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SEWAGE PURIFICATION AND STANDARDS OF PURITY.

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A DISCUSSION on such a subject as the natural purification of sewage, when promoted by the Sanitary Institute, is of special value, for the reason that it brings together a meeting which may be held to be fairly representative of the many arms of sanitary science. The biologist, the chemist, the engineer, the medical officer of health, etc., are here to compare notes, and to record their individual experiences and views; and the desirability of generally adopting the biological method of sewage purification must depend not upon the favourable verdict of one of these, but of all. In opening a discussion on a complex subject, our knowledge of which must still be regarded as very imperfect, it will be best perhaps to make an attempt to summarise the facts which, in our opinion, the work of the past few years appears to have established.

SEWAGE PURIFICATION.

In the first place we shall probably all agree (*a*) that the biological purification of sewage is not only the most rational, but also the most effective method of sewage disposal; and although exaggerated notions as to the economy of the method are very prevalent it is doubtless cheaper than any artificial means of purification that may be adopted; (*b*) that biological purification disposes in a large measure of the sludge difficulty; (*c*) that "double contact beds" (in which the sewage is locked up in the bed) give excellent results with partially sedimented sewage or septic effluent; (*d*) that such contact beds may be worked with satisfactory results when their depth amounts to twelve feet; (*e*) that contact beds three feet deep can be worked at the rate of 500,000 gallons per acre in eight hour cycles (one hour to fill, a rest of two hours full, one hour to empty, and four hours rest) per diem, as recommended by the Local Government Board; (*f*) that intermittent filtration of sedimented sewage through beds, the outlet pipes of which are constantly open, has given excellent results; (*g*) that the material for construction of bacterial beds is not of prime importance, so long as the particles do not disintegrate and are not too large or too fine, and that the best size for the particles probably ranges from about half-an-inch to two inches; (*h*) that it is very difficult indeed to maintain the capacity of the bed, but the more complete the sedimentation of the sewage, the less is the holding capacity of the bed affected; (*i*) that different sewages resist the natural agencies of purification very differently, and therefore it is not safe to dogmatise that because one installation gives satisfactory results with a certain sewage, it will furnish equally satisfactory results with a similar bulk of another sewage; (*j*) that bacterial beds increase in efficiency with use, and some time must elapse before they exert their maximum amount of purification; (*k*) that there is no material difference, from the point of view of subsequent purification, in the effluents from a closed (septic) tank, and a scum tank.

It may be disputed as to whether aërobic or anaërobic conditions are the more favourable for the liquefaction of the organic matter in suspension, and for reducing the stability of the more stable organic matter, but it may be taken for granted that circumstances favouring high oxygenation promote the ultimate changes of purification and the production of an effluent with good physical characters—incapable of putrefaction and of developing odour. This condition of things is sought to be promoted in all installations. One sees it in the system of emptying and filling fine bacterial beds in Dibdin's method, the aërating channels and beds of the "septic tank" method, the nitrifying channels and trays of Scott-Moncrieff, the Ducat filter, Stoddart's filter, Lowcock's filter, Adeney & Parry's suggestions for the addition of small quantities of an oxidising agent in the effluent, and in the use of sprinklers. Continuous contact beds do not meet this essential so well as some of the other alternatives in practice, and our observations go to show that the best results are to be obtained by an intermittent application of the previously sedimented sewage upon a bed, the outlet pipe of which is constantly open. The difficulty of evenly applying the sewage, freed from suspended matter, has now been mastered in a way which meets a very desirable object, namely, aëration of the effluent before it enters the bed. The distributor patented by Mr. Stoddart, which is almost identical in principle with a distributor fitted on to an experimental model used at University College by us some three years ago, rains the effluent very evenly over the surface of the bed, with the result that it becomes charged with a considerable amount of air; and similar results are obtained by various sprinklers, one of the first and most satisfactory of which is the Candy-Caink. Either by means of siphonage, an ejector, or by other automatic gear, the beds can also be dosed intermittently, at intervals of a minute or two. There is a positive advantage in thus allowing the effluent—in thin films—to be constantly in contact with air, and this cannot be secured in so-called "contact filters." We are inclined to attach so much importance to the continuous aëration of the effluent, that in our opinion every effluent drain should discharge into a shallow open conduit where possible.

This question of distributing the sewage evenly over a bed is of great importance, even after the sewage has been robbed of the bulk of its suspended matter, and where continuous contact is permitted; for all sewages applied to a bed will contain more or less suspended matter, and unless they are evenly applied this suspended matter is unequally distributed over the bed, and certain parts of it become over-taxed even before other parts have really settled down to work. The method of applying the crude sewage by a single carrier to a coarse continuous contact bed must have the effect of overtaxing the bed on either side of the trough, for the material there will go on collecting more than its share of suspended matter until the bed is full; such a filter therefore is not given the best chance of carrying on its work.

While it is an open question how far the disintegration, liquefaction, and decomposition of sludge is hastened by maintaining it under strictly anaërobic, as against aërobic, conditions, it does not appear that there is any gain by keeping liquid sewage free from contact with the air. And our observations show that sewage hastens to its ultimate goal of inoffensiveness by the oxidation of its polluting matter, undeterred, and indeed favoured, by the fact that from the outfall it is freely exposed to the air.

