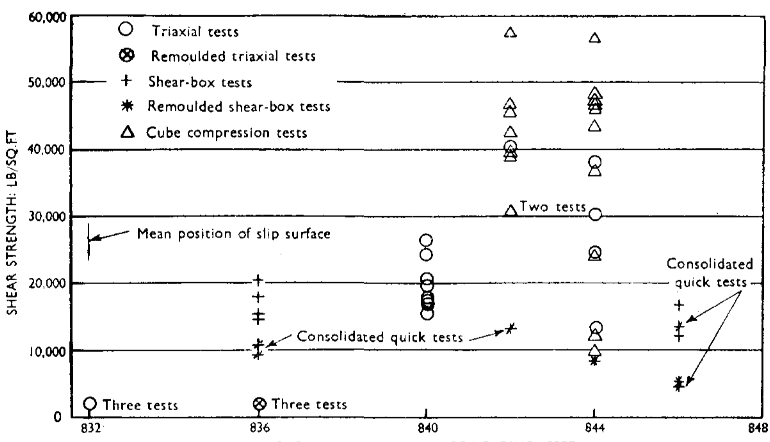

FIG. 42.—FOLKESTONE WARREN NEW HEADING. GAULT SHEAR STRENGTH BEHIND 1915 SLIP SURFACE

TABLE 1.—FOLKESTONE WARREN, NEW TUNNEL HEADING, APRIL 1953

Tests on undisturbed samples of clay obtained from main (1915) slip surface and at various distances behind it

T = triaxial test — R = remoulded test
S = shear-box test — C.Q.S. = consolidated quick shear-box test
C = cube-crushing test

Type of test	Distance back from slip surface: ft	Test conditions: Overburden: lb/sq. in.	Lateral pressure: lb/sq. in.	Rate of strain: % min	Shear strength: lb/sq. ft	Moisture content: %
T	0	—	* 79	1·5	1,950	24·8
T	0	—	* 78	1·5	2,250	21·4
T	0	—	* 75	1·5	1,930	27·5
S	4	100	—	1·5	20,500	21·7
S	4	100	—	1·5	15,450	19·3
S	4	100	—	1·5	18,000	19·1
S	4	100	—	1·5	14,700	18·4
C.Q.S. (1 hr.)	4	100	—	1·5	10,900	20·7
C.Q.S. (overnight)	4	100	—	1·5	9,350	—
R.T.	4	—	20	2	670	(1) 32·2
R.T.	4	—	40	2	930	(1) 31·2
R.T.	4	—	60	2	900	(1) 31·0
R.T.	4	—	95	1·5	2,000	(1) 28·6
R.T.	4	—	90	1·5	1,930	(1) 29·6
R.T.	4	—	97	1·5	2,050	(1) 29·2
T	8	—	100	0·2	19,750	20·0
T	8	—	100	0·2	26,400	20·05
T	8	—	100	0·25	15,900	20·1
T	8	—	100	0·2	17,150	20·5
T	8	—	100	0·2	17,400	20·4
T	8	—	100	0·25	17,900	20·4
T	8	—	100	0·2	20,800	20·2
T	8	—	100	0·25	26,600	19·9
T	8	—	100	0·2	24,200	20·7
T	10	—	100	0·1	40,300	18·0
C.Q.S. (overnight)	10	100	—	1·5	†13,200	(2) 17·2
C	10	—	—	—	46,800	Initial M/C = 18·1
C	10	—	—	—	30,700	,,
C	10	—	—	—	39,000	,,
C	10	—	—	—	39,600	,,
C	10	—	—	—	42,500	,,
C	10	—	—	—	57,600	16·6
C	10	—	—	—	45,700	Initial M/C = 18·7
C	10	—	—	—	30,700	Initial M/C = 18·7
C	12	—	—	—	36,600	10·5
C	12	—	—	—	46,800	15·7
C	12	—	—	—	56,500	17·0

TABLE 1—*continued*

Type of test	Distance back from slip surface: ft	Test conditions: Overburden: lb/sq. in.	Lateral pressure: lb/sq. in.	Rate of strain: % min	Shear strength: lb/sq. ft	Moisture content: %
C	12	—	—	—	47,200	17·8
C	12	—	—	—	24,200	19·2
C	12	—	—	—	9,970	22·1
C	12	—	—	—	12,200	25·2
R.S.	12	200	—	—	8,400	Initial M/C =19·2
T	12	—	98	0·15	13,100	—
T	12	—	98	0·026	24,150	19·6
T	12	—	98	0·033	29,800	19·1
T	12	—	98	0·024	37,500	19·2
T	12	—	100	0·2	24,700	19·3
C	12	—	—	—	46,000	Initial
C	12	—	—	—	43,400	M/C =19·2
C	12	—	—	—	47,850	,,
S	14	100	—	1·5	12,200	19·0
‖C.Q.S. 1	14	100	—	1·5	4,750	24·4
*C.Q.S. 2	14	100	—	1·5	13,600	22·8
S	14	100	—	1·5	16,700	19·4
‖C.Q.S. 3	14	100	—	1·5	5,400	25·3

* Limit attainable with compressor then in use.

