*Discussion on Paper Nos 6852 and 6885**

Investigation of sea outfalls for Tyneside sewage disposal

by

H. R. Oakley and E. A. Dyer

and

Investigation of wind-induced currents and their effect on the performance of sea outfalls

by

P. J. Rance

AUTHORS' INTRODUCTION

Introducing the Papers, Mr H. R. Oakley said that not many engineers in Britain had had an opportunity of planning and conducting a survey of the character and scale of the Tyneside investigation, but there was evidence of increasing public concern about pollution of the beaches and sea, so that similar studies were likely in the future. The Authors were glad therefore, to have had opportunity of recording their experience and hoped that the ensuing discussion would provide comparison with experience elsewhere, for the profit of engineers responsible for future investigations.

2. Techniques had improved since the investigation commenced and more elegant methods of carrying out oceanographic work were now available. Nevertheless full understanding of the behaviour of marine discharges in situations similar to those off Tyneside still necessitated very careful and comprehensive investigations.

3. The Authors then showed a short film of the work to which Mr E. A. Dyer provided a commentary. The film showed the apparatus and methods used for observations at sea and the collection and analysis of samples.

Captain C. W. McMullen (Marine Consultant) said that he had one or two remarks to make about the M.F.V. *Essex Girl*. The vessel had had to operate for about 600 days, off and on, in open and quite dangerous waters and the Consulting Engineers took the rather bold step of buying the boat and becoming shipowners. This worked well; it was also economical and a considerable amount of money was saved to the Client.

5. There were, however, certain requirements in buying, operating and selling one's own boat, none of which was easy. The right boat had to be found at the right time for the right price, and it must be possible to sell the vessel well and quickly at the end of the job. Also, and one of the most difficult requirements, it was necessary to engage the right skipper for the whole period, and to have a resident engineer who was capable and enthusiastic where boat maintenance and seagoing were concerned. He thought that certainly these last two conditions, regarding the skipper and the resident engineer, were completely fulfilled, and as a result the operating was successful, and the cost reasonable.

* *Proc. Instn civ. Engrs*, 1966, 33 (Feb). 201–230; 231–260.

6. With regard to the current readings, he thought that a boat of 45 ft length was perhaps a little small for this type of water, as the vessel was very lively. The anchoring arrangements were a continual worry, as *Essex Girl*, like other M.F.V.'s, used a wire rope and her trawling winch. Ideally, a really heavy chain cable coming to its own winch was needed, but, unfortunately, very few M.F.V.'s were fitted in this way. Nylon could be used, although this chafed at the stem head. Because of the difficulty of getting current readings in strong onshore winds the results obtained from the coal drilling tower were extremely valuable.

7. With regard to an investigation of this size, there were arguments for using a remote-controlled current meter with a radio link. However, apart from the cost, about £3000, as opposed to the £890 for the equipment used in this investigation, the instruments could not operate at more than one depth unless several current meters were hung below the buoy. This could, of course, be done through a programmed radio link but was complicated. The bottom-moored instrument was also vulnerable to fouling by shipping, especially in the area off the Tyne, with five ports in the vicinity. However, the great advantage of a remote-controlled current meter was that it was possible to obtain readings in strong onshore winds, which were the conditions which were of particular interest.

8. As a last point, throughout the study the Consulting Engineers maintained a link with the various authorities. Co-operation was maintained with the Chamber of Shipping of the United Kingdom in London, which asked the opinion of various shipping companies, the Tyne Improvements Commission, the Tyne Pilotage Authority, the Royal Yachting Association, and also with authorities in adjacent ports. As the study progressed these authorities were kept in the picture as to the possible position of the outfall and its nature. It was understood that as a result of this co-operation it would probably not be difficult to obtain approval for such an outfall under the Coast Protection Act, 1949.

Mr F. L. Terrett (Messrs Lewis and Duvivier) said that he felt sure that in the future engineers concerned with the design of sewer outfalls would feel greatly indebted to the Authors of the two Papers for the amount of data which they had made available. However, it was still not possible to produce a standard to which engineers could work.

10. It was suggested in the Papers that this might be based on the coliform tests, but he doubted whether this was practicable for use as a design criterion, because of the conflicting evidence on the death rate of sewage bacteria and uncertainties about the mixing processes in the sea. If such a standard were to be considered, he would suggest that the coliform count should be related to the frequency of recurrence. On the other hand, he agreed that such tests should be made to check the performance of sea outfalls, and the information used when considering amendments to any interim standard which might be adopted.

11. In this connexion, he mentioned that at one site at which his firm had recently built a sewer outfall, a series of coliform tests was carried out before the work was constructed. These tests had been repeated, and he understood that the results were to be published.

12. For coastal authorities relatively small in comparison with the whole of Tyneside, the expenditure of upwards of £50 000, the amount spent on the investigation described in the Papers, could not be justified. In such cases, float tracking over complete neap and spring tide cycles provided the minimum information necessary. If it was found that the set of the tide was distinctly onshore or offshore, the problem was clear. If, however, the tidal currents were generally parallel to the shore, which was not uncommon, the effect of the wind was of paramount importance.

13. He saw no reason why the results given in § 20 of Paper 6885 should not be generally applicable round the coasts of Great Britain, and he would be pleased to have Mr Rance's views on that point.

14. In that paragraph and in § 54 of Paper 6852 a relationship between the wind speed and the induced current on the surface of the sea and at a depth of 1·5 m

was given. If the drift of the surface layers of the sea was onshore under the influence of an onshore wind, there must be a compensating offshore drift at some depth below the surface. He would like to know if any attempt was made in the model experiments to determine the form of the lower part of that velocity distribution. Mr Rance implied in § 59 of his Paper that the existence of such a current distribution would accelerate the dilution of polluted water as it travelled shoreward. He wondered whether Mr Rance could perhaps be more specific and give some indication of how the importance of that effect might be estimated.