Sewage exhibits a remarkable tendency to aërobic purification, and this is so whether aërobic conditions are favoured or merely permitted. In fact we have by experiments observed instances in which nitrification proceeds at the same rate in the same sewages and effluents under such dissimilar conditions as free exposure in shallow open vessels to the air, and so enclosed in stoppered bottles that there are only a few c.c. of stagnant air above the surface of the liquid.

We were not surprised, therefore, when some of the conditions, under which these changes were found to occur, were reproduced on a large scale, to find that the same changes were to be observed.

Two Urban District Councils, in accordance with our advice, set aside one of their precipitating tanks for conversion into a scum tank, and during the past three months we have made observations on the changes undergone by the sewage in its passage through the tank before any scum formed over the surface.

The raw unscreened sewage passed slowly through the tank so as to deposit the bulk of its suspended matter, the rate of flow being such that it was calculated that the liquid contents of the tank were changed about once in 50 hours. At this rate of flow the degree of purification attained was very considerable.

Taking the mean of a series of analyses of the Willesden sewage, the composition of which is fairly uniform, we found its average composition to be:—Free ammonia 9 parts per 100,000, albuminoid ammonia 1·4, oxygen absorbed in two hours 7·9, nitrates nil. The mean of many analyses performed subsequent to six weeks' work under the above-mentioned conditions, furnished the following figures for the tank effluent:—Free ammonia 2·9, albuminoid ammonia ·46, oxygen absorbed in two hours 3·2, nitrogen as nitrate 1. Frequently the effluent was inoffensive, and at the most but a slight fæcal odour was appreciable. These changes, it should be noted, occurred while the sewage was passing over between two and three feet of sludge which had been deposited in the tank, and none of which—doubtless owing to the low temperature that prevailed during the early stages of these experiments—got itself buoyed to the surface as scum.

The effluents furnished by similar sewages from a scum tank and a closed septic tank are very similar to each other. The scum provides a cheap roof which preserves the heat in the sewage and thereby promotes bacteriolysis. One may broadly summarise the advantages of tanks as follows:—They promote uniformity of effluent; they permit of the digestion of from 30 to 40 per cent. of the solids, while the suspended solids in the effluent we have found amount to only from 10 to 30 parts per 100,000; the stability of most of the organic solids is rapidly reduced by a short sojourn in a tank, and subsequent purification thereby much facilitated.

The sedimentation and liquefaction tank, either open or closed, will, it appears almost for certain, figure largely in the future, owing to the simplicity of adapting such a method to the existing plant; and the less expensive open tank will generally be preferred to the closed tank.

The method of applying unsedimented sewage to coarse bacterial beds of the Sutton system is capable of producing a good effluent, but for large communities it must be more expensive. In biological installations it is the bacterial beds that constitute the chief expense, and a much larger cubical area of bed is necessary where the unsedimented sewage can be applied only at the rate of 100 gallons per square yard per diem than when, after sedimentation in a tank, as much as 500 gallons per square yard can be dealt with. Moreover, there is a gain in point of time in purifying sewage which is no longer in association with practically the whole of its suspended solids.

Even when the flow of sewage through a deposition tank is so rapid that a volume of sewage equalling the capacity of the tank passes through in twenty-four hours, the deposition is so considerable that it is easy to effect a reduction in the suspended matter of the sewage to some twenty to thirty parts per 100,000, on an average sewage, and the matter which still remains suspended appears to have little tendency to overtax a fine bacterial bed. We have experimented on this particular matter for several months at the Willesden and the Finchley Sewage Disposal Works, and we can state that our results are in accord with the above expressed views; we find that the sludge tends to maintain a fairly uniform bulk, and it certainly does not accumulate sufficiently to require removal for many months. If more time is given for sedimentation and the sewage is given a longer rest in the tank (of from 36 to 48 hours) then, as is well known, the sludge will maintain a remarkably low and uniform level for long periods of time, as shown at Exeter and elsewhere.

One of the best scum tanks we have seen is at the Acton Works; there the rate of flow is such that some 200,000 gallons of sewage passed through the tank of 145,000 gallons capacity in every 24 hours. The tank has been working for over a year, and the sludge is said not to average more than a few inches over the bottom of the tank, and a thick spongy scum some eight to ten inches deep has been maintained upon the surface. We have found the sludge to contain about 78 per cent. of non-volatile matter, while the scum contained about 39 per cent.

But what is another important thing to note as bearing most directly upon the practicability of a more rapid sedimentation in practice, is the circumstance that when it is necessary to remove the deposited sludge this may often be done with but little offence, and when it is applied to land it soon becomes resolved, and no nuisance is appreciated *in the immediate neighbourhood.* At Finchley such sludge has been spread over small areas of land without giving rise to any offence even in the immediate neighbourhood, and Dr. Eustace Hill and Dr. Robertson have both recently testified to a similar experience.