(1) These tests, on clay remoulded at higher than natural moisture content, were performed to ascertain ϕ. Result confused by variation of moisture content but ϕ apparently small.

(2) Shear-box sample took up water on shear surfaces on shearing.

† "Immediate" strength exceeded 27,000 lb/ft^2

‡ These results are not shown on diagram from which Fig. 43 was made.

‖ Initial moisture content = 20·4% { over week-end . . 1; $1\frac{2}{3}$ hour . . . . 2; overnight . . . 3 }

The overburden at the slip surface in the new heading is approximately 110 ft. Assuming the weight of soil to be 130 lb/cu. ft, that was equivalent to a pressure of 100 lb/sq. in.

All tests carried out at natural moisture content (approx.) except where otherwise shown.

strength was below the calculated mean value required to keep the main slip in equilibrium, the strength as measured in normal tests rose very rapidly as one moved away from the slip zone, to values which were very much in excess of those required for equilibrium, even though there did appear to be a softer zone at a distance of about 14 feet behind the slip surface.

At present it was not at all clear how clay, considered to be far too stiff to permit shear failure under the static stress system existing at points behind the known slip surface, could in fact give way and allow the shearing of further slices of the high cliff and the Gault deep down below ground surface.

Mr Toms then showed lantern slides of stages in the wave-tank model experiments conducted by Professor Naylor at Queen's University, Belfast. Scour was shown to occur in front of both the " Halcrow " and " Southern Railway " wall profiles without protective aprons when subjected to a full tide (+11·3 O.D.) cycle test with maximum estimated waves (10 feet high). It had been decided to adopt the S.R. profile for simplicity in construction and to protect the foreshore in front of it with the apron determined by Professor Naylor's experiments.

Mr G. Bisson speaking on behalf of **Mr F. H. Edmunds,** of the Geological Survey, who was unfortunately prevented by illness from being present, said that the opportunity of co-operating in the investigation of the Warren landslips had been very welcome, and it was hoped that the officers of the Geological Survey would be able to continue to co-operate in future work.

Whereas in many civil engineering site investigations the most important geological considerations were the rock types geological structure and water problems, in the present case the most important consideration had been the identification of the horizons of the Gault based on fossils. Mr Muir Wood had perhaps been fortunate in being able to call on the services of Mr Casey, who was an authority on the palaeontology of the Gault.

Mr J. A. Lewis referred to the artificial beach which had been maintained by constant tipping. He had carried out some experiments on a small scale in connexion with a similar problem and had found that a very great improvement could be effected by having the groynes at the right height in relation to high-water mark and the level of the beach. Given a certain position of high-water mark there seemed to be a corresponding position and level to which the beach would build up, depending upon the waves experienced on that coast and upon the size of the material. If the groynes were carried high enough, so as to stop the movement at that level, it was possible to retain the material to a large extent. The excellent beach which existed at the Warren in 1939, and which apparently had had few or no groynes to maintain it, apart from the buttresses against the wall, would probably have been there still if there had been adequate groynes, sufficiently closely spaced and constructed to the correct height.

Mr Robertson had asked what conclusions had been reached about the future course of erosion and the stability of the works. Mr Lewis had noticed that the London Clay on the north coast of Kent seemed to wear down to a platform, and when a depth of one fathom had been reached no erosion seemed to take place for a long time. The Gault might not behave in the same way, but it might be found that when a depth of 1 to 2 fathoms had been attained there would be a great slowing up in the rate of erosion and it would be reasonable to predict that the level of the Gault seaward of that depth would remain reasonably stable. Was any information available on that matter ?