15. The next problem on which he would like to comment was that of dilution, and he wished to return to the question of a standard. The Medical Research Council had said that a standard based on bacteriological considerations was impractical, and also that the most important requirement in the discharge of sewage into the sea was that adequate dilution should take place. Presumably in that context this meant that the oxygen demand should not exceed the available oxygen. The figures given in Paper 6852 suggested that a dilution of 200:1 gave a more than adequate margin of safety. That was based on a peak flow of three times the dry weather flow. He wondered whether the Authors would consider an initial dilution of, say, 300 times the dry weather flow a reasonable basis for design, with, say, a minimum neap tide flow across the diffuser of 600 times the dry weather flow. Figures such as those he had mentioned would enable a decision to be reached as to the depth of water into which the sewage should be discharged and the length of the diffuser.

16. It seemed that in calculating the initial dilution it had been assumed that the depth of the sewage field over the diffusers would be between a quarter and a third of the depth of water. He did not think that these figures could be applied generally as the depth of the field would be dependent upon the tidal velocity and the rate at which the sewage rose to the surface, which was a function of density difference, and also on the design of the diffuser. If the velocity of the current was high relative to the rate at which the sewage plume rose, it might be possible to achieve almost complete mixing of the effluent with the seawater.

17. The last point he wished to make concerned the removal or breaking up of solid matter which would float to the surface and be carried ashore by the wind, a matter which was emphasized in Paper 6852. No mention was made of grease or oil, which formed a slick on the surface and was an objectionable feature of sewer outfalls. On that point, he wished to ask the Authors whether they had considered the practicability of removing grease or oil from the effluent before it was discharged into the sea.

Mr W. Fyffe (formerly of the Engineering Inspectorate of the Ministry of Housing and Local Government, and former Chief Engineer, Port of Madras, India) said that he was very much impressed by the thorough care given to the investigation in the two Papers. They were most valuable additions to the available information on tidal streams and wind currents off the NE coast.

19. He wondered whether the Authors had taken any account of the onshore wind which often came in during the afternoon in summer. The prevailing summer offshore wind would reverse, usually between 3.00 p.m. and 6.00 p.m., and set up an onshore current for some hours until the land cooled and the sea wind had died down.

20. With regard to the effect of onshore winds on sewage, he lived at Lancing and had noticed the effect of the macerated sewage coming from the Worthing outfall which was a quarter of a mile long. It travelled beyond Worthing pier, which was 2 miles away, and was obviously oily because it calmed down the waves. It caused unpleasantness for bathers at Lancing, and, in fact, they did not bathe on the ebb tide if there was any wind coming from the south-west for that very reason.

21. On the coast of Northumberland most of the winds were offshore, and a long sea outfall might be satisfactory for a great part of the year, but on the south coast

it was entirely different. The North Atlantic Drift came across with the prevailing SW winds and went right up the Channel and along the west coast. There were on-shore winds for a great part of the year and he did not think that a sea outfall of that type would be satisfactory as it would discharge into water at a depth of about 30 ft, and the slick would float on the surface and come back to the coast in a matter of a few hours.

22. He understood that the idea of the sea outfalls came from California, but the conditions there were entirely different from anything in the United Kingdom. There one had lofty mountains along the coast, with very deep water close inshore. The 100 fathom limit was only a few miles out. The North Pacific Drift divided near Vancouver Island and the southern part went down the coasts of Oregon and California to about latitude 30° N, where it met the NE trade winds. These produced a major offshore current throughout the year. Any sewage going into that sea was naturally carried offshore. The outfalls were sometimes five miles long, terminating in a depth of water of about 250–300 ft.

23. He would like to know what the Authors thought of the chance of the sea outfall at Tynemouth being as successful.

Mr R. C. H. Russell (Director, Hydraulics Research Station) referred to the point made by Mr Terrett that there might be some local authorities who could not afford £50 000 for an investigation, and said that between the very cursory study of tidal currents conventionally undertaken before laying a sewer outfall, and the thorough and expensive investigation with which the Papers were concerned there lay a third method. By the use of radar it was possible to extend the conventional type of float-tracking until it became in fact a study of surface currents, revealing the stream-lines over an area which might be as much as five miles long and several miles out to sea. By this method it was possible to study the changing current pattern for a whole tidal cycle. It was necessary merely to increase very greatly the number of floats which were followed simultaneously.

25. The procedure adopted by the HRS involved the use of floats mounted with radar reflectors, and the repeated photographing of a radar screen, a ship-borne Decca 202. The main objective was to overcome the limitation imposed by optical position-fixing on the number of floats which could be tracked simultaneously. There was now no need to observe and record the various positions of each float: photographs which showed not only fixed objects such as the coastline, in relation to the floats, but also the time by including the dial of a clock in the photograph, could be taken and analysed at leisure.

26. The HRS had used the technique in Bideford Bay, in Swansea Bay, and in the Wash. So far they had not found it necessary to use more than twelve floats at one time, but this had been because of the nature of the problems. If one was studying a sewer outfall and wished to determine the stream lines and velocities in a large area, there was no reason why this number should not be multiplied many times. It was thought that up to 50 floats at a time could conveniently be distinguished from one another on a screen. The problem was not in distinguishing between floats but in starting them off in the right place and retrieving them, a limitation imposed by the craft available. The craft on which the radar set was mounted might, however, be used additionally for laying and retrieving some of the floats.

27. The advantage of getting stream lines direct from the sea rather than computing them from a combination of model and field data was that it provided a much more direct method. There was no need for calculations, nor for the assumptions (for example that the current at a point was obtained by adding the tidal current, the wind drift, and the residual N–S circulation) on which the calculations depended. On the other hand, the data were only applicable to the weather conditions prevailing on the day of the survey. It was not possible to extrapolate, whereas using the tech-

nique which Mr Rance had described it was possible to extrapolate to consider weather conditions other than those under which the surveying was done.

28. He thought that if the object was to observe the movement of floats under rather limited tidal and meteorological conditions, the direct method of following a great many floats simultaneously was the cheaper, but if one needed to predict float tracks under many tidal conditions, wind speeds and wind directions, the employment of Mr Rance's model technique brought about considerable economies.

Mr J. R. Preston (Messrs J. D. and D. M. Watson) wished to comment briefly on some elementary but fairly important aspects of technique.

30. With regard to the Decca navigator system, the accuracies stated in the Paper were those pertaining to daylight. At night or in adverse weather the errors might be two or three times those stated. A general system had been used, but more refined apparatus was available.