Rapid sedimentation, therefore, appears to be eminently practicable, and this, followed by the application of the effluent to primary and secondary bacterial beds treated by intermittent contact, with outlet pipes constantly open, and fed by sprinklers or Stoddart's distributor, appears to afford the best means at present known of dealing efficiently with sewage in the shortest time and on the smallest area. Generally speaking, land is becoming too valuable to be put to purposes of sewage purification, hence the modern endeavour to reproduce all the most favourable conditions of land and to concentrate them in a small area known as a bacterial bed; but where land, suitable in nature and quantity, can be procured equally good results are obtainable.

Five hundred gallons per square yard, per twenty-four hours, can generally be purified by such beds; very high figures of oxidised nitrogen are obtained, frequently reading from four to six parts per 100,000; and we have found dissolved oxygen amounting to about seven mgms. per litre. A large proportion of the total nitrogen of the sewage is thus oxidised to nitrate and the free ammonia may be almost nil. We have analysed samples of effluent from beds fed by sprinklers in which Nessler's re-agent added to the original effluent has scarcely given a re-action, and in which the albuminoid ammonia is represented by a small figure in the second place of decimals, and we are satisfied that many sewages of average composition can, after preliminary sedimentation, be purified at the rate of 500 gallons per square yard in twenty-four hours, when in constant work night and day, so as to produce entirely satisfactory results.

We have dealt elsewhere with the importance of secondary beds for the more rapid completion of a purifying process, the initial stages of which may not be disclosed in the ordinary form of an analysis; and we have satisfied ourselves by many experiments that by the use of secondary beds a much greater degree of purification can be obtained *in a shorter time* than by means of primary beds alone. Hence we attach a high importance to such provision.

One of the greatest difficulties which the natural agencies of the purification of sewage have to encounter is the disposal of the fat which tends to accumulate on the material comprising the beds. The amount of fatty material in sewage very materially affects the rapidity and ease with which it can be purified naturally, and we have found it possible to recover large quantities of fat from the particles of a bacterial bed which has been rested for several weeks. While, then, the discharge of most trade effluents into the general volume of sewage is not attended with any drawbacks, no trade discharges should be allowed which prejudicially affect the treatment of the bulk of sewage. These should, so far as possible, be collected and dealt with separately.

STANDARDS OF PURIFICATION.

A study of the reports on the results of the bacterial purification of sewage reveals a great want of system in the collection of samples, the methods employed for analysis and the construction of standards. It must always be remembered when speaking of the work of a biological installation, that it is not entirely manifest in the terms of a chemical analysis; and those of us, who have had experience of the varying characters of sewage, and even of effluents, will attach a qualified importance to a chemical report expressed in terms of a percentage purification of sewage calculated from the albuminoid ammonia or oxygen absorbed figures.

We have, for instance, observed in a large number of our experiments, variations in the albuminoid ammonia figure of the same effluent of at least 25 °/o in twenty-four hours. These great variations are to be got most generally in the first two or three days after collection. Consider how this matter may affect the calculation of the percentage purification according as to when, in point of time, the analyses are performed. We find that there is generally a positive advantage in favour of the effluent if it is not analysed until the third day after collection, so far as the estimation of albuminoid ammonia is concerned. This circumstance will explain in some measure the fact that, when samples of the same effluent are sent to different analysts, strictly concordant results are rarely obtained. If A analyses an effluent very shortly after its collection and he finds ·4 of albuminoid ammonia, B analysing twenty-four hours subsequently, may find ·3 or sometimes ·5, or even a greater difference. Suppose both calculate from a sewage with 1·5 of albuminoid ammonia, A will calculate 73 °/o of purification and B only 66 °/o. Surely the standard of a satisfactory effluent cannot or ought not to be one in which ingredients are below some necessarily more or less arbitrary chemical standard—especially when the standard to be maintained deals with an indefinite and ever varying quantity, such as the albuminoid ammonia and oxygen absorbed figures.

The reduction of so much per cent. in albuminoid ammonia, or of the putrescible matter, as indicated by the oxygen absorbed from permanganate in two or four hours, is assumed to represent a definite reduction of the total organic matter of the sewage. This is wrong because neither estimate is an inclusive one. It has been enunciated, for instance, that a satisfactory effluent should contain no more than one-tenth of a grain, per gallon, of albuminoid ammonia, nor absorb from permanganate in four hours, at 80° F., more than one grain of oxygen to the gallon. The observations and experiments in which we have recently been engaged demonstrate the futility of adopting any such arbitrary standards, as, in any sense, the measure of the pollution present in an effluent; and still more of the fallacy of assuming that a reduction in the amount of albuminoid ammonia, or of oxidisable organic matter, represents a corresponding reduction of the total organic pollution. It may be stated at once that the most remarkable feature exhibited by our analyses was the instability and changeability of an effluent, which would ordinarily be regarded as finished. It is the exception if on two consecutive days the results of analysis are constant, and the range is too great to be accounted for by error of experiment in analyses conducted with the greatest care. The futility of taking the albuminoid ammonia figure as the standard is clearly exhibited in the rapid and ample changes observed. Almost equally surprising is the great range to be observed in the quantity of nitrates present. We have noted in our experiments a leap from 2·6 to 5·8 in 24 hours, which is a variation so great as to make one doubt almost that the same effluent is being dealt with.