Professor Naylor had already dealt with the design of the wall profile, but it would have been interesting to see the effect of making the bullnose type of wall somewhat bigger. Professor Naylor had mentioned a height of 4 feet, but Mr Lewis wondered if a wall twice that height would have been more successful in throwing back the waves. It had been stated that a vertical face at the bottom was better than a curved face, and Mr Viner-Brady had referred to experiments which had been carried out where one part of the sloping apron at the top of the wall had random rocks embedded in it, and another part was left smooth but the wall behind had a bullnose coping. Could he give more information about the effect of that ? Mr Lewis had found that stones embedded in a sloping apron or in the sloping face of a harbour wall had a very marked effect in reducing the height to which the waves ran when they struck it, and he felt that the combination of the two would probably be very successful in reducing the height to which storm-water was thrown on breaking against a solid structure.

Fig. 26, facing p. 435, showed the set-offs in the wall to stop the run of the waves, and it would be interesting to know whether any optimum size and setting had been found for them. Mr Viner-Brady had said that he was going to increase the number and size of them. So far as Mr Lewis knew, very little experimental work of that nature had been done. On the east coast some walls had been constructed with small set-offs to stop the run of the waves, and it had always seemed to him that they tended to be constructed too close together and too small. In conditions when the waves were not very large the small set-off might be adequate to stop the run of the waves, but in that case it did not matter much whether they were stopped or not, because they were not going to do much damage ; but to prevent big storm waves causing damage it was necessary to have bigger set-offs.

The erosion at the set-back seemed to correspond exactly with what occurred elsewhere on the south coast. When there was a set-back on the beach the erosion was more rapid and the beach more difficult to maintain. The east side of Eastbourne provided an example. Mr Viner-Brady's conclusion that it was due to the angle of the waves, had been confirmed by Mr Lewis's recent investigation of some serious erosion which had occurred at Eastbourne in December 1954. He did not know what could be done to effect an improvement in that instance except perhaps the provision of adequate groynes with the correct angle of incidence to the foreshore to minimize scour. Could Mr Viner-Brady give any further information on the question of groyning to be done at Folkestone Warren ?

Mr E. J. C. Reed said that at the Warren there were two slopes in the foreshore : a fairly steep slope up to the wall and a flatter one on the seaward side. They had taken great care that their work should be on the flat slope on the seaward side of the steep one. With regard to the piling, the Diesel pile-driver might appear to be rather a delicate instrument, but it had a kinetic energy of a little more than 4,000 lb., equivalent to a No. 7 McKiernan-Terry. It drove many of the heaviest type of Larssen piles (No. 5) and up to fourteen piles had been driven in one tide. That had been quite sufficient to keep well ahead of the work.

The apron had, of course, been built before the foundation. It had been a little disappointing at first to find that they could not complete a length of apron in one tide ; each day it would be found that all the previous day's excavation had been filled, partly with water, partly with material which had already been excavated, and a lot of seaweed. It was very discouraging for the men to have to clear all that out and skim the damaged Gault before concreting could take place. However, as soon as the navvy bucket was used on each side of the piles, so that the only hand work required was to clear the troughs of the piles and trim the ends, an 8-foot length of apron had been completed in one tide, and that made a great deal of difference to progress. When double-tide work was done it was necessary to choose two sites for the apron, because it was not possible to strip the shuttering and the covering to the apron which had already been done in the morning in order to concrete the one next to it.

With regard to the wall itself, in the 1948 work, as Mr Viner-Brady had already said, the plant experts had doubted whether it would be possible to operate plant on the foreshore. Moreover, to get plant down to the foreshore a ramp would have to be built, occupying time which they could ill afford. However, as the work progressed it became apparent that plant could, in fact, be operated on the foreshore and in the 1951–53 work it had been successfully done.

It was interesting to compare the progress of block-laying under the two conditions. When the crane operated from the top of the wall the progress during one tide had been 18 feet, or three block-lengths ; that was all that the crane could reach with a 28-foot radius, and to build up the seven courses took 9 or 10 days. With the cranes operating on the foreshore a 30-foot length could be completed in one tide. To maintain that rate of progress it had been necessary to lay two or more 30-foot lengths of each course, so that the grouting gang could follow at the next tide on the work that the block-layers had done the tide before. That was very important. The result was that it had been possible to get about 60 feet done in 14 days, so that the progress achieved by operating on the foreshore was a little more than double what could be managed from the top of the wall.

Mr Raymond Casey observed that Folkestone was well known as one of the principal areas of recurring slips in the British Isles, whilst to the geologist it had an even greater claim to fame: it was world-renowned for the beauty and abundance of its Gault fossils. The sequence of fossil-bearing zones in the Folkestone Gault provided a comparison for strata of Gault age throughout Europe.