31. With regard to the technique for current metering, when current meters were used from small boats, boat-movement was transferred to the current meter unless a damping technique was adopted. Kelvin-Hughes had suggested a simple but effective method whereby the meter was given neutral buoyancy with small floats and weights, and was then tied to the main suspension cable by means of a long flexible offshoot, which absorbed the effect of boat movement.

32. On the question of drogues, the main requirement of a drogue was that its behaviour closely resembled that of the band of water in which it was floating. This meant that most of the mass of the drogue must be at the experimental depth and the remaining mass must be as small as possible so as to minimize the effect of wind and other factors. That had, of course, to be consistent with being able to sight the drogues from a distance of perhaps a mile or more, if two or three drogues were being tracked simultaneously.

33. With regard to the use of a light during night tracking, they had found that the battery had to be of at least 6 V to give a light which was bright enough, and it had to be a flashing light as otherwise it became indistinguishable from shore lights when the boat was to seaward of the drogue.

34. On the subject of tide measuring, gauges were first set up and fixed at a common datum, within harbours so as to be in deep water, but near enough to the sea to avoid distorting effects. It was decided that the gauges were to be read every 10 min, which meant that local water movements due to passing boats had to be damped out. This was achieved by putting a small fluorescent float into a $1\frac{1}{2}$ in. transparent plastic tube and introducing the sea through a hole only $\frac{1}{8}$ in. in diameter in the base of the tube. It was found that this quite effectively damped out any local discrepancies, and gave steady readings.

35. Finally, on outfall monitoring generally, he felt that no better model existed than the prototype. These prototypes varied considerably, and the more that was known of the variations the easier it was to estimate the behaviour of a proposed outfall. He felt that whenever an outfall reached the detailed design stage a sum of money should be included in the budget to permit a post-constructional monitoring programme of the sort that had been described in the Papers.

Mr K. M. Gammon (Central Electricity Generating Board) said that the Board had some practical experience in trying to trace the dispersion of warmed water. While the movement of the warmed water depended very much on the type of outfall, the direction of the flow and the conditions, especially wave action, he would regard a typical dispersion as one which very quickly came up to within a few feet of the surface. According to Paper 6852 (§ 85 and Fig. 12) it seemed that a very much deeper dispersion of fresh water was achieved. That surprised Mr Gammon, as he would have expected it to have had a similar density difference and to spread just as quickly as the warmed water, if not more quickly.

37. Turning to Paper 6885, he noticed that the Author referred to the effective centre line of the surface layer as being 1·5 m, i.e. about 5 ft. He wished to ask whether that was chosen because Mr Rance wished to correlate with the float tracking. Mr Gammon did not think that float tracking could be done effectively less than 5 ft below the surface.

Mr A. S. Crockett (City Engineer's Department, Edinburgh), said that Edinburgh had a problem very similar to that of Tyneside insofar as it had eight sea outfalls on a 7 mile coastline to the River Forth. He supposed the basic difference was that the water into which the Edinburgh outfalls discharged was an estuary rather than the sea, and this complicated the problem. The matter had been considered by his office for some time, and he and his colleagues were most gratified to read the two Papers because they filled a wide gap in present knowledge.

39. Reference had been made earlier to costs. He understood that the initial capital cost envisaged when the decision was made, was of the order of £20 million for a land-based works. The figure of £54 000 for the cost of the investigation did not seem untoward when measured against this amount. Turning to the summary of costs, he felt that the cost of the feasibility survey at £7000 was lower than the Edinburgh authorities had expected. He wondered whether Mr Oakley would indicate whether that figure was in the estimate that they first had in mind.

Mr D. H. Little (Messrs Walter C. Andrews and Partners) said that he had been associated with marine works for a good deal of his life, and model work, as such, interested him considerably. It was that aspect which he wished to discuss.

41. A recent paper[D1] dealing with model analysis and testing as a design tool had implied that there were limitations in model work. Paper 6885 did not clearly state whether the model was worthwhile or not, and Mr Little felt the Author might have done himself a considerable injustice.

42. Mr Rance had said that in the proving test most of the float movements observed unfortunately did not apply. He had been able to use only one in order to produce Fig. 12, and was rather disparaging about it, saying that the agreement was not very good. But in the diagram the distance used was 8–10 miles, and in these circumstances Mr Little thought the results were excellent, and that Figs 12–14 in themselves substantiated the method.

43. However, he wondered what justification the figures in fact had. Only ideal conditions could be produced in a model, and these rarely obtained in practice. For example, in a model a wind of constant value could be produced, but out at sea with many varying winds it was not possible to determine which one of a large number of floats bore the closest relation to the model results. The Author had been so diffident about the results that Mr Little could not tell from the Paper whether the Author thought the model was viable or not.

44. With regard to Paper 6852, his own experience with submarine pipe lines for water and oil was that actual costs usually exceeded estimates by at least 100%, and the pulling of pipes and similar work under water produced almost as many uncertainties as models.

Mr W. B. Harris (Messrs C. H. Dobbie and Partners) said that he had a point to make which might be considered rather irrelevant, but which concerned the standards which Mr Terrett had mentioned. In Paper 6852 a dry weather flow of 75 gal multiplied by three was mentioned. Obviously, what did not go down the pipe to the sea must, presumably go into a river, and the standards would be governed by the local river authority. One of the recommendations made in the interim report of the Ministry's Technical Committee on Storm Overflows was that the domestic

[D1] The references appear on p. 648.

sewage should be diluted by six, and the industrial element by two or three, and that any infiltration should also be withheld from the river. He would therefore be interested to know how the dry weather flow figure of 75 gal was broken down.

46. The object of the coliform count testings was not clear to him. He would have thought that this might be done once an outfall had been established. He had understood that the Water Pollution Research Laboratory were of the opinion that 90% of the bacilli died off after about four or five hours' passage through the open sea, and it would therefore be probable that quite a large proportion of the bacteria discharged from the end of the pipe would in fact die off before reaching the coast, even if their passage was directly onshore.

