Discussion

The Author introduced the Paper with the aid of a series of lantern slides.

Mr J. A. Cashin (Engineer-in-Chief to the Lyttelton Harbour Board), in a written contribution read by the Secretary, suggested that it might be of some value to harbour engineers faced with problems at all similar to those at Lyttelton to know something of the situation and events which had led to the hydraulic model investigations described in the Paper.

The province of Canterbury, New Zealand, had been settled about 1850, and the existing harbour at Lyttelton had been built between 1862 and 1867 to accommodate the small vessels of that day. The port was separated from the plains of Canterbury by steep hills about 1,000 ft high; those had been originally part of the rim of a volcano of which Port Lyttelton was the centre.

The harbour[4] had been enclosed by two rubble breakwaters totalling 3,200 ft in length, and as trade developed wharfs and jetties had been constructed until at the present time the length of deep-water berthage was 11,000 ft and the harbour shell was full. The volume of trade was still increasing and it was obvious that further extensions of berthage had to be built outside.

In 1936 a scheme of enlargement had been prepared—essentially a repetition of the pattern of the existing moles and so placed outside them as to enlarge the enclosed area. The length of proposed moles was 7,000 ft.

When the scheme had been devised the knowledge of wave behaviour and the techniques of investigation by hydraulic model had been much less advanced than they were today. It had been felt, no doubt, that to repeat the forms of the existing breakwaters would with safety cover the need to protect the harbour from storm waves. That scheme had been considered by the Board on many occasions subsequent to 1936, but, on account of the great expense involved, it had never been put in hand.

Since its inception the Board had been able to provide extensions of berthage, additional shipping facilities, and so forth without difficulty, but any additional berthage now involved the building of new and expensive protection against storm waves.

The existing channel, which had a length of about 2½ miles and a low-water depth of 32 ft, required about 25 years' dredging to cut it to its present depth and its maintenance at constant depth now cost about £30,000 per annum. The 1936 scheme would have necessitated the abandonment of that channel and the cutting of a new one by intensive dredging over as short a period as possible, for the existing channel would have had to be maintained until the new one was ready for use.

When Mr Cashin had become Engineer-in-Chief, it had appeared to him to be of the utmost importance to review the whole question of extensions with a view to reducing the cost to the minimum.

The most urgent need of berthage had been for the longest vessels, and having in mind that the average registered tonnage of vessels using the port in 1862 was 810, it had seemed likely that protection against waves from the south-west would not be necessary for modern deep-sea vessels. Consideration of the fetch in that direction and the wind velocity and duration had shown that the waves, observed to have a maximum height of 4 ft, would be of such short wave-length as not seriously to inconvenience large vessels. It had therefore been decided to abandon the proposed western mole and to use the new berths for only the larger vessels during the infrequent period of south-westerly gales.

There was no doubt that protection was needed against waves from the east. Although the present harbour was 5 miles from the sea, long swell waves, occasionally up to 10 ft high, reached the western breakwater. Measurements of wave-heights by an echo-sounder in a launch had shown that at times waves travelling up the inlet from the heads actually increased in height as far as Purau Bay, where some energy was abstracted by refraction. The maintenance of wave energy during the run up the harbour was due, as stated in the Paper, to the extreme flatness of the bed in cross-section; slopes rarely exceeded 1 : 5,000, except at the extreme edges. The longitudinal slope was 1 : 1,000.

If the length of a new eastern breakwater was such as not to interfere with the existing channel, i.e., if it projected from the northern shore only as far as the northern edge of the existing channel, then a very considerable amount of work and money would be saved. It was decided that the new wharf should be built about 500 ft from the existing shoreline to allow sufficient room for wharf apron, shed, roadway, open space, and railway marshalling facilities.

Knowledge of wave action against a wharf and breakwater of the type thus envisaged had been until comparatively recently a matter of empiricism and experience on the part of the designer, but recent researches (largely connected with war-time requirements on the North African and Normandy coasts) now enabled an assessment to be made on a theoretical basis. Reference to information and Tables contained in United States Beach Erosion Board Special Issue No. 1, 1st July, 1949, had indicated that, with the arrangement envisaged (Scheme A), wave conditions along the berth would be satisfactory from the breakwater to a position about 2,000 ft west. However, the radical departure of the new proposals from the existing form of harbour, and from anything hitherto envisaged, had made it highly desirable to obtain confirmation of the behaviour to be expected. Furthermore, a saving of about £2,000,000 of the £2,500,000 hitherto thought necessary for protection had appeared to be a possibility well worth the expenditure of an estimated £10,000 for site and model investigations. Apart from any consideration of reduction of cost, where large expenditure was involved it was essential to take all possible steps to ensure that the completed scheme would function satisfactorily. However, concerning the particular proposal for Lyttelton Harbour, if, in the light of experience gained in operation, increased protection was thought desirable, it could be gained by extending the mole to any desired extent, as for instance, from KI to KII. That would necessitate movement of the channel to the south, a work which could be carried out easily (and very cheaply when compared with the cost of cutting a new channel) by increasing the rate of dredging and concentrating on the southern side.