The following Table will serve to show the daily fluctuations in the figures of the Free and Organic Ammonias, and of the Nitrogen as Nitrates, in a sample of the Willesden general effluent, which was collected on November 20th, 1899:—

Date of Analysis.	November, 1899.									December, 1899.					Oct. 2nd, 1900.
	21st.	22nd.	23rd.	24th.	25th.	27th.	28th.	29th.	30th.	1st.	2nd.	4th.	5th.	6th.	
Free Ammonia.........	2·0	2·3	1·9	2·3	2·1	2·3	2·5	2·7	1·9	2·6	2·2	2·0	2 0	1·8	0·0
Organic Ammonia ...	0·42	0·53	0·45	0·34	0·29	0·35	0·40	0·31	0·27	0·24	0·32	0·20	0·30	0·30	0·09
Nitrogen as Nitrates ..	4·6	3·0	3·4	3·5	3·4	4·0	3·8	2·6	5·8	4·1	2·8	3·0	2·8	3·0	3·3

The following are the results obtained on six consecutive days from a sample of the Finchley general effluent, collected on November 29th, 1899:—

Date of Analysis.	NOVEMBER, 1899.	DECEMBER, 1899.					NOVEMBER, 1900.
	30th.	1st.	2nd.	4th.	5th.	6th.	
Free Ammonia	1·4	2·1	1·5	1·9	1·8	1·9	0·0
Organic Ammonia	0·34	0·23	0·20	0·18	0·23	0·20	0·08
Nitrogen as Nitrates ...	2·1	2·3	2·5	3·0	2·5	2·4	2·5

These two instances are selected as fairly typical of some dozens of others the facts of which we have in our possession.

It is clear then that what may be regarded as finished effluents may undergo daily changes so great that analyses made, even on consecutive days, give results so varying as to constitute the sample, as judged by present methods of analysis, a totally different liquid. We decided, therefore, to extend our observations for a longer period, and to deal with effluents of wholly different characters. Samples which came to the laboratory for analysis were utilised, and subsequent analyses were made from time to time as was thought desirable. These observations on the changes undergone by identical effluents through days, weeks, or months, subsequent to their arrival at the laboratory, show striking results, not a little unexpected, and in many respects puzzling. We have observed that generally during the first seven to fourteen days the tendency of the free ammonia is to increase when kept in a stoppered bottle and exposed to the light and temperature of the laboratory. This initial increase is not constant, and it is almost invariably associated with an intermittent rise and fall in the interval. After the fortnight a decline sets in, which though not steady, proceeds until the whole of the free ammonia has disappeared. In every case where an effluent was found ultimately to have lost its free ammonia, nitrates were found to be present in considerable amount, whether they existed originally in the effluent or not. Effluents kept from six to twelve months were generally found almost or quite ammonia free.

It is somewhat more difficult to speak of the behaviour of the albuminoid ammonia. Certain series of analyses demonstrate a tolerably steady decline for a fortnight or three weeks, to be followed later by a temporary rise; but others are to be found in which from the first a preliminary increase in the albuminoid ammonia is to be observed, and samples which have kept sweet for twelve months, and long been free from saline or free ammonia, showed albuminoid ammonia in excess of ·2 part per 100,000. The table already given will serve to show the almost explosive rapidity with which albuminoid ammonia will increase or decline in an effluent. It is interesting, moreover, to note that the albuminoid ammonia in offensive effluents was, in some instances, initially materially below that of several of the inoffensive effluents, and it may be stated of these latter effluents that at no time were they offensive.

The analyses clearly establish that sewage effluents may exhibit, while recent, low albuminoid ammonia, and after prolonged changes—which must be regarded as of a purifying character—display a higher albuminoid ammonia figure. On the other hand, recent effluents with a high albuminoid ammonia figure may remain perfectly free from offence, and exhibit a rapid decline in their albuminoid ammonia. And again, offensive effluents may start with a comparatively low figure of albuminoid ammonia and slight nitrates, and concurrently with an increase of albuminoid ammonia and disappearance of nitrate become inodorous.

These results may be puzzling, but they are facts not to be lost sight of when judging an effluent on the results of a chemical analysis.

It was to have been expected that when we came to the final mineralised products of sewage purification something like stability would have been met with. But our figures show that not even the evidence of the nitrogen which has been oxidised is preserved. Again, a reference to the table will demonstrate the extreme rapidity with which an increase or decline of this figure may take place. With amazing rapidity nitrates will increase in an effluent and with equally astonishing quickness decline. An analysis of a recent sample may exhibit low nitrates which, had it been deferred for 24 hours, would have shown the same effluent rich in this desirable product, and an effluent may be caught in which three-fourths of its total nitrogen is mineralised, which in 48 hours will have lost more than half of its oxidised nitrogen, and have added considerably to its unoxidised constituents (as disclosed in an ordinary analysis) without offence.