Mr J. T. Calvert (Messrs John Taylor & Sons) said that the Water Pollution Research Laboratory were carrying out fundamental experiments for the Coastal Pollution Research Committee set up by the Ministry of Technology. The work was only in a preliminary stage but some of the results conflicted with the assumptions made in Paper 6852. Perhaps the Authors might like to comment on these points.

48. Firstly, the work of the WPRL seemed to indicate that the mass of an effluent discharged from an outfall did not follow exactly the same line as floats or drift cards. The latter could be influenced much more by wind and therefore results relying merely on floats or drift cards might be misleading. As a result of this the WPRL had been using radioactive indicators which he thought followed more closely the path of the main discharge. This method could only be used when working from an actual outfall because, as had been pointed out, one could not get density effects unless the radioactive material was mixed with a large quantity of sewage. A second point was that Rawn's results for the initial dilution on discharge had been used whereas the work of the WPRL seemed to indicate that the initial dilution as the sewage rose to the surface was greater than indicated by Rawn's formula. Thirdly, a coliform half-life of nearly 4 h had been assumed. The WPRL work indicated that the rate of die-off in sea water varied enormously with climatic conditions, in particular the magnitude.

49. On the general question of sea outfalls Mr Calvert felt that the Papers were not sufficiently clear in stating the target aimed at in the design. In many places round the coast of the United Kingdom the outfalls were quite short and yet the Medical Research Council had come to the conclusion that they were no danger to health. Was it therefore right to adopt the amount of coliform organisms as a standard, and if so, what was the proper standard? On the other hand, if one was aiming merely at the preservation of amenities, he felt that this could be achieved by the removal of visible solids. Why had 3½ miles been chosen as the length of the proposed outfall? Unless it could be demonstrated that such a long outfall was essential in order to meet some prescribed amenity or public health requirement he wondered whether the cost of going so far to sea was justified.

Mr P. J. Woolland (Messrs Lemon and Blizard) said that Paper 6852 described the short-term aspects of the problem. What was the possibility of a long-term build-up of pollution in the North Sea? He had read recently that water in one of the American lakes was apparently suffering from oxygen deficiency, and wondered whether there was a possibility of this happening to the North Sea from cumulative discharges of sewage from Edinburgh, referred to by an earlier speaker, and from other towns on the east coast of Britain and the north German and Scandinavian coasts.

51. The rate of discharge had been referred to as being fairly high. He wondered whether a flow of 292 mgd discharging out to sea would modify the tidal currents in the area. The Authors had come to the conclusion that the length of three miles was adequate for unsettled sewage, and Mr Woolland wondered whether consideration had been given to primary settlement or full treatment, and if so, whether the

length of the outfall could be reduced with a consequent reduction in the cost of the scheme.

52. He had become slightly confused in trying to decide which were the correct figures in Fig. 11. The number of occurrences did not quite agree with the frequency. Perhaps the Authors would elaborate on this point.

The following contributions were received in writing:

Mr A. W. Shilston (Consulting Engineer) observed that all forward-looking engineers with a special interest in advancing the standards of public health would welcome these Papers as a significant contribution to the placing of the design of outfalls discharging domestic sewage and industrial wastes into the marine environment on a more practical scientific basis than had been evident in the past.

54. In § 102 of Paper 6852 the Authors referred to the contributions made by others during the investigations. Increasingly the role of the civil engineer concerned with the treatment and disposal of sewage was that of co-ordinator of the specialist knowledge contributed by a range of other disciplines. The skills of the chemist, biologist, zoologist, bacteriologist, geologist, sociologist, plant operator, and specialist contractor, came to mind. The time had passed when public health engineering works were designed with little or no awareness of many of the underlying problems of a non-civil engineering character involved. Mr Shilston used the word 'awareness' advisedly as the civil engineer could not possibly hope to be deeply knowledgeable in the various fields mentioned. The important point was to be aware of the significance of the contribution to be made by others and readily to accept the need for parallel working with colleagues specializing in the contiguous disciplines. The other point he wished to make in this connexion was the importance of engineers educating their clients or employers in the need to allocate sufficient funds for adequate site investigation and feasibility studies as a prelude to the design and construction.

55. Mr Shilston had recently attended a public inquiry concerned with a proposal to provide a long sea sewage outfall at a coastal resort. The all-powerful impression with which he had been left was the extreme sensitivity of the public and indeed general medical practitioners to the prospect of faecal matter, however small in quantity, being returned to the beaches by onshore currents. Particularly where onshore winds were commonplace during the recreational season, it was generally unlikely, around the shores of Britain, that a long sea outfall discharging crude sewage would entirely eliminate the possibility of some faecal matter appearing on the beaches from time to time. The public might not then be sufficiently reassured by experts that the risk of bacterial infection to sea bathers could be dismissed. In that connexion Mr Shilston was not sure whether the present state of medical knowledge enabled an implication to be drawn that freedom from risk of bacterial infection also meant freedom from risk of virological infection.

56. The emotional aversion of the public to a rare possibility of encountering purportedly sterile faecal matter (possibly in association with seaweed) whilst sea bathing contrasted with the equanimity with which dogs were accepted as uninhibited members of the beach community. That might seem equivocal to the rational mind of the applied scientist; nevertheless substantial weight should be given to the amenity aspect of the problem, particularly as the viability of a coastal resort might depend in large measure on the aesthetic appeal of the facilities offered. The ultimate weight attaching to the purely amenity aspect of any proposal to discharge crude sewage into the sea was a policy decision for the promoter of the scheme, but the professional adviser should not adopt an unduly clinical view of those subjective aspects of the overall problem.

57. In view of the increasing importance attached to the amenity aspects of the sea disposal of raw sewage within an area of recreational activity, where the conditions on land allowed, the use of medium length outfalls in association with the discharge of settled sewage might be the most suitable course to adopt in certain situations

particularly where the tidal range was small. That would palliate the amenity interests but underline the sludge disposal problem. Modern sewage sludge was becoming increasingly less suitable for return to the land, and conveyance to sea for disposal—regarded by some as an obsolescent procedure—an entirely acceptable idea for sludge disposal by coastal authorities. Successful as apparently was the procedure of the Greater London Authority in disposing of liquid sludge to sea by tanker, that method was probably unsuitable on engineering and economic grounds for the majority of coastal areas. Raw sludge could now be partially dewatered mechanically with a fair measure of success and there was scope for imaginative thinking as to possible ways of transporting partially dewatered sludge to sea for disposal should land sources of disposal not be available.