For a long time, until the engineering works had commenced, there had been more or less constant exposures of Gault clay on the Warren foreshore. Mr Casey believed that that was made possible only by an almost incessant extrusion of Gault from beneath the superincumbent strata. In the late 1920s and throughout the 1930s, when he had been resident at Folkestone, he had had the foreshore reefs in the Warren under very close observation. It had been clear that movement of some sort was going on nearly all the time. A few weeks before the 1937 slip it had been evident that considerable movements of Gault were taking place on the foreshore. Masses of Gault had been squeezed up through the sand—almost like toothpaste out of a tube—to a height of 6 to 8 feet.

Fig. 43, facing p. 451, was reproduced from a photograph taken nearly 60 years ago, shortly after the 1896 slip. It showed a mass of material which had been squeezed up through the western side of the Warren foreshore. The dark patches were Gault amid the lighter chalk. At one point, 20 feet above the beach, was a seaweed-covered block of chalk which had been elevated prior to the slip. The notes accompanying the photograph stated that the slip had been heralded by that upheaval of material on the foreshore, and it might be added that the position of that upheaved mass was very near to that of the mass similarly pushed up just before the 1937 slip.

Fig. 44, facing p. 451, was another view taken after the slip of 1896, showing the usual phenomena of ridges of Gault squeezed up through the sand on the foreshore and the development of long lagoons behind those ridges. The similarity between the effects of the two slips suggested that the slipping path in 1896 and that in 1937 followed the same lines.

Mr D. J. Henkel, dealing with the stability side of the problem at Folkestone Warren, said that Mr Muir Wood, and Mr Toms before him, had published a great deal of information about what had been going on in the Warren, but there were still a number of points which required clarification before it was possible to say that what had happened was really understood and that any reliable quantitative estimate could be made of the factors of safety before the slips occurred and the increases due to the remedial measures which had been taken. Mr Muir Wood had used the

$\phi = 0$ analysis in his calculations of stability, but Mr Henkel did not regard that as a very promising line of approach to the problem, because it was so difficult to take into account the effect of changes in water level, which seemed, from what had been said, to be a very important feature in the Warren.

In Fig. 19, if Mr Henkel understood it correctly, the effect of lowering the water level by 20 feet at the cliff face was to increase rather than decrease the reaction R_5, and it would be interesting if Mr Wood would explain the effect of that on the factor of safety.

He thought that better results would be obtained if the problem was tackled by effective-stress methods, using the cohesion and angle of shear resistance determined from drained shear tests on the Gault clay. Those methods were of advantage in heavily over-consolidated clays, in which stability was essentially a long-term problem. The advantage of that procedure was that it was possible to predict to a reasonable degree of approximation how soft the clay could become. Mr Toms had had difficulties with his undrained tests on the clay. One found a narrow failure zone which might be only 2 inches thick ; the slip surface was very soft, whilst adjacent to it the undisturbed clay had enormous strengths of 300 to 400 lb. per square inch. It was very difficult to use a $\phi = 0$ analysis with that sort of material.

He did not think that the Author's explanation of, or reason for, the lower strengths on the slip surfaces was very convincing, because very heavily over-consolidated clays tended to expand rather than consolidate when subjected to shear. Mr Henkel showed a slide which gave some typical stress/strain diagrams for a normally consolidated and a fairly heavily over-consolidated clay which also showed the changes in pore-water pressure during the test. They had been obtained on samples of Weald clay, but there was no essential difference for other types of clay. The stress/strain curve for the overconsolidated clay was quite normal, but, after an initial slight increase at small strains, there was a very marked decrease in the pore pressure inside the clay specimen. It was that marked decrease which led to such very high strengths being measured in the undrained tests on over-consolidated clays. In nature, in a slip, where a heavily-over-consolidated clay was subjected to shear stresses, there was a tendency to set up those negative pore pressures, but on a long time-scale they could not exist ; pressure equalization took place and water was sucked into the stressed zone of the clay and softening took place ; in time the clay became soft enough and slips occurred.

*** **Mr F. L. Cassel** felt sure that there could have been few investigations of a major landslide in Britain as extensive as the one described in Mr Muir Wood's and previously Mr Toms's Papers. The reasons were

*** This contribution was submitted in writing upon the closure of the oral discussion.—SEC.

obvious. Such investigations were expensive and lengthy, and could only be undertaken if the object to be protected or saved was such a valuable investment as the main line from Folkestone to Dover.

That it had been so successful should be attributed to the intuition on which the boring and testing programme was built up, the more appreciable because there were so many false cues originating from much older investigations of previous slips at the site.