Nor can any rule be discovered in the facts before us; for if in many cases it is observed that there is first a decline to be followed by a rise in the nitrates present, the converse holds true in no exceptional degree.

It was something of a surprise to us, however, to find that the albuminoid ammonia in an effluent was capable of material increase as the processes of purification proceeded, since further pollution of the effluents with which we were dealing was out of the question. It has never been claimed for Wanklyn's process that it was an inclusive estimate of the whole of the organic nitrogen present in a polluted water, but it has generally been supposed that it constituted a fairly serviceable ratio to the total. But our analyses demonstrated that as a gauge of the organic nitrogen present in a sewage or sewage effluent, the albuminoid ammonia process of Wanklyn may prove very unreliable. Standards of pollution based upon this figure may be fallacious for two reasons, for not only will they vary with the time at which the analysis is made, but they will often fail to furnish even an approximate indication of the total organic nitrogen present. The fact that the albuminoid ammonia may increase even where the amount originally present was low, and that it may be many times lower in a sewage which has undergone no purification than in a clear, bright, and inodorous effluent, shows the absurdity of fixing on any standard of this figure as a definite measure of purification. The fact is that albuminoid ammonia, like oxidisable organic matter, is only a partial estimate of the total organic matter present, and represents only the less stable portion—that, in fact, which is incapable of resisting the attack of the alkaline permanganate. Owing doubtless to the changes induced by the action of micro-organisms, resistant organic matter is continually being reduced to that state in which it is incapable of resisting the attack of the alkaline permanganate, and so it appears as albuminoid ammonia. Albuminoid ammonia then has largely the significance of oxidisable organic matter. It expresses, with regard to the nitrogenous organic matter, a phase in its transition which is comparable to the condition of the organic matter estimated in Tidy's process. It appears that a sewage effluent is capable of continuously absorbing oxygen from the permanganate at a temperature of 80 degrees, not only for hours, but for days, and we have found it impossible to fix upon a period of time when it might be held that the oxygen absorbed formed a fairly approximate proportion to the entire amount of oxidisable organic matter in the liquid. What is needed undoubtedly is an inclusive estimation of the organic matter still in solution in an effluent, and until we get that it is impossible to lay down a hard and fast chemical standard applicable to all cases. High nitrates have always been considered desirable in an effluent, but our observations show that nitrates may be high at one time and at a later period become enormously reduced. It is evident, therefore, that no particular figure can be fixed upon as a standard of oxidised nitrogen.

The presence of nitrates in an effluent must not be regarded as a sure index of purification, although if they are found to persist in an inoffensive effluent for a few days after its collection the effluent is not likely to become offensive. An offensive effluent, if odourous after many months, will invariably be found to contain no oxidised nitrogen, and to show no material reduction in free ammonia. If, however, it is inodorous after several months, oxidised nitrogen will usually be found to be present and there will be little, if any, free ammonia. We are of opinion, therefore, that one of the best tests for a satisfactory effluent is to see if nitrates are present after incubation at 80 deg. F. for 48 hours, and if they are it will remain inoffensive no matter whether the original albuminoid ammonia was ·05 or ·5 part per 100,000. We must again insist, however, that the presence of nitrates is not a *sine qua non* for a satisfactory effluent, and that thoroughly bad effluents may be found at times to contain marked amounts of nitrates.

Any purely chemical standard, which is based upon our present methods of analysis, cannot in practice be an arbitrary one. Standards must have regard to the nature of the sewage, and to the conditions, volume, and uses of the stream which is ultimately to receive it. But certainly all effluents should conform to the following requirements: they should contain but very little suspended organic matter (certainly not more than five parts per 100,000); they should possess no odour of sulphuretted hydrogen; and there should be no physical evidence of putrefaction when they are incubated for a week in a closed vessel at 80 degs. F.

The bogey has been raised of the danger of the effluents from bacterial beds gaining access to rivers, because these effluents may contain specific organisms. We are not disposed to attach much importance to the issue raised. It is very rare that a stream which receives the effluent from a sewage disposal area is used for drinking purposes, and it ought never to be so used.

If the risks to be faced were very great, surely we should have had greater indications of it in the past, for hitherto some of the sewage on many sewage farms has been purified by the agencies encountered by little more than surface flow, over an area planted with vegetation, and the storm waters of most sewage disposal areas have only been run on osier beds or through coarse burnt-ballast filters, and receive no chemical treatment. The point is one on which some scientific data will be interesting and valuable.