58. In one scheme involving the disposal to sea of raw sewage, the point had been raised of the possible deposition of solids on the sea bed at the outfall point and their likely movement onshore as a result of ground swell. Whilst the views of the maritime community espousing those opinions, avowedly based on observation over many years, were expressed with conviction, they might not have been entirely dispassionate in content. Mr Shilston asked the Authors of both Papers whether during their investigations and enquiries the mechanics of such bottom movements, in relation to the wind forces in the generating area, creating the swell, were identified and the effects assessed.

59. In conclusion, whilst all engineers would agree with the Authors of Paper 6852 (§ 102) that before an engineer could design he must have criteria to meet, the subject of treatment of sewage generally called for a measure of sound intuitive judgement, and that aspect of the subject which dealt with treatment by dilution with seawater was no exception.

Dr D. I. H. Barr (Department of Civil Engineering, University of Strathclyde) wrote that the Authors of Paper 6852 had demonstrated the complexity of designing a sea outfall for the efficient yet economic disposal of sewage. They had considered one particular case, but many of the problems with which they had dealt were quite general. The Authors had assumed that if a body of non-homogeneous water possessed sufficient buoyancy to keep it on the surface of a main flow, its spread and diffusion might be influenced by density current effects. This apparently reasonable assumption was in agreement with the writer's opinion, but appeared to be contrary to those of many workers in this field. Dr Barr was gratified that the Authors had referred [12] to some experiments of his which were relevant to the situation of spread of a buoyant effluent being carried on a main current. These experiments had been carried out in the Hydraulics Laboratory of the Department of Civil Engineering at Strathclyde, some years previously, and had been pilot experiments in rather restricted apparatus. A flume was now on order whereby the scope of this type of experiment could be much extended. The flume would be 10 ft wide and 20 ft long and would be run at depths up to 15 in. In most density-current experiments the problem was to provide the service flows and it was intended to provide a main current flow of 5 cu.ft/s and a non-homogeneous flow of 0·5 cu.ft/s.

61. Dr Barr had been interested to find in Fig. 12 that the full strength of the density non-homogeneity had been shown as occurring right up to the edge of the pollution fringe. He had always found this to occur in laboratory experiments. This was an aspect of phenomena that pointed strongly to the relevance of density spread.

62. The Authors had sometimes used the term 'densimetric velocity' for the velocity of the densimetric flow although they had at other times been quite specific and used densimetric flow velocity. Dr Barr considered that it was important always to retain the original meaning of densimetric velocity as defined by Keulegan; $\sqrt{(\Delta\rho/\rho)gH}$, where $\Delta\rho$ = the small density difference, ρ = the density of one or other

of the bodies of water and H=a representative length, usually a depth. The densimetric velocity was analogous to the friction velocity $\sqrt{sgd}$ of uniform pipe flow where s=the rate of piezometric head loss, d=the pipe diameter and g=the acceleration due to gravity.

63. Dr Barr shared the Authors' opinion that it would be worthwhile to attempt a model study with simulated density differences. Some years previously he had been concerned with site and model investigation where a single exit sewer outfall was present in the area. A reasonable representation of the spread from this outfall was obtained in the model when the density difference was simulated.

64. As the Authors had stated, much study was needed of density current phenomena. At a pollution fringe such as was shown in Fig. 12 various mechanisms of vertical mixing could be envisaged. If the main transport was into the surface layer which retained its identity, its capacity to spread would not be affected. If on the other hand, the surface layer was eroded, and thinned without suffering dilution, its capacity to spread would be reduced. The comparative turbulence levels of the two layers were therefore of great importance in determining whether density spread would be a minor or a major influence.

65. Dr Barr wished to know more concerning the initial treatment proposed for the effluent. The float card tests and the model experiments pointed to the necessity to discharge only fully comminuted solids at all times.

66. With regard to Paper 6885, Mr Rance had described how a great deal of valuable information to be applied to prediction of the surface movement of buoyant pollution was obtained in a hydraulic model where density differences were not in fact simulated. The operation of such an involved model was invariably a spur to the pursuit of understanding of the phenomena to be simulated. Without wishing to question the Author's decision not to introduce density differences into this particular model study, Dr Barr considered that he might amplify his indirect dismissal of this aspect of the problem given in § 4.

67. By stating that it was not possible to reproduce diffusion in a distorted model, the Author was presumably casting back to the familiar example of a submerged stream from a source within a horizontal turbulent flow. It would not be possible to obtain an approximately elliptical cross section divergence in a distorted model, to represent the approximately circular divergence of the prototype.

68. However, it was not clear that the foregoing example typified the problem in hand. It was generally admitted that the fresh, though contaminated, water released from a sewer outfall would rise to the surface of a homogeneous saline sea, and would initially form a surface layer. If such a surface layer were carried by tidal current, the mechanisms of vertical and horizontal dispersion normal to the main flow were quite different. In the vertical direction, the stratifying effect of density gradient had to be overcome in order for mixing to occur. In the horizontal direction there was no such inhibition to turbulent mixing, and, indeed, there might be spread due to the density difference.

69. Dr Barr thus considered that the problem of simulation of dispersion on a distorted model had two aspects. First, when a buoyant surface layer was under consideration, and second, when buoyancy was or had become negligible. The first might be approached in a hydraulic model by the use of density difference; the second was a much more difficult problem in distorted model studies. However, once this latter stage was reached in the prototype, the need for concern appeared to be past if the concern was, in fact, with an effluent. The introduction of wind-induced movement did not alter these premises. The effect of wind on the warmer surface layers of inland bodies of water was a well known agency for the initiation of internal surges.

70. It appeared to Dr Barr that the presence or otherwise of larger discrete elements within the discharge would have considerable bearing on the problem. The Author had mentioned the 'buoyant fraction' of the sewage, but it was not clear to Dr Barr what fraction of the sewage would not be buoyant or indeed whether the

discharge of completely untreated sewage was in fact contemplated. If so, the use of floats in model and prototype to demonstrate possible movements of larger elements was valid. But discharges of this kind, even well offshore, appeared quite undesirable. If, on the other hand, the effluent contained only finely divided solid particles, indications of movement derived from floats might well lead to over-estimation of the problem.