Mr Cassel referred to the difficulty of obtaining satisfactory specimens of the Gault clay, and the difficulty of testing them. The Gault clay was heavily preconsolidated and was, for a good portion of its depth, far below the plastic limit; it was actually more like a soft rock than a clay. Such soils were notoriously difficult to sample except in open excavation. The usual core barrel pushed into the soil was likely to shatter it along the numerous potential fissures. Even if one succeeded in obtaining a likely core, the preparation of undisturbed specimens for a triaxial or shear-box test often became impossible.

On the other hand, the rock was too soft for diamond-drilling purposes, which required a circulating liquid; either the tool became clogged or the clay softened. A lubricating liquid which did not soften the clay and had no affinity to it had not been tried yet.

British Railways, on the basis of some older ideas, had developed the tool shown in Fig. 12. Had that tool actually been used in the hard Gault beds and with what success? Was the core shown in the Figure taken from that stratum? It looked rather fissured. How many cores had been obtained? Were they unfissured? Could they be tested? And how did the results compare with those obtained in other tests?

It was a major problem to know how to obtain undisturbed test specimens in soil too hard for push coring and too soft or liable to softening for rotary sampling. He had in mind the hard Keuper Marls and similar soils, and felt that the problem was still unsolved.

Even where apparently good 4-inch-diameter cores could be obtained, the testing for shear strength was difficult, because of the fissures which became apparent when one started preparing test specimens. That could be overcome by picking out the best portions of a core and paring them down to unfissured 1-inch cubes. Those were tested in a special 1-inch-square shear box—probably the smallest shear box in existence.

The results were quite satisfactory and compared well with those obtained by other means. That shear box, which was specially made for such work, had since been used quite frequently in similar cases. Some apprehensions had been expressed whether the disadvantages of a shear box would not be enhanced by its reduction of size, but comparative tests carried out by Mr Cassel did not confirm those fears.

Mr Wood had mentioned on p. 419 some compression tests carried out by means of a concrete testing machine. Some of them obviously came from a heading, but it seemed also that some had been tested from Gault lenses obtained by core cutters. Could the Author say how the results compared with those tested in that 1-inch shear box ?

Mr Muir Wood, in reply, said that Folkestone Warren was so firmly associated in the mind with recurrent landslips that any prediction of future stability would be viewed with suspicion—the landslips were regarded as irresistible phenomena. Nevertheless, in the light of what was now known about the landslips, it did appear that the problems of the Warren could be reduced to those of maintenance of drainage and sea defence works. As Mr Toms had remarked, additional beach weighting to the west of the 1948–1950 work might become necessary in view of the continued high rate of erosion and very slight signs of movement, in the region of the outfall from No. 1 Heading. Further west, however, it was perhaps a little paradoxical to comment that the 1937 slip might well have contributed to the overall stability, since the sea wall had been swung around and thrust forward but only slightly damaged, and permanent weighting was thereby added to the toe. The stability that had followed every previous slip was transitory only since the sea rapidly encroached the broken material at the toe.

In reply to Mr Robertson's second question, site investigation at Folkestone Warren was virtually complete, with a single exception referred to below. Further field work would of course add detail and colour but was not expected to change the general picture. The main problems yet to be solved had to be tackled in the laboratory and concerned research on the more fundamental properties of the gault, including a study of the way in which initial failure of the gault occurred and how it was propagated.

He was grateful to Mr Toms for the information he had provided about the 6-ft-6-in. shield-driven drainage heading. That heading had struck the face of the undisturbed ground about 70 feet below the base of the Lower Chalk, in Bed XI of the Gault. In interpreting the figures in Table 1, it might be helpful to know that the natural moisture contents of samples clear of the slip surface lay 7–9% below the plastic limit (25–30%). The liquid limits of the samples were between 60 and 65%, as might have been predicted from existing knowledge of the way in which the Atterberg Limits varied through the thickness of the Gault. That relationship had been established from tests on samples and was shown in Fig. 45. The extension of the 6-ft-6-in. heading, reduced perhaps to a rectangular handmined form by an additional 35 ft or so, would allow the direction and degree of continuity of the enigmatic deep-set slickensides to be established and their full significance in the overall problem of stability understood ; that appeared to constitute the one outstanding urgent piece of field research.