Mr Cashin had first considered having a model of Lyttelton Harbour constructed in Canterbury University College (N.Z.) a project which the Professor of Civil Engineering would have welcomed, but upon looking into the matter further he had realized that the science and practice of that subject had advanced since he had been engaged in it, to such an extent that it was desirable to enlist the services of a specialist. He had therefore consulted Sir Claude Inglis, who was then building the Hydraulics Research Station at Wallingford, and a mutually satisfactory arrangement had been concluded. Putting that part of the work in the hands of a specialist did not permit the harbour engineer simply to sit back and await results. It was important that engineers contemplating having their problems investigated should be aware of the amount of preliminary and contributory work which had to be carried out on the site under investigation.

For a model to operate correctly it should faithfully represent the prototype in all relevant respects, and only the harbour engineer could supply the necessary information to enable that to be done. He should, therefore, some time before enlisting the services of the hydraulic model specialist, make extensive and intensive investigations into the history and behaviour of the prototype, a project which might take years to complete.

Referring to the model under consideration, in addition to the charts and drawings of piled structures, pitched slopes and so forth, the required data had included wave heights, directions, and periods, and the length of waves at the mouth of the sea inlet and at places requiring protection; also whether seiche or range was prevalent at the berths. In connexion with dredging it had been necessary to determine and tabulate over as long a period as possible, the amount of dredging carried out in the harbour and channel, and to show how it varied in different parts of the channel, also where the spoil had been dumped. Concerning the regime of the harbour it had been necessary to know the type of material forming the bed in the different areas, where the material had come from (sea or land) and whether the regime had been maintained by tidal action, wave action, or both. The methods adopted to determine those factors were outlined briefly in the Paper. The work had been extremely interesting and although given every assistance by the departments of engineering, geology, and biology, at Canterbury University College, and by the N.Z. D.S.I.R., it had covered a period of 2 years continuous work and had been started more than 12 months before the construction of the model had been put in hand.

Whilst, as the Paper showed, it was possible so to adjust scales and so forth as to obtain reasonable predictions of wave behaviour in a projected harbour, he had always had some reservations concerning the accuracy of predictions of erosion and accretion, because of the diversity and number of the factors concerned. Obviously the correct scale representations would not be maintained for all those factors and it was necessary, therefore, to effect the best possible compromise. Unless a model had been shown, during the proving period, to be able to repeat a known change of bed regime following some known change, such as the introduction of a harbour work, he would have no faith in its ability to forecast bed behaviour in a projected harbour. He had, therefore, asked that, in addition to reproducing the channel-siltation pattern, an experiment should be carried out to repeat an occurrence of some years previously when, owing to a reclamation wall slipping outward, a large mound of mud had been raised about 15 ft above the prevailing bed level. It had been restored to its original level by natural forces in the course of a few weeks. Accordingly the experiment had been carried out—with complete success.

The model experiments on Scheme A had indicated that protection against storm waves would be rather better than the theoretical investigation had suggested. It had appeared also that the embayment at the western end was a bad feature. Thus Scheme K had been evolved as a modification of Scheme A, KI being that with the minimum length of breakwater, and KIII that with the breakwater extended if found desirable for the reasons given earlier.

Mr Cashin felt it no exaggeration to say that the present state of knowledge and the available techniques of hydraulic model investigation were such as to enable engineers to set out maritime schemes which until recently would have been deemed to be taking an undue risk in the interest of economy.

He paid tribute to the happy relations which had existed throughout the investigations between the Research Organization and the Harbour Board, and ended by quoting Osborne Reynolds, the originator of hydraulic models, who after making but two models in 1887 had said:—

> "This method of experimenting seems to afford a ready means of investigating and determining beforehand the effects of any proposed estuary or harbour works; a means which, after what I have seen, I should feel it madness to neglect before entering upon any costly undertaking."

Mr Gerald Lacey (Consultant, Drainage and Irrigation Adviser, Colonial Office) said that although 40 years ago the Paper would have been regarded as highly theoretical, today, owing to the very welcome spread of knowledge of model behaviour, it was immediately recognized as a valuable and eminently practical Paper.

In carrying out model experiments of the type described by the Author it was frequently the practice to have two models, one large and one to a smaller scale, and to refer to the smaller model as a "pilot" model. That was hardly a correct description of the Author's models, because they had been devoted to two different purposes, the larger being exclusively a short-wave model and the smaller a long-wave model. In the first model there had been short waves and since the depth did not enter the wave equations it had been desirable to make the model as geometrically similar as possible. On p. 6 the Author had stated that if the experiments had been made with an undistorted model the waves would have been excessively damped by friction, whilst calculations had shown that with the distortion actually used—i.e., with a horizontal scale of 1:180 and a vertical scale of 1:90—the damping of the waves had been just tolerable. The first model was nevertheless a short-wave model, and the exaggeration of the vertical scale had had no significance other than its function in reducing the damping of the waves to reasonable proportions.