Major-General C. P. CAREY, R.E. (London), said for the most part he agreed with all the points raised in the paper. So far as the septic tank treatment was concerned he considered that the Leeds experiment proved conclusively that an open septic tank was really quite as effective as a covered tank—that the automatic covering produced by the scum collecting on the surface really took the place of the artificial covering of the septic tank. So far as they knew the results were the same. With regard to the intermittent sprinklers on the filters he also agreed that it would be an extremely valuable addition generally in bacterial treatment. The tendency of the experiments carried on at Leeds and other places had been in favour of continuous filtration, with perhaps deepened filters, but using the intermittent sprinklers. The objections, of course, to the contact beds when working in cycles was the difficulty connected with the opening and closing of valves at certain periods. They could hardly expect—at all events, in the smaller districts—that this arrangement would be carried out, although upon its proper control the purification would, no doubt, to a very great extent depend. In regard to the standards he also agreed that the albuminoid ammonia and the oxygen-absorbed tests were, perhaps, not at all conclusive as to the purity of a sewage effluent. They were introduced by Sir Henry Roscoe for the Mersey and Irwell Rivers Board as a general guide which they had asked for in order to judge of the purification effected. They wanted to have some standard by which the members of the Rivers Board, who were not chemical experts, could judge for themselves whether an effluent was fairly pure or not, and to that extent they answered the purpose, but the real test of a sewage effluent, *i.e.*, that it should not putrefy, was after all what they would have to come to eventually, and he did not know that the albuminoid ammonia or the oxygen-absorbed test showed that conclusively. Adverting again to the intermittent sprinkler he had been very much surprised at the increased rate at which it is possible to filter sewage—it might be, very weak sewage: but Mr. Stoddart claimed to filter nearly a thousand or more gallons per square yard in 24 hours, which was a much higher rate than anything which had been done yet. It was very doubtful whether this rate could be permanently maintained. He felt strongly that previous sedimentation of some kind would be necessary, and if trade refuse were mixed with the sewage it would be absolutely necessary to provide that the mixed sewage should not be treated on bacterial contact beds, unless it had been previously settled, or, perhaps, chemically treated beforehand.

Dr. S. RIDEAL (London) congratulated the authors upon bringing before the meeting their views, with which he was generally in accord. Some of them were views he had held for years. In December, 1896—nearly five years ago—he brought this question of bacterial purification before The Sanitary Institute, and then stated that the closing of a septic tank was not an essential part of the process, but that an anaërobic preliminary was necessary before going on to the contact beds. He also then said, in reply to a question by Mr. Roechling, that in so far as the Exeter works had adopted contact beds they were wrong, and that when "the right ratio of air and effluent was maintained, the filters should work continuously." Contact beds, when they were full, were not allowing the effluent to come into contact with oxygen, therefore contact beds were only doing oxidation work when they were apparently empty. A "resting full" period was consequently a waste of time. That was in the very early days, and experiments for the last four years had gone in this direction. It was interesting to find Dr. Kenwood and Major-General Carey were now in accord with those views. Dr. Kenwood and Dr. Butler did not quite admit that the anaërobic preliminary was essential; they said "it may be disputed as to whether aërobic or anaërobic conditions are the more favourable for the liquefaction of the organic matter." He could not follow them in this view, unless they meant that an open septic tank, being exposed to the air, was therefore not strictly anaërobic; but he still claimed that an open septic tank exposed to the air, even when there was no scum on the surface, was anaërobic in its action, and he believed that the main change must necessarily be anaërobic. Further on in the paper the authors gave a marked illustration of that in regard to the fat difficulty: "We have found it possible to recover large quantities of fat from the particles of a bacterial bed which has been 'rested' for several weeks." This, in his opinion, proved that in a bacterial bed which had been resting for several weeks exposed to the air under full aërobic conditions the particles of fat were permanent. In a septic tank, open or closed, the particles of fat either sank down to the bottom or adhered to the bottom of the scum, and were then not under aërobic conditions, and consequently were hydrolised. They knew of no aërobic organisms which had the function of hydrolising fat, nor did they know of any organisms which had the function of hydrolising cellulose, so they could say certainly that two of the principal non-nitrogenous constituents in the sewage, viz., the fat on the one hand, and the straw and the paper and the cellulose and vegetable fibres generally on the other, were not at present known to be susceptible to aërobic putrefaction. These were the constituents which produced the major part of the sludge, and cause the filling up of a Dibdin bed, and therefore an anaërobic preliminary, or some other preliminary, was essential to get rid of these two constituents. They knew perfectly well that there were aërobic organisms capable of liquefying gelatine as well as anaërobic organisms, but because aërobic organisms had the function of liquefying gelatine, therefore to say that an aërobic process is capable of bringing about the preliminary necessary hydraulic change for the non-nitrogenous solids of sewage was misleading. With regard to the experiment at Willesden, it would be noticed that the results were obtained before any scum was formed on the surface, and therefore they were not meant to indicate the actual changes which would take place. These results were in direct conflict with the experience at Leeds, but further experiments were being made at Leeds in an open septic tank with a seventy-two hours' sojourn, so that further evidence would be forthcoming on that point. Twenty to thirty parts per hundred thousand for the suspended matter from a septic tank was a high figure, and he should have said it was easy to effect a reduction of suspended matter in the sewage to some fifteen parts per hundred thousand. He cordially agreed with the authors when they stated that "rapid sedimentation therefore appears to be eminently practicable, and this, followed by the application of the effluent to primary and secondary bacterial beds treated by intermittent contact, with outlet pipes constantly open, and fed by sprinklers or Stoddart's distributor, appears to afford the best means at present known of dealing efficiently with sewage in the shortest time, and on the smallest area." That seemed to sum up very well the position at the present time. With regard to standards, these were a perennial source of contention, and in his practice he adopted the rule to analyse samples as soon as possible. That had been laid down by the British Association Committee, of which he was Secretary some years ago. Most analysts of any considerable practice had met with varying results similar to those described by the authors. The albuminoid ammonia was bound to increase from a liquid which was not sterile, and which contained carbonic acid and nitrate. The disappearance of nitrates, too, had also been noticed, and the term he used to describe this was a useful one., *i.e.*, denitrification. This might be accompanied either by the decrease of albuminoid ammonia, as first shown by Gayon and Du Petit, or the albuminoid ammonia might be increased by the algæ and the green growth in other cases. Denitrification would account for many of the changes recorded in those experiments. After all it must not be a question of a particular standard, but one of ratio of the oxidised and the unoxidised constituents. That was a far better measure of purification than percentage purification—the ratio or amount of organic matter oxidised to the amount of organic matter that is to be oxidised—that would give a very fair measure of the actual quality of a liquid at a particular time, bearing in mind always that those liquids could go up and down according to the organisms which are present in the liquid.