Where a certain amount of remoulding of the gault had occurred along a slip surface, the $\phi = 0$ analysis might be expected to yield fairly consistent and reliable results, as indeed had been the case at that site ; it had more-

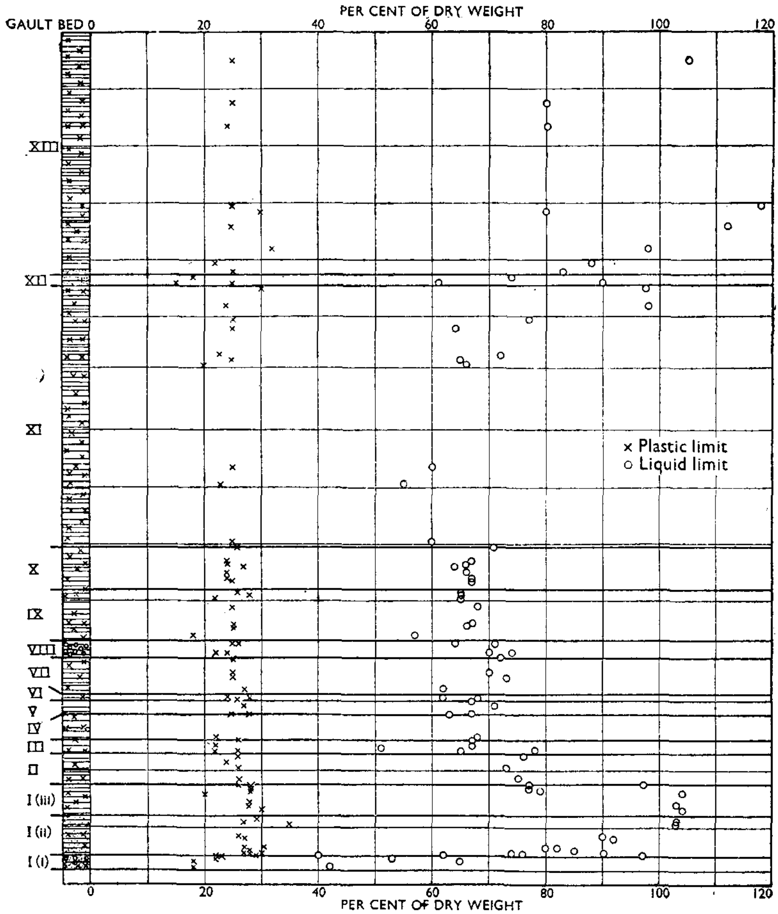

FIG. 45.—ATTERBERG LIMITS OF THE GAULT AT FOLKESTONE WARREN
(From sample tests 1939 and 1948–50)

over the undoubted merit of simplicity. The problem of initial failure of the firm gault required an entirely different approach which would have to be preceded by laboratory studies of the Gault which, he believed, were now being planned. The driving of the 6-ft-6-in. heading allowed, for the first time, truly undisturbed samples to be taken from the virgin gault.

Mr Henkel's remarks on dilatancy of gault related to fairly small values of strain and might well be relevant in evolving a final theory of the weakening of the structure of the clay. The point to which he had taken exception on p. 427, however, concerned the gault in a thoroughly remoulded condition; the dominant effect was no longer one of intergranular stress but of particle rearrangement.

As stated in Appendix II, the full hydrostatic head along the slip surface had been considered in slip analyses. The effective masses of the prisms of soil at the back of the slips were naturally increased by the lowering of the water-table. The very much greater adjustment to the horizontal hydraulic thrust did not appear in the force polygons in Figs 19 and 20, Plate 2, but had been taken into account in arriving at the figures of average gault strength along the slip surface.

Professor Naylor's tests were very valuable in assessing the qualitative merits of the various designs of sea wall and apron. Apart from scale factors, however, there was one over-riding difference between the action of the sea on a sand bed and on the gault. The latter, once in suspension, would only be re-deposited in deep, still water. The considerable scour indicated in the sand bed in the models would greatly modify the motion of the water close to the bed. Such limitations of the tests had, of course, been well realized by Professor Naylor. The hot-wire instrument for measuring velocities in water close to a boundary had recently been successfully evolved from the similar device that had long served aerodynamicists. It should therefore be possible, if presented with a similar problem in the future, to obtain a series of integrals of selected functions of velocity-time at points on the fixed bed of a model throughout a cycle of tides to represent relative rates of erosion of a solid, although destructible, foreshore.

In studying the problem of erosion at Folkestone it had been apparent that no quantity of shingle passed the breakwater arm of Folkestone Harbour. Across East Wear Bay, Admiralty charts indicated a sandbank, at a depth of somewhat less than 10 fathoms, following the line from the tip of Copt Point to Abbotscliffe. Whatever system of groyning might be evolved, the supply of shingle in the bay would steadily diminish, by attrition if not by drift, unless it was artificially replenished from time to time.