71. The Author was to be complimented on the degree of agreement achieved in the comparison of float movements in model and prototype.

72. Many fundamental points were raised in the brief theoretical treatments of §§ 21 and 38. The Author should have been much more explicit regarding the procedure for arranging data for Fig. 7 and references to the sources of the data would be helpful. The group $(\rho_a/\rho_w)u_*^3/\nu_w g$ of equation (2) was dimensionless, but its structure was complex. Dimensionlessness was not an absolute virtue in itself. Equation (2) might represent an evaluation of

$$\frac{u_*}{\nu_a/Z_0} = \phi\left(\frac{u_*}{\nu_w/Z_0}, \frac{u_*}{(\rho_w/\rho_a)gZ_0}\right)$$

but Dr Barr would not endorse this equation. Whatever derivation of equation (2) was in the Author's mind, some justification for the multiplication of groups was necessary. Dr Barr considered that §§ 21 and 38 should have been much amplified. Correlations such as those shown on Fig. 7 had in the past been obtained by the omission of a truly independent variable from the groups. In such cases the correlation became a check on experimental procedure and accuracy of readings.

Mr A. Smith (Department of Civil Engineering, University of Strathclyde) wrote that the work described by Mr Rance was of particular interest to him since he had recently attempted to observe the effect of gravity waves on the spread of a buoyant effluent.[D2] It was surprising to find in the Paper no mention of the mass transport component due to wave action. Under the action of a 30 mile/h wind it was likely that waves of a height of 3 ft or more and with periods approaching 4 s could develop within the travel time of 4·5 h. It was possible to estimate that the surface velocity due to the mass transport was of the order of 1·4% of the wind speed and the question arose as to whether wind drift or wave transport was the dominant factor under those conditions. Moreover, the incidence of onshore wave transport was considerably higher than that of wind drift, as a result of wave disturbance from a relatively distant storm source.

74. In § 50 Mr Rance had defined the object of the study as the determination of wind effects on surface movements of the sea. The mode of interaction of these movements with the mechanism of a density difference spread was, however, very much open to speculation and the interpretation of results from a homogeneous model study would require considerable judgement, supplemented possibly by auxiliary laboratory tests. The order of front velocity which might be expected in a sewage raft of the magnitude described might well be between 1 and 2 ft/s depending on dilution and distance from the source, whereas the total increment of velocity at the surface due to wind and/or wave effects would seldom be as much as 1 ft/s. This front velocity might be more correctly described as a celerity, since it was relative to the average velocity of the receiving body of seawater.

75. It was very questionable, however, whether a velocity gradient in the upper layers of the order of magnitude quoted above would have a significant effect on the spread velocity. When a density overflow was opposed by the mass transport velocity due to deep water waves, little or no deceleration of the front resulted until the opposing velocity gradient was sufficient to overcome the circulation pattern within the front. When this occurred, the tip of the front was fed not with fresh water from the

surface but with mixed water from the discarded layer at the interface. The result was immediate halting of the front followed by a rapid breakdown and progressive deterioration of the stratified layer. When the velocity gradient imposed by mass transport acted in the same direction as overflow front, the shape of the front was distorted. Preliminary observations, however, lent some support to the view that the imposed velocity gradient merely augmented the feeding/discarding mechanism of the front and added little, if anything, to the net velocity of propagation of the front.

76. Although the foregoing hypothesis was based on observations on the interaction of mass transport due to waves with front propagation, it seemed reasonable to suppose that the velocity gradient set up by the drift plus wave transport of wind over water, would act in a similar manner. The observations with waves had highlighted one other point of importance, namely the considerable resistance to vertical mixing of an established stratified system. The statement in § 59, concerning the increase in dispersion due to greater wind speeds should be treated with some caution.

The Authors of Paper 6852 thanked those who had enhanced the value of the Paper by contributing to the discussion, much of which had centred on the question of acceptable standards. Attention had been drawn to the necessity to take account of the emotional reaction of the general public, and Mr Calvert had felt that the design target was not stated sufficiently clearly; these comments pointed to the biggest difficulty met in the design of sea outfalls, of defining an objective and measurable standard acceptable to both lay and expert opinion.

78. In the introduction to the Paper the general aim had been given of effecting disposal without damage to health, amenity and economy. It was clear from the Paper that in respect of health, reliance had been placed on the coliform standard; but the Authors regarded coliforms as a convenient indicator rather than having direct public health significance. The minimum length of outfall had been determined by reference to the coliform standard, and whilst this might be subject to debate, it was considered preferable to reliance on vague generalities. The coliform degradation assumed in the calculations was considered to be a conservative figure and this followed the general philosophy of making calculations based on the most unfavourable conditions which would be expected to recur with reasonable frequency.

79. Amenity considerations started with conditions near the diffusers, which were dealt with in § 77. Each case had to be decided on its merits, and the Authors felt that the generalized approach suggested by Mr Terret was not feasible. Mr Fyffe's interesting comparison between conditions in California, at Lancing and off the NE coast was relevant. The main concern was with visual and identifiable solids, and although the terms of reference for the Tyneside outfall were to consider discharge of crude sewage, the Authors had made a firm recommendation that pretreatment should be given to remove gross solids and floating matter, and had drawn up a preliminary design of a screening and flotation chamber. Nevertheless, they agreed that it would not be feasible to avoid entirely a visual change in the quality of the water over the diffusers, but this was felt not to be serious having regard to the proposed length of the outfall. The reversal of offshore winds in summer which was mentioned by Mr Fyffe occurred infrequently off the NE coast and was of such short duration that it was unlikely to have a significant effect in this instance.

80. Considerations of economy were more tangible and the repercussions of the discharge on fishing and shipping interests had been carefully considered in locating the outfall.