The case of the second model was quite different. In a tidal model with long waves exaggeration was not only permissible but desirable; all experience of tidal models with mobile beds had shown that exaggeration was essential if silt movement was to be reproduced. In the model under discussion the scales had been 1:600 horizontally and 1:100 vertically, and in his introduction the Author had referred to a simple relationship in terms of the scales of the model, which determined what the terminal velocity of the silt particle in the model should be. On p. 18 the Author had condensed a large subject into three or four lines by saying: "It can be shown that particles in a model that are being carried along by a stream, as they settle out of suspension, can deposit in the correct place only if the speed of fall of the model particles is $x/y^{3/2}$ of the speed of fall of the prototype particles; where $1/x$ is the horizontal scale and $1/y$ the vertical scale of the model." Reference had been made to that equation some years previously by Professor Gibson[5] and more recently by Professor Jack Allen.[6] Referring to exaggeration in the vertical scale of tidal models, Professor Allen had stated that according to the theory of Mr Lacey,[7] y should be equal to $x^{2/3}$, and in an earlier chapter in the same work, had referred to the complication which inevitably arose when the vertical scale of the model was exaggerated. Professor Allen had quoted the ratio $x/y^{3/2}$ and had stated that the speed of the particle would have to be made "faster" according to that ratio (it was possible that he had severe exaggeration in mind), whereas in the case of the present Author's model the Author had had to make it "slower."

From the way in which Professor Allen had put forward that equation it might be inferred that whenever the vertical scale was exaggerated the same complication arose, but it did not arise if $x/y^{3/2}$ were made unity. In that case it was possible to use the same material as in the prototype, and since $y = x^{2/3}$ the required exaggeration was uniquely determined from the equation:

$$\frac{x}{y} = \frac{x^{2/3}}{x} = x^{1/3}$$

It was of interest that the equation in question gave exactly the same value for the exaggeration of the vertical scale as Mr Lacey had suggested 25 years ago. It had not occurred, apparently, to Professor Allen or to the Author to consider the case in which that ratio $x/y^{3/2}$ was made equal to unity. In the Author's tidal model, with a horizontal scale of 1:600 and a vertical scale of 1:100, if the horizontal scale had been made 1:1,000 it would have been possible to employ sea-water and the same material in the model as in the prototype. If the relation quoted in the Paper was correct it applied to much more than the mere deposition of silt during a slack period; it applied to all the mobile particles at any time when they were in suspension or saltation in the water. Since the Author had referred to that point, Mr Lacey could not resist the temptation to diverge a little from the subject of the Paper and to suggest that the implications of making the ratio $x/y^{3/2}$ equal to unity certainly deserved examination.

In the matter of the correct reproduction of silt movement there was a little difficulty, in the long-wave model. It was true that the Froude number was the same in the model as in the prototype; but in the one case, however, there was associated with that Froude number a rather finer silt than in the other, and therefore, although the Author had arranged for his rate of fall to be correct, Mr Lacey thought that the finer material might possibly be moved more readily, and therefore in greater quantity. He did not say that that was a criticism of the results, because great care had been taken to prove the model and compare, by applying a suitable ratio, what had happened in the model with what had actually occurred in the prototype.

As for the rest of the Paper, in the first model, with short waves, there was very little distortion. The Author was to be congratulated on the results which he had obtained, and one could only conclude that the harbour eventually decided on was undoubtedly the best one in all the circumstances.

Mr E. I. Loewy (Senior Engineer, Sir William Halcrow and Partners, Consulting Engineers) made a strong appeal to all harbour authorities to keep the most comprehensive records possible of local data concerning waves. A model investigation such as that described in the Paper could offer little hope of accurate predictions unless such information was available. Such an investigation also required a great deal of time, patience, and experience if it was to give useful results.

The type of basic site information required was difficult to get and called for a great deal of patience; the instrumentation was still rather crude. It was, moreover, very easy to be misled (particularly if the theoretical knowledge was incomplete) by subjective visual impressions. For works with which Mr Loewy had recently been concerned a very interesting instrument had been acquired. The measure of its success could not yet be stated because it had only just been installed, but he understood that it had been used successfully abroad. It was made by the French National Hydraulics Institute and consisted of a self-contained pressure-type recorder which was sunk in the sea at the desired location; after a predetermined time it was brought up and the record developed and analysed. The use of such an instrument should be encouraged as much as possible. It was not cheap, but it was robust and quite objective.

The Author was fortunate not to have been bothered with the measurement of wave direction, which was usually important. There was apparently as yet no instrument which would measure it, so that one had to rely on photographs and observations; but a great deal of patient looking at the sea and making notes was possible for engineers in the field and ought to be done. In the present case the Author had been able to exclude all directions but one. That was rarely the case, and it was usually necessary in such model work to decide what wave directions should be chosen, with very little field information upon which to base the decision.