It was generally agreed, in part reply to Mr Lewis's questions, that erosion would be very slow seaward of the limit of the pitch line of waves, since the velocities over the sea-bed would be low and dissipation of energy small. Any actual exposure of the gault, however, would continue to soften and disintegrate unless provided with sufficient sand cover. Where there was an obstruction to advancing waves, such as the face of a sea wall, the resulting *clapotis* would greatly increase the rate of erosion for the depth of water.

Mr Muir Wood was glad to have the opportunity to acknowledge again the measure of help received from the Geological Survey whose members were becoming more frequent visitors to Institution meetings. Comparison of notes between Civil Engineers and Geologists led to an increasing appreciation of the measure in which the two professions were complementary. He was afraid that the occlusion of so much of the gault exposure on the foreshore at Folkestone Warren might be considered by collecting Geologists as an unfriendly act. Mr Casey's remarks on the foreshore eruptions were extremely interesting and his illustrations were eloquent witnesses to the theory of toe movement that had been advanced. The surface of the ground in Folkestone Warren was so scarred by slips and falls that upheaval of the foreshore would be the only remarkable feature of a minor creeping slip. Eventually, if that continued unchecked, a slip of more spectacular dimensions would be precipitated.

Mr Reynolds had provided very interesting and relevant information of the movement of water in the chalk behind the Warren. Rainfall records had been studied over a long period to establish the degree of correlation with the incidence of landslips; as he had stated, landslips had occurred after periods of more than average rainfall, sometimes spread over a period of weeks, sometimes of months, and often by no means exceptional in quantity. It should be understood, however, that the scanty sea defences prior to the nineteen-thirties had allowed the sea continuously to encroach upon the undercliff. For that and for other reasons, conditions of stability during a period of no slipping would steadily deteriorate and sensitivity to the effects of heavy rainfall would become more acute. Certain conditions of high water-table that would not have been critical a few years previously, could then suffice to set off a slip.

The spring at 173 O.D. occurred about 80 ft above the base of the Lower Chalk. The sub-zone of *Actino camax Plenus*—marking the top of the Lower Chalk—at that point lay between 352 O.D. and 346 O.D., obtained by a theodolite survey of points on the face of the high cliff. It was apparent, therefore, from present knowledge of the position of the upper surfaces of the Gault that the Lower Chalk there was about 258 ft thick, against the thickness of about 250 ft attributed to it by Jukes-Browne [9] and Osman.[5]

The ideal system of drainage had both to lower the general water-table in the Warren to the greatest possible extent and also to intercept water entering the Warren from the cliff. The first requirement was satisfied by existing drainage headings but the new 6-ft-6-in. heading struck the cliff face about 150 ft below the presumed spring line and any decision to construct the " ideal drainage gallery " mentioned on p. 424 would have to take heed of that fact.

In reply to Mr Cassel's inquiries, no further work had been carried out on developing a full-size sampler on the lines of the prototype shown in Fig. 12, since, as stated in the text of the Paper, time had not allowed the use of such a sampler for the 1948–1950 investigations, and the requirement

[9] Jukes-Brown, "The Cretaceous Rocks of Britain." Mem. Geol. Survey, 1900-1902.

had not yet recurred. An American manufacturer of soil mechanics appliances had recently figured in his catalogue a sampler quite indistinguishable in design, but whether that represented an anticipation of demand on the other side of the Atlantic or whether the tool had been independently evolved, Mr Muir Wood could not say. Short of increasing the borehole diameter to allow samples to be hewn out by hand, a tool of that type seemed to be the most promising method of taking undisturbed samples from such stiff clays !

Mr Cassel would see from a re-reading of the passage on p. 419 that all the samples tested in a concrete testing machine had in fact been obtained from borehole No. 5.

Mr Viner-Brady, in reply, said that of the seven very interesting and direct questions posed by Mr Robertson, the majority had been answered by Mr Wood.

Up to the time of writing the Paper and since the triangulation had been overhauled, no major recordable movement had taken place. The level checks taken upon the gault and other materials in front of the sea wall, however, had shown a gradual and continuous erosion in places. The areas affected were, in the main, the setback east of the 1948 work and a short length at the foot of the old sea wall west of heading No. 1.

Slight movements prior to the present works being carried out had been heralded by cracks forming in the sea wall. Since the completion of the stabilizing works opposite the zones of movement, none of those happenings had recurred, but there had been no relaxation of the constant watch throughout the length of the Warren and a 24-hour patrol of its entirety was maintained.