81. The Authors agreed that their assessment of the behaviour of the sewage field depended on a number of assumptions, of which the most fundamental was that drogue-type floats move in a manner representative of the body of water in which they were immersed. This seemed an entirely reasonable assumption which was supported by the observations of the Tyne efflux. The Paper had stressed the im-

portance of wind drift and this might account for the divergence referred to by Mr Calvert of floats of different types and of drift cards from existing sewage fields.

82. The assumptions as to initial dilution and depth of field were more difficult to defend, but Mr Calvert was in error in supposing that the calculations had depended on Rawn's original formula, as the more recent work described in references 15 and 16 had been taken into account. The decision to consider the behaviour of floats centred at 5 ft depth followed the assessment of the likely depth of field, which was consistent with observations made on discharges of similar magnitude; as Mr Terret pointed out, several factors offset field depth, and more research was needed on this aspect.

83. Another important subject of discussion was the techniques used in the investigation. Rapid developments were taking place and Mr Russell had indicated one of them, although the windage of the necessary radar target was a disadvantage. Whatever methods were used, the uncontrollable variables remained, and the outstanding advantage of the model was to enable predictions to be made of field behaviour under any chosen conditions.

84. Understandably, questions were asked as to the outcome of the investigation. It should be explained that parallel consideration was given to the alternative of discharging to the estuary after an appropriate degree of treatment, and following comparison of capital and annual costs and other considerations, the latter was in the event preferred to sea discharge of crude sewage.

85. Turning to a variety of miscellaneous points raised in discussion, the Authors said in reply to Mr Crockett that the feasibility study in fact cost a little more than expected because prolonged bad weather delayed the diver's inspection of the sea bed, but some saving had resulted from the availability of information on soundings and current velocities obtained in other parts of the survey. New geophysical techniques were now available which could effect substantial economies in future feasibility studies.

86. On the cost of construction as apart from investigation, Mr Little appeared to have been unfortunate in his experience. The Authors believed that careful investigation was an essential prerequisite to accurate estimates of cost and that the cost of the work described in the Paper was justified by the more accurate assessment made of the feasibility and cost of the proposed outfall, on which very important policy decisions had been based.

87. The Authors said that Mr Harris had drawn attention to an important point in questioning the maximum rate of flow assumed for design purposes. Determination of the effect of storm sewage overflows on the estuary was the responsibility of the City Engineer, Newcastle, in collaboration with the River Authority and it was his decision to adopt a maximum rate of flow of three times a dry weather flow of 75 gal.

88. Mr Woolland had questioned the possibility of pollution in the North Sea building up to undesirable concentrations. There was considerable interchange of water into the Atlantic ocean, and the situation was quite different from that in a lake. Even without this interchange, rough calculations would show that the potential dilution was fantastically large, quite apart from the rapid degradation and removal of polluting matters by the physical, chemical and biological processes to which they were subject. His question on the effect of the outfall in modifying tidal currents could also be answered by reference to scale, the sewage flow being only of very local significance. The Authors thanked Mr Woolland for drawing attention to an error in Fig. 11, where the frequency scale should read from 10–300 days and not 1–30 days as shown.

89. The movement of bottom sediments referred to by Mr Shilston was a subject of some interest and complexity, but without attempting to analyse the mechanics, the Authors were persuaded that the long-term movement was seawards as described in § 33. The total weight of suspended matter in the crude sewage was only a small

fraction of that already conveyed to sea by the River Tyne or dumped in adjacent areas by barges carrying spoil and colliery waste.

Mr Rance, continuing the reply said, in answer to Mr Terrett that the relationships between the wind speed and the induced current at a depth of 1·5 m should be generally applicable around the coasts of Great Britain. In point of fact the effect of latitude should be taken into account:

$$\frac{u_s}{u_a} \propto (\sin \phi)^{1/2}$$

where u_s was the induced current as measured 1·5 m below the surface,
u_a was the wind speed, and
ϕ was the latitude of the point of observation.

Thus within the latitudes containing the British Isles, 50°N and 60°N, a variation of only ±3% was to be expected and could therefore be ignored.

91. However one should still be careful in making general use of the information. For example, if the area being considered was too close inshore, the localized flow pattern was an important factor to be considered. This led to the second point raised by Mr Terrett. With an onshore drift, there must be some form of compensating flow. Close inshore there were two types of return flow possible. It could take the form of rip currents (circulations in plan), or it could take the form of an undertow where the compensating flow was well below the surface. In all probability the slope of the bed determined the flow pattern. If there was relatively deep water close inshore, one might expect undertow rather than rip currents. The presence of undertow did raise one important point. Under these conditions the surface flow dived down a little way offshore leaving a trash line to mark the point of descent; such trash lines were to be observed in nature under favourable conditions. The implication was that any pollutant would be held offshore in this manner. However, wave action would carry it across the trash line and bring it inshore.

92. Because of its buoyancy the polluted water would tend to remain on the surface and in this light the subsurface return flow was not likely to contribute significantly to the dilution. Where currents passed over the diffuser area, the main effect was to deflect the rising plume rather than materially to increase the mixing.

93. In answer to the question on dilution by diffusion, unfortunately very little was known about dispersion in surface layers when strong winds were blowing. The existence of shear flow implied turbulence, and the greater the surface shear the larger the turbulence. Perhaps the greatest effect would be in mixing with depth rather than with lateral diffusion, but until research into this aspect was carried out, a quantitative assessment was not possible.

94. Mr Fyffe's remark on effluent calming the waves was interesting. However, it must be pointed out that although the presence of an oily slick would inhibit the breaking of the waves, the existence of marked stratification between the polluted water and the ambient sea could reduce the wave action at the surface.

95. Mr Gammon wished to know why the surface layer was treated as being centred at 1·5 m. At the outset it was intended that velocities in the model should be measured as close to the surface as possible. The instrument to have been used was the miniature propeller meter and the closest to the surface that it could be operated was, when scaled up to prototype terms, equivalent to 1·5 m. Accordingly, the drogues used in the float-tracking were centred at this depth. As it happened, the depth of the wind-induced currents was fairly shallow so that 5–6 ft was quite representative of the middle of the surface layer.