Mr Loewy had been very surprised to learn that the short-wave model described in the Paper was to a distorted scale. He had always understood that for energy models, or wave models other than very long-period models, scale distortion should be avoided at all costs. It seemed always that the range of reasonable horizontal scales tended, with the size of the average harbour, to produce insufficient depth of water, in an undistorted model. A method of reducing the viscosity of water (other than by heating it), or the use of some other cheap fluid which had a reduced viscosity would avoid that critical problem. Perhaps there was some chemical substance which might help to reduce viscosity as easily as surface tension could be reduced by the introduction of detergents. Even without scale distortion there were enough uncertainties.

The existence of those uncertainties had been well illustrated by the Author, for at the beginning it had been felt that resonance was the explanation of the peaky nature of the response curves, whereas later it had been realized that the real reason was the haphazard coincidence of waves reflected in many different directions. It seemed to Mr Loewy, therefore, that every method other than distortion should be considered, and if necessary the client should be told that the model work had to take longer. In the present case it might have been possible—he put that forward as a suggestion, not as criticism—that instead of running the two models together one could have been made first and the other afterwards, so that the scale might have been perhaps about 1:120, which might just have been acceptable in both directions. On p. 16 the Author had mentioned a particular aspect of uncertainty arising from scale distortion which presumably he had had to leave as there stated, because it was beyond the present range of knowledge. It seemed a pity.

With regard to the third paragraph on p. 17, it would be interesting to know what the effect would be on the relevant response curves and iso-wave-height lines of taking a different definition of the prototype wave. Mr Loewy wondered whether the "amplification factor" should not be called an "attenuation factor," because it was to be hoped that the height of the wave in the sea would be attenuated in the chosen harbour rather than amplified.

The Author referred to the limitations of the iso-wave-height lines, which was very interesting. It was clearly a very fruitful way of showing the results, but, as he had said, hundreds of such patterns were needed to define the conditions of the harbour completely. Since there was thus as yet no complete answer to that "presentation," Mr Loewy wondered whether some kind of statistical evaluation might not be possible, for picturing the state of the harbour as a whole. At present, even if one was prepared to carry out hundreds of those tests, there would still be the problem of interpreting hundreds of charts. The commissioning engineer was at a loss when such a mass of data was presented to him and looked for some way of regimenting and simplifying it.

The Author had mentioned that because of the limitations of the apparatus the wave-heights were not made constant for the different periods for any one run of tests. It would be interesting to know whether it was possible to be completely satisfied that that did not introduce unknown errors, because otherwise one would feel inclined to say that somehow the apparatus should be devised—it was largely a matter of mechanical devices—which would make it possible, by setting dials rather than by the laborious method of unfastening cranks and eccentrics, that for every change of period there was a corresponding change in the throw of the wave-maker, so that wave-heights did remain constant. Mr Loewy wished to ask whether, among the various tests, at least one should not be carried out with the maximum known wave-height in the prototype; in fact, even further, a test, using waves as near as possible to the known maximum wave-height, and with the complex prototype character, to calibrate the response curves against the complex conditions which applied in reality.

Could the Author give some particulars of the apparatus used for measuring wave-heights ?

Mr F. H. Allen (Assistant Director, Hydraulics Research Station, D.S.I.R.) had been greatly interested by the Author's remarks about silting and turbidity currents. In certain portions of the Thames Estuary a similar material had been found on many occasions. The main body of water in the area in question, between Barking and Tilbury, had perhaps 500 to 1,000 parts per million by weight of solids in suspension; but under certain conditions near the bed a fluid mud layer was found, and an analysis of samples taken from that layer had shown that the concentration of material in it might range from 30,000 to 100,000 p.p.m. by weight. An interesting point was that in the Thames that material could be detected by an echo-sounder under certain slack-water conditions. Not a great deal could be said about the fluid mud itself yet, but apparently under slack-water conditions the upper boundary was fairly clear-cut and was capable of giving a subsidiary echo, which showed on the echo-sounder chart as a faint shadow above the hard line of the bed. That had been apparent for some years in the Thames, and it would be interesting to learn from the Author whether or not similar traces had been found on the echo-sounder charts presumably used at Port Lyttelton.

Mr Allen's second point concerned the behaviour of the turbidity currents in the model. He presumed that the density scale was 1:1, and in that case he would be interested to know whether any figures were available for the concentration of solids in the fluid mud layer in nature, and similar figures for the concentration in the model. It might not be possible to give that information because he realized that relatively little information was available on the turbidity current in nature.