There was, of course, always the possibility of erosion going on unnoticed for a short time. In that case, while the seas were running, the sand overlying the gault or the foreshore material was thrown into suspension and the foreshore was then eroded. Where the tide had receded, the sand covering was once again in place and a small amount of foreshore had been lost, but that again would be detected when checked by levelling.

In reply to Mr Robertson's last question, Mr Viner-Brady could only say that he had confidence that the sea wall would perform the function for which it had been designed. It was known that by placing weight in that area the factor of safety had been improved. In 1951 a crushing of No. 4 heading had occurred, causing a lowering of the invert to the heading, and resultant ponding-up of water. Movement had been recorded on the tell-tales, showing that there had been a longitudinal compression of the ground containing the heading, owing to resistance by the toe-weighting 1948/49 works directly in front of the heading. That in itself had been encouraging, and also where works had been carried out purely to combat erosion a fair accumulation of beach had formed in front of them. The tension that had existed prior to 1948 over that stretch of railway had been relaxed considerably.

With regard to Professor Naylor's comments, Mr Toms had given the main reasons why the Halcrow No. 2 wall had not been adopted. It should be borne in mind that the working period for all those works had been very limited and simplicity of construction had played a large part in design. It had been essential to keep to the minimum the extent of formwork and in-situ concreting that would be covered by the sea at each tide.

When the eastern corner of the 1948 work had been turned, only a very short working period was possible, owing to the lower level of the foreshore and work in that area had had to be carried out mainly during spring tides. After the erosion east of the work at p. 29 the level of the foreshore had been lowered still more. When carrying out the 1951–53 works it had been decided to set back the wall so that the foundations and protective aprons would be at a higher level, thus assuring a longer working time. It had since been realized that that was a wrong decision, since the effect of the re-entrant had been to increase the amount of erosion at that point. However, it was hoped that the measures being carried out now, in extending the protective apron and the provision of additional and larger set-offs to the wall, would halt the erosion.

Mr Viner-Brady agreed with Mr Toms's suggestion that toe weighting should be extended west of the 1948 work to Martello Tunnel. Mr Wood had mentioned that the natural supply of shingle was very small and had given sound reasons for that fact. The western end of the Warren got even less of the share of shingle and the covering upon the gault was very sparse and often non-existent. That made the area very vulnerable to erosion.

The interesting facts mentioned by Mr Lewis regarding the behaviour of the London Clay on the north coast of Kent might well be taken as an omen for the future of the Warren. Records showed that where quite often erosion had been going on at a considerable rate, it had since slowed down, and very often had stopped completely, but there was no relation between the level of the gault at those places.

The bull-nosed coping added to the existing sea wall at the experimental bays was highly successful. Although perhaps a little larger than the more general bull-nose for a wall of that nature, it had a desirable effect in reducing "*clapotis*" and also stopped high seas from washing away the filling behind the existing sea wall. Since no other works had been undertaken where the run up for the waves had been so little, that coping had not been repeated. It had been hoped that the same effect would be gained by placing random rocks in the other bay of the experimental works, but it had been concluded, from the results, that much larger rocks would have been needed, and at a higher level, to have been as effective as the bull-nose.

The new set-offs being constructed in front of the 1951/53 works were more numerous and considerably larger than those existing. For the new set-offs, use of the large pre-cast block construction had been retained and the bonding of the blocks in the profile adopted now made it a relatively simple matter if, after observation, it was found necessary to enlarge or diminish the size. Mr Lewis was right when he stated that there had been very little experimental work done of that nature.

The lantern slides shown by Mr Toms, whilst illustrating the good shingle beach which had been maintained artificially before the 1939/45 war, had apparently misled Mr Lewis. There were groynes throughout that length of the Warren at that time but they had been largely covered by beach where close to the wall. Unfortunately, during the war years the anti-invasion measures adopted along the shore had prevented the tipping of shingle and impeded maintenance on those groynes. Consequently, the condition of the Warren foreshore had deteriorated. The District Engineer for that section had endeavoured to make good the dilapidation but the programme of new works carried out in that area had not made the task an easy one and had also altered the whole position, often making the existing groynes less effective and sometimes obsolete. A new layout of groynes was now being built and some old ones were being removed. Efforts were being made to keep the line of the groynes 11° S. of S.E., hoping to intercept as much as possible of the drifting shingle. All groynes ran down to low-water mark and were at approximately 250-ft centres. That construction differed very little from those built in the past, using rail piles driven at 8-ft centres with used sleepers five to six high in between them.

The closing date for Correspondence on the foregoing Paper was the 15th May and no contribution other than those already received at the Institution will now be accepted.—Sec.