96. The paper referred to by Mr Little was devoted to structural models, but nevertheless there were limitations in hydraulic models (a clear example of this was given in § 38 of Paper 6885). A large part of the science of hydraulic models was devoted to overcoming these limitations. Mr Little appeared to have misinterpreted

§§ 44 *et seq.* The attributed statement that most of the prototype float movements did not apply was misleading since the method of producing theoretical float tracks was versatile, and indeed one prototype track under offshore drift conditions was used for proving purposes. However, since the investigation was concerned with onshore drifts it was natural that emphasis should be placed on proving the capabilities of predicting this condition.

97. It also appeared that Mr Little took the Author's explanations of the discrepancies between observed and predicted float tracks to be disparagement. On the contrary, the critical discussion was to demonstrate to those not conversant with this type of work just how good the agreement was. The conclusion given in § 49, that the method for predicting float tracks was satisfactory, was a clear statement that the model investigation was worthwhile since this was the sole object in view. In amplification, the appreciation of the model investigation given by the Authors of the companion paper (Paper No. 6852) in § 96 showed it to have been well worthwhile within the context of the sea outfall investigation as a whole.

98. It seemed that Mr Little's main difficulty was in his appreciation of the part played by the model. It must be stressed that the investigation was not a model investigation as such. The conventional model was one where the boundary conditions were imposed and subsequent results were immediately available by observation. In this instance the model represented one step in a programme of calculations: results were not available directly from the model.

99. His point about the variability of winds in nature was acknowledged and should it be necessary this could be accounted for by the method employed. In point of fact, the object of the investigation was to predict the worst conditions and surely this was achieved by winds blowing constantly from the worst direction, not by variable winds.

100. In answer to Mr Shilston, the possible movement of sediment by wave action was assessed. At the proposed area of discharge, the water depth was about 20 fathoms, and in such a depth of water the oscillatory motion at the bed due to waves with a significant period of 7 s would be insignificant.

101. Dr Barr's main criticism appeared to be the lack of detail on the more theoretical aspects of the Paper. He would appreciate no doubt that it was impossible to elaborate without exceeding the prescribed length for the Paper. The aim was to record the manner in which the investigation was carried out rather than to deal thoroughly with the scientific points it raised.

102. The decision not to introduce density differences in the model was made after rather more thought than the cursory examination implied by Dr Barr. The Clients were intending to design a diffuser such that the minimum initial dilution would be about 1 in 100. Thus the maximum density difference between the sewage field and the sea would be of the order of 0·0003—very small indeed. Considering the relative motion of the buoyant field to the sea one would expect a Reynolds' number Re of the order of 10^5 and a densimetric Froude number Fr of the order of 10^{-1}. Thus the value of the parameter $(1/(FrRe))^{1/3}$ would be of the order of 0·05 and with such a value one would expect considerable mixing to take place at the interface.

103. However, in the model, this parameter would be about 0·6, and therefore mixing would not occur at the interface due to the relative motion: this would result in pronounced stratification. In nature the turbulence of the sea would assist mixing, whilst in the model the stratification would inhibit this mechanism. One further point was that, if the model had been sufficiently large to permit mixing at the interface to be reproduced, the turbulent mixing in the x, y and z directions would not have been correct because of the distortion.

104. Turning to the analysis of the problem of wind blowing over water, Mr Rance did enlarge on this particular topic to some extent at the meeting. Most of the information used was taken from data collected together by Francis[D3] and in using

this information it was necessary to assume (*a*) that wave height, wave steepness, surface tension, etc. were relatively unimportant; (*b*) that the data applied to neutrally stable wind profiles. The main point was that an understanding had to be achieved within a limited period of time and pursuance of the finer points was out of the question.

105. The equation A suggested by Dr Barr would certainly not be endorsed by the Author. This equation implied that z_0 was an independent variable, whereas in fact it was a dependent variable: the roughness of the interface changed with the shear at the interface. Also, since there were six independent variables there must be three dimensionless parameters. A better expression of the variables would be

$$\frac{u_* z_0}{\nu w} = \phi\left(\frac{\rho_a}{\rho_w}, \frac{\mu_a}{\mu_w}, \frac{u_*^3}{g\nu_w}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (B)$$

Thus the parameter $(\rho_a/\rho_w).(u_*^3/g\nu_w)$ was not complex, as equation A would make it appear, and it could be interpreted as being the Froude number of the interfacial layer

$$\frac{\rho_a}{\rho_w} \cdot \frac{u_*^3}{g\nu_w} = \frac{\rho_a}{\rho_w} \cdot \frac{u_*^2}{g\delta},$$

where δ was a measure of the sublayer. This parameter was a measure of the stability of the interface.

106. Dr Barr, although he did not say so explicitly, seemed to feel the plotting of $z_0 v_*/\nu_a$ against $(\rho_a/\rho_w).(u_*^3/g\nu_w)$ introduced some form of spurious correlation. If he examined the plot carefully he would see that this was not so and furthermore the omission of other important independent variables would be shown up by an increased scatter of the points, not the reverse, as he suggested.

107. It was surprising that Mr Smith should have found no mention in the Paper of the mass transport due to waves, since § 22 did introduce this consideration. The figures he introduced in support of his contention that the mass transport due to waves was at least as important as wind-induced currents were even more surprising. Using the usual hind-casting methods, one deduced that under a 30 mile/h wind blowing for 4·5 h the significant wave period would be about 7 s and the significant wave height about 6 ft. This was in agreement with observations at the North Carr and Dowsing Light Vessels. The mass transport velocity at a depth of 5 ft due to waves with these characteristics was about 0·16 ft/s, i.e. about 20% of the current induced by the wind. In view of the many other variables which could not be accounted for, the contribution by waves, although by no means insignificant, was sufficiently small to be ignored in this instance.

108. When considering wave disturbance from distant storms it must be remembered that the shorter period wave tended to become filtered out, leaving the longer period swell as the main contribution. As Mr Smith was aware the mass transport varies inversely as (wave period)^3 and therefore distant storms are relatively unimportant.

109. He also suggested that the celerity of the sewage front would be 1–2 ft/s: this appeared to be about one order too high. Perhaps in deducing this figure Mr Smith did not take the initial dilution of the effluent into account. Since the density difference between the buoyant field would be so small and the interface would be unstable, any mixing in the upper layer would contribute to the diffusion.

References

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