Turning to the measurement of wave-heights, he had heard of a method known as "the starry sky" for recording wave disturbances in models, though he did not know very much about its use. The method used by the Author was, it was understood, somewhat tedious, taking a very long time and requiring a great deal of patience. It involved the measurement of wave-heights at a great many points and along a large number of sections. The "starry sky" method involved suspending a grid of small lights above the model. If the water-surface were still, the reflexion of the grid was undistorted, but when waves were present there was a highly distorted reflexion. If photographs were taken of that reflexion when waves were present, the photographs could be analysed to reveal wave-height information of a kind which he understood the Author wanted in connexion with Lyttelton Harbour.

On p. 6, when dealing with the collecting of piles together, the Author had stated:—

> "Whereas the flow around the piles in the prototype would be turbulent, the same flow around perfectly scaled model piles would have been viscous, in accordance with

> the much reduced Reynolds number. By collecting many piles together into a smaller number of ½-in.-dia. piles, which together presented the same total frontal area, the Reynolds number was raised to the transition region between viscous and turbulent flow."

Could he be more explicit on that point and explain exactly the lines on which he had calculated the enlargement required in the piles?

On p. 12 it was stated:—

> "In a rectangular harbour like scheme B the disturbance was expected to be built up by resonance whenever the length of the waves was a simple fraction, namely, 1, ½, ⅓, ¼ and so on, of twice the distance between the break-waters."

Would the Author explain why twice the distance between the breakwaters had to be introduced there, rather than the actual distance?

Dr L. J. Murdock (Manager, Central Laboratory, and a member of the Board of Management, George Wimpey & Co. Ltd) said that in reading the Paper he had tried to place himself in the position of a harbour engineer to whom the Author was trying to sell the idea of building a model. At first he had tended to be lulled, by the able and concise way in which the Author had dealt with the subject, into the belief that a model would perhaps give him a very good picture of the conditions that were likely to occur in a harbour which was to be built, but then he had become rather worried by the large number of uncertainties which seemed to exist in model-making for hydraulic work, and, like a previous speaker, he had been particularly struck by the apparent lack of data from Lyttelton harbour itself by which the model could be corroborated.

One point which had struck him especially was that with the distortion at present necessary in model-making it might be possible to distort the model so that, while it gave wave-heights and wave conditions corresponding to the actual harbour at certain points, such correspondence might not apply generally, leading to misleading interpretations.

Dealing further with those apparent uncertainties, there was an exaggerated vertical scale, a time scale, the extreme sensitivity to prototype period, and the use of well-water instead of sea-water. There were also the questions of the bed material, with its possible different deposition characteristics, scale effects, flow characteristics, and so on, wave reflexion from all the points in and around the harbour, absorption of wave energy, and possibly others which the Author had not mentioned, but which according to other investigators might have some influence, e.g., surface tension effects and the effect of dust on the water surface. Would the Author say whether he agreed with others on that point?

All those uncertainties had been mentioned, and possible errors of 10 or 20% had been indicated at various points in the Paper, so that one wondered just how large the cumulative error could be. It would obviously be wrong to add all those percentages together, but presumably if a statistical analysis was made one might get a figure of 60% or more.

Were the models likely to be substantially correct in seven cases out of ten, or something of that order, or was it more or less? Some assessment of the risk of failure would be valuable knowledge, because more damage could be done to model-making in future by unexpected failures than by the acknowledgement of difficulties in that respect and of the fact that the right answer would not always be given.

Were the problems which had arisen during the model study of Lyttelton harbour being made subject of further research, in order that future models would benefit from the results of the present one?

The time required for such model investigations might be a source of worry to the harbour engineer; it seemed that the scientists might require anything from 6 months to 2 years to get results while his ships and harbour were waiting; so that there was certain to be some pressure to get the work done. A result would be wanted even if some accuracy had to be sacrificed for the sake of speed. In view of the other uncertainties, there might not be any harm in a certain amount of "short-cutting" to speed up the work.

In spite of his apparent doubts, Dr Murdock felt that model-making would prove of increasing value in the future, and he wished the Author and the Hydraulics Research Laboratory success in their work.

Mr A. M. Hamilton (Consulting Engineer) observed that the Author's model was not the first model of Lyttelton harbour. An earlier model had been made by himself and Mr White-Parsons, and they had been in the happy position, unlike the present model-makers, of being able to examine the real harbour as they did their model experiments. The work had been done at the request of the late Mr Cyrus Williams,[4] then Chief Engineer and one of the great harbour engineers of modern times, who had believed that by means of models he could find out what was necessary for his harbour. The present model was the result of the ideas which he had passed on to his successors. In the earlier model they had tried every period there could be, with regard to both wave-height and wave-length, and had done the same class of complete investigation as had been done by the Author, though they had not had the same accurate means of finding out the movements at the different parts of the model harbour.