Dr. S. BARWISE (Derby) observed that Dr. Rideal had touched on most of the points with which he had wished to deal. He quite agreed with Dr. Rideal in what he had said in regard to denitrification, although there were very many variations of the process. A case in point was that of an excellent water supplied to the Derbyshire County Asylum—a water containing practically no free ammonia, and the albuminoid ammonia represented by the third place of decimals, but containing a considerable amount of nitrates. A sample of that water kept in the sun for about a fortnight would yield ·02 and ·03 per 100,000 of albuminoid ammonia, owing to the confervoid growth on the sides of the bottle, while at the same time the nitrates have been found to disappear to a very great extent. He objected to the term "continuous filters" because they ought never to be continuous ; filters must have a rest, and it was awkward to know what word to use to describe them : intermittent sprinkling filters was a correct term but verbose.

Dr. Rideal suggested the term " percolating filters."

Dr. Barwise said that his experience had suggested the material of which the beds were composed should be of such a size as would go through a $\frac{1}{4}$-inch screen and stop at a $\frac{1}{8}$-inch screen. They had no works in Derbyshire which he would like to see working at the rate mentioned by Dr. Kenwood except one, where a little works was carried on by a defunct company which ingeniously pumped spring water into the sewage before filtering it. With regard to standards of purification, those who like himself had to advise bodies of business men, were compelled to adopt a standard of some kind as Major-General Carey had mentioned in the case of the Mersey and Irwell Board. The result of publishing a standard would be that all the authorities would require to be treated in the same way. It would be a useful thing to know from the authors, and also from Dr. Rideal, whether it had been found possible in practice to add to the effluents some antiseptic to prevent denitrification whereby samples could be kept for a week or two before analysis without the fatal and contradictory results Dr. Kenwood had obtained by keeping his samples. Having given the results of experiments and analyses he had carried on, Dr. Barwise pointed out that in considering the question of standards they must take into consideration the character of the river into which effluents had to be discharged. No doubt there was a real danger of effluents containing specific organisms being discharged into rivers, but the remedy to his mind was that where the effluent was discharged into a stream above the intake of any water company, that after the sewage had gone through the bacteria beds, or the biological process of filtration, it should be continuously filtered as water is at present filtered through a sand strainer. He thought the authorities could afford to filter the effluent at the same rate as the London water companies filtered their water, and by that means they would be doing everything possible to minimise the danger. Personally he would much prefer to see the effluent from a bacteria bed filtered through a sand filter than that after it had gone through the bacterial process it should be subjected to a process of irrigation on land.

Dr. Barwise then handed in the following as the standard adopted by the Derbyshire County Council, and he pointed out that he placed most reliance on the results of the incubator test.

Derbyshire County Council.—Purity of Sewage Effluents.

Organic Ammonia. A good effluent is one which contains *less* than 0·1 part per 100,000 of organic ammonia.

Nitrogen as Nitrates. An effluent should contain *more* than ·5 parts per 100,000 of nitrogen as nitrates. The nitrates are produced by the action of the oxygen of the air in the pores of the land or intermittent filters. The amount of nitrogen as nitrates is the best index of the efficiency of the action of the land or filters.

Incubator Test. A good effluent should be so thoroughly oxidised that it does not absorb more oxygen after incubation for one week than it does at the time of collection.

Shake Test. A simple test which can be readily applied is to shake vigorously for one minute a bottle half filled with effluent. All frothing should disappear in three seconds.