Under Mr Williams the work on that earlier model had been done in a very thorough way, though in a very short time. The scale of the model had been much the same as that of the present one, 1:600 horizontally and 1:120 (instead of 1:100) vertically. He exhibited photographs of the earlier model in action, which were so similar to those which the Author had shown that it would be difficult to distinguish between them; yet the Author on the one hand and he and Mr White-Parsons on the other had tackled the difficulties of the model in different ways, and both seemed to have succeeded to a certain degree in overcoming them. He spoke particularly at the moment of the relative heights of the two scales. Various speakers had referred to them and said that perhaps it was impossible to get the exact answer if the difficulty of the scale height could not be overcome. The question had been raised of getting a fluid which would act correctly at a true scale height.

The reflexion of the waves from the sides of the model had been mentioned. He and Mr White-Parsons had seen that the reflexion of the waves was greatly disturbing the surface. They were in the happy position of being able to climb a hill and look down on the harbour when certain wave-lengths were coming in, and could say "We must get it like that" and had tried to do so. Their answer had been to attach wire netting all round the sides of the model and put ends of rope tow into the water; that had had a damping effect. Whether it had been completely effective or allowed a certain amount of reflexion he would not like to say, but it certainly gave nearly the correct answer, and the photographs which they had obtained were, as he had already said, almost the same as those which the Author had shown.

The object which Mr Williams had had in view had been precisely the same as that envisaged at the present time to study harbour modifications and reclamations. They had tried the effect of a mole in different positions, but Mr Williams had not had so much money to spend in those days, and he had wondered if he could more cheaply lessen the effect on the ships and anchorage when certain seas were coming in at certain wave-lengths which caused damage to be done and moorings to be broken, so that some ships would not use Lyttelton. He had carefully aligned his main jetties to face the opening. The Author had shrewdly referred to that, although he had not seen Lyttelton. The object of aligning the jetties in that way was to have the motion of the water always longitudinal and not transverse to the jetty. Mr Williams had been so certain of that principle, proved both in the model and in the actual harbour, that he had given Mr White-Parsons the task of making a deep-sea jetty at Tolaga Bay,[8] which was one of the longest jetties leading out into open water in a very open harbour; for 25 years that jetty had been usefully employed by deep-sea ships without any of them suffering damage. They had been able to enter and leave in quite rough water. Mr Hamilton had also written a Paper[9] (unpublished) on his researches and was pleased to see that many of the points which had been raised in the present discussion had been investigated in that early model.

He would like to make one contribution to the work which had been done at Wallingford. When experimenting with his model, Mr Hamilton had observed a rather curious thing, namely, that the water at certain stages could go in and out through the entrance in what appeared to be a seiche. Upon looking into the story of seiches he had found that many were known, but the one at Lyttelton was different. He had, however, deduced a simple formula $t = K\sqrt{\frac{\text{depth}}{\text{area}}}$ in which t denoted the time of oscillation and K was a constant of an average value of 1·97 for the model or actual harbour. As the horizontal and vertical scales were altered, those two factors, area and mean depth had to be brought into it. Upon applying the formula he had discovered that the real harbour should be repeating the phenomenon of the model seiche at a slower speed. The seiche had a period of 13 sec in the model, and the period in the real harbour should be about 12 min. They had checked by standing at the end of the Gladstone pier and observing the movement of seaweed. The seaweed period or main harbour period had in fact been 12 min ! It had not affected the action on ships in the harbour, and he merely gave it as an instance of the almost uncanny accuracy which models could show.

The seiche had not been known of before because it had not been apparent on the tide-gauges then in use, but the Harbour Master had told them that some years earlier an old tide gauge which had a more open scale had been in use. He had found for them some of the old tide-gauge records, which showed the ordinary tidal range going up and down, but also showed clearly that at certain periods there was a definite oscillation in and out. Mr Hamilton displayed one of the old tide-gauge charts, and on it could be clearly seen the 12-min rise and fall. There were not sufficient records to indicate whether the oscillation was in any way dependent upon the weather, or what caused it.

Mr Hamilton believed that much could be done with the dynamic effects of water on model structures and thought that the future of model work was very promising; he did not believe that there was any limit to what could be ascertained. Mr Loewy had mentioned the need for an exact fluid. Mr Hamilton thought that either the exact fluid would be obtained or methods would be known to be adequately accurate without having to find some magic fluid to do the job. One could possibly alter g by centrifugal means, or surface tension say by vibration and viscosity by temperature, as well as employ ways already discussed.

For purely static models and their strengths as compared to full-size structures, similarity methods and theory were even more certain and accurate, and he had used them repeatedly himself in checking Callender-Hamilton bridge and hangar designs. There the proportionate loading in exact scale models of the same material in bolts and members, varied as the square of the scale reduction; or in bridges the unit deck loading had to be equal in model and original after allowance for dead load. Such model tests had been shown at Inst. C.E. Conversaziones of 1933–34, and perhaps afforded the first instance of bridge design by models.

Thus the absolute value of the Author's model research work on Lyttelton Harbour could not be doubted, and the Wallingford Research Establishment was to be congratulated in following up the work of Reynolds, Professor Gibson, and others who had such confidence in the practical value of harbour and river models for engineers, and dynamical similarity properly applied.

The Author, in reply, said that it was very humbling to observe what little difference there was between his work and that done by Mr Hamilton in 1927. After the discussion he had looked at some photographs of the earlier model and he agreed that they might have been shots of the recent one. He had also seen the report written by Mr Hamilton on the uses of dynamical similarity with particular reference to his Lyttelton model and was struck by its modernity. Only in the matter of technique did the Author have the advantage, and that was because of the great deal of experimental work that had been done on wave models in the interval. For example, experimental work had shown what a large proportion of the energy of long waves was reflected by sloping boundaries and consequently how necessary it would be to reproduce the southern cliffs of Port Lyttelton in the model. In the earlier model only the harbour side of Port Lyttelton had been reproduced, and an imaginary training wall had been built down the centre of the inlet. Another rather big difference between the models was that whereas the Author's 600 : 100 model had been used to study the behaviour only of very long waves, longer than 60 sec in period, Mr Hamilton's 600 : 120 model had been used for a study of storm waves. Such a distortion would not be acceptable now in a study of storm waves.

There was again some coincidence between Mr Hamilton's and the Author's findings regarding the long 12-min seiche. Mr Cashin had described it as a seiche of 10-min period and the model had shown it to be of 9½-min period; but that change of periodicity was easily—and most likely correctly—explained by deepening of the harbour and entrance that had taken place between 1927 and 1953. 10 min was too long a period for any system of standing waves in Lyttelton Harbour, and the Author had been able to show that it was the period of an oscillation in which the water level in the harbour rose and fell above and below the water level in Port Lyttelton. An experiment had been made in which the harbour entrance was blanked off temporarily and the water level raised inside it: the entrance had then been suddenly reopened, and the subsequent movement of the water surface had been recorded. The natural period of the rise and fall had then been 30 sec in the model, corresponding to 9½ min in the prototype.

Replying to Dr Murdock, he said that it had been far from his intention to lull anyone into false beliefs; rather was it his intention to state as precisely as possible what the models could do and in what way they fell short of perfection. Taking Dr Murdock's origins of uncertainty in the rigid models first: vertical exaggeration, he thought, he had dealt with in the Paper. The existence of a time scale introduced no errors. Sensitivity to prototype period introduced no errors if the speed control on the wave-generators was adequate. The use of well-water instead of sea-water introduced no errors. The existence of surface tension introduced errors in the speed at which the shortest waves travelled, but never with any of the waves that had been employed had the error amounted to as much as 1%, and that error had been neglected. He had not heard that dust on the surface introduced errors. The mobile-bed model, on the other hand, had to be judged by quite different standards. It was well known that not all the phenomena that should be reproduced in such a model could be made to scale similarly, and it was to some extent a matter of opinion which phenomena were the important ones. Confidence concerning a moving-bed model would depend largely upon whether it reproduced known changes satisfactorily.

With reference to the possibility of making short-cuts in the work the Author said that one of the objects of the Paper was to emphasize that a complete analysis of the wave action in a harbour took several months. It was not accuracy that would suffer from short-cuts so much as confidence that the worst conditions in the harbour had been observed. Whereas it was desirable that a model of the harbour, as it was finally to be built, should be tested thoroughly, it was reasonable to test preliminary designs sufficiently only to show whether they were probably better or worse than alternatives.

Some of the problems that had arisen during the investigation were being given thought at the Hydraulics Research Station, and accordingly succeeding models differed in some respects from the Lyttelton model, but it would be inaccurate to give the impression that the problem of how to get waves to travel across a small-scale undistorted model without loss of height would be solved, or that waves would ever be persuaded to follow in a distorted model the paths followed by the waves in the prototype. The biggest advance that he sought was in a method of presenting an enormous mass of evidence on the disturbance in a harbour under many different conditions in some compact and digestible form, so that an unerring choice could be made by the client between alternative harbours. That difficulty had been lucidly described by Mr Loewy in his remarks.

Replying to Mr Allen, the Author said that Mr Cashin had been asked whether any shadow had been found on echo-sounder records which could be attributed to a layer of fluid mud. He had replied that no shadow had been found.

Regarding the density scale for turbidity currents the model could only work correctly if the scale was 1 : 1. But it was not known whether in fact the densities were the same in model and prototype. He had no figures for either. Although he had spoken of a turbidity current, suggesting perhaps a well-defined current of uniformly dense fluid, it was more likely that during a storm in Port Lyttelton and during an experiment in the model there was a continuous variation in solids concentration from almost clear water at the surface to compacted mud at the bed. Because the concentration of solids at a certain point at a certain instant depended partly on the rate at which a fluid mud compacted into immobile material, and because the time scale for that compaction between model and prototype was unlikely to be the same as the Froude time scale, he could not expect the density of turbidity currents to have been correctly reproduced.

The "starry sky" was a very wonderful idea. A photograph of a model with a "starry sky" over it showed the water covered with a large number of "orbits," the size of which was related to the waves in the area. The method fulfilled a need for a quick if not completely correct assessment of the disturbance in a harbour. It suffered from two defects that derived from the fact that the "orbit" size was a measure of the change of slope of the water surface. In the first place, whenever there was a standing wave—and the disturbance in a harbour nearly always consisted of systems of standing waves—the maximum orbit would be found at the nodes, the places of maximum change of slope, where, however, vertical movement was zero. The places of maximum vertical movement were shown on the photograph as places of minimum orbits. One might say that the method gave one as wrong a picture of the disturbance as could be imagined. The other defect derived from the necessity to operate with unrealistically low waves if satisfactory orbits were to be obtained. That introduced important errors in the energy that was reflected from sloping boundaries. The Author accepted the possibility that an experimenter after some experience with the method might become adept at interpreting the photographs and obtaining from them the level of disturbance in the real harbour.

He wished to associate himself with Mr Loewy's remarks on the need for wave records and for data on wave directions. The factor by which waves were reduced was normally of no value unless the height of the storm waves was known. They were fortunate in the Lyttelton investigation to have a harbour there already, the performance of which was known to be satisfactory, or almost so; by studying in the model both the existing and proposed harbours the proposed harbour could be assessed without the need for accurate wave data. A further advantage of having an existing harbour lay in the way it obviated the need for data on what waves in the harbour were tolerable. The subject of permissible wave-heights was a complicated one, relatively unexplored except theoretically,[10] and to the Author's knowledge there was no accepted data on it.

Mr Loewy had hit upon an important point in suggesting that some uncertainty was introduced by operating the model with one wave-height for each value of the same period; but he did not agree that working with a constant wave-height would have been any improvement. It was known that the longer waves—those longer than, say, 25-sec period—were lower than the storm waves and that was partly taken care of by the characteristics of the wave generator. Although the results were presented in the form of amplification factors, perhaps thereby suggesting that the factors were not affected by the incident wave-height, it was known that incident wave-height did affect amplification factors. It was probably adequate to operate with one wave-height only, provided it was close to the maximum that was expected; and that was the practice at the Hydraulics Research Station. The thought of having to duplicate or triplicate all the experiments in order to deal with several wave-heights was horrifying.

The wave-height recorder consisted essentially of a shallow circular float 4 in. in diameter, fixed by a ball-joint to a vertically sliding shaft. There was a pin-hole in the shaft through which light was directed to fall on a photographic paper. As the paper was drawn past the shaft and as the float rose and fell a curve was drawn on the paper. The advantage of such an instrument was that it was simple and that the amplitude measured off a record was without doubt and with no possibility of error the amplitude of the float's travel, but there was a small error involved when measuring short waves of the order of 1 ft long because the float tended to span the troughs and sink into the crests. An electric-resistance wave recorder was now being used at the Hydraulics Research Station. It had the property of permitting the record to be magnified or reduced at will—something of a mixed blessing—and it overcame the difficulty that the float had in following short waves; but above all it had the advantage of allowing the record to be examined while it was being made.

In reply to Mr Lacey's remarks on the choice of scales he said that in the main there was no disagreement. It was agreed that the speed of fall of model particles should be $x/y^{3/2}$ of the speed of the prototype particles, and it was further agreed that if $x/y^{3/2}$ were unity then the prototype material could be used in the model. The Author was not certain whether Mr Lacey thought that $x/y^{3/2}$ should be unity because of the convenience of using the prototype material in the model, or because it resulted in a more satisfactory model. He would not have willingly used Lyttelton mud in the model because of its lack of uniformity from layer to layer. Analysis of core samples had shown that the surface was often ten times as coarse as a sample taken from a depth of 1 ft; and that as big a change was often found between depths of 1 ft and 2 ft. China clay, on the other hand, could be obtained in a consistent grade. Only if 1 ton of the material was similar to the next could experiments be repeated and the results of successive experiments compared.

The movement of sand and shingle by flowing water had been studied for many years, both experimentally and by observation of rivers and canals; and that work had enabled Mr Lacey and others to formulate empirical laws governing the shapes of large rivers and of small rivers that could be said to be scale models of them. Even accepting for the moment Mr Lacey's contention that the prototype material should be used in the model, the Author did not think that rule could reasonably be transferred to models where turbidity currents played an important role. The mechanics of material movement by turbidity currents had barely been touched experimentally—indeed he thought that the Lyttelton model was the first three-dimensional model in which they had been reproduced—and there was no experimental evidence for suggesting that the prototype material should be used in the model. Nor could he admit that there was a theoretical reason for the practice.

Mr Cashin's opinion on the investigation, as one who had in the past operated hydraulic models and who was now a harbour engineer, was greatly to be valued.

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The closing date for Correspondence on the foregoing Paper was 15 March, 1956. No contribution received later than that date will be printed in the Proceedings.—Sec.