Mr. W. D. SCOTT-MONCRIEFF (London) congratulated the authors of the paper on the large attendance, which showed the interest taken in the subject by practical sanitarians. The views expressed in the paper were greatly supported by his own experience. In referring to the material for the construction of bacterial beds, the authors should, he thought, have taken the opportunity of warning those who proposed to adopt them against the use of burnt ballast, on account of its tendency to disintegrate. He agreed with Dr. Rideal that the reference made to preliminary anaërobic treatment was somewhat vague. Experience obtained from sewage which had travelled through long lengths of sewers might have somewhat confused the issue in the minds of the authors. It was unfortunate that more accurate information had not been obtained on the question of the amount of anaërobic fermentation which gave the best results. The value of the anaërobic factor ought to be estimated from its capacity to render the organic matter more readily broken down in the oxidising changes which followed it. He thought that no conclusions were reliable which overlooked the biological element in the changes which the authors referred to under the head of Standards of Purification. All the experiments pointed to the improbability of any two samples being biologically identical, and the extraordinary variations which occurred from time to time could only be accounted for by each chemical change offering a favourable or unfavourable environment for the life processes of organisms which flourished or were retarded as the conditions altered. He quite agreed that the standard of purification ought to be based upon the inherent capacity of an effluent to resist putrefaction, and not upon the quantity of albuminoid ammonia present. He quite agreed that contact beds were not capable, under ordinary conditions, of giving results at all comparable with what could be obtained from well aërated "percolation" beds. He suggested the term as a better one than downward filters.

Mr. KAYE PARRY (Dublin) said that in the presence of so many chemists and medical officers he would prefer not to discuss the early part of the paper at all, although he recognised the value of the accumulation of interesting facts which the authors of the paper had presented to the meeting; in the sewage purification question facts were wanted, not theories, and they could congratulate Dr. Kenwood upon having added very considerably to their knowledge on this important question. With regard to tank capacity for sedimentation, he might mention that practical engineers knew that if they were called upon to provide 50 hours' tank capacity for sedimentation alone, their works would become very costly, because tank capacity was a big question in connection with sewage purification, and it was an element that must not be overlooked. It was equally true in regard to the whole question of filtration that the consideration of cost must come in, because that system which did the best work in the most economical way was the one which would come to the front in the end. Possibly, however, the most interesting points to consider in the paper were those relative to the standards of purity for sewage effluents. It was instructive to hear Dr. Kenwood's observation as to the legitimacy of drawing inferences from analyses regarding sewage effluents. In this connection he would like to refer to the methods which his friend and colleague, Dr. Adeney, had advocated for many years as the best in estimating sewage effluents, namely, by estimating the dissolved gases. If there was a loss of atmospheric oxygen, and side by side there was an increase in carbonic acid, they might legitimately infer that something like fermentation was going on in the liquid. This test was valuable, not only in regard to sewage effluents, but also in connection with drinking waters, because they might have a water yielding a good analysis under ordinary conditions, but which had been robbed of its dissolved oxygen by some previous organic fermentation. The ordinary analysis would give them no indication of that, but if they estimated the dissolved gases they would find that the normal quantity of dissolved oxygen was absent, and then they could legitimately conclude that the water had been subjected to some previous pollution, although all other traces of pollution had disappeared. Years ago at the Leeds Sanitary Congress, he had pointed out that Dr. Adeney had divided the purification, or fermentation, of sewage into two distinct states—the first, in which carbon oxidation takes place; and the second, in which the nitrogen oxidation takes place. He did not agree with Dr. Barwise that all effluents should be required to reach the same standard of purity, no matter under what conditions and into what volume of river water they were discharged; when they were discharging into a small stream he considered they must have a higher degree of purity than when discharging into a larger stream. The volume of the liquid into which sewage effluents were discharged was always a matter for consideration, because if they were discharging into river water containing its normal quantity of atmospheric oxygen, the atmospheric oxygen would do all that was necessary, or very nearly all that was necessary, as regards the purification of the liquid discharged into it, provided there had been a preliminary removal of the solids. There was one other question which he hoped would some day come to the front, and that was the protection of our foreshores from sewage, and the recognition of the necessity of having some standard regulating the discharge of sewage onto foreshores and into tidal estuaries, as well as into rivers and streams, in order that the foreshores of these islands might be protected. His colleague and he had been making observations in regard to this matter so far as concerned the River Liffey, and probably the results obtained would before long be given to one of the scientific societies.

Mr. DOUGLAS ARCHIBALD (London) said that the question of suspended matter had been dealt with rather loosely. How far was the sewage supposed to be freed from suspended matter? Bacterial preliminary treatment in a tank was found to yield an effluent with about 10 grains per gallon, according to Dr. Rideal and other authorities. The authors of the paper had put it even higher, viz., up to 30 parts per hundred thousand or 21 grains per gallon. He mentioned this because it emphasised the regret that chemical treatment had been so much overlooked. He could mention a chemical effluent in which, to his certain knowledge, there was *no* suspended matter at all, and which consequently led to no sludging up of the subsequent filter. It seemed to be taken for granted that contact beds with a sedimentation tank would do all that was required, but if those contact beds sludged up with the suspended matter, then a very serious question would have to be practically faced by towns that adopted the bacterial system. It would have to be calculated how soon these beds would sludge up. The result of the four leading experiments at Leeds, Manchester, London, and Leicester, on raw and partially settled sewage, gave sixteen months for the life of a filter, or in other words, they sludged up at the rate of 88 per cent., per annum. Even after septic tank treatment they sludged up at a rate of over 40 per cent. Now, if filters were to be useless in sixteen months or two years, the material must either be replaced or new filters put in, and this in regard to the practical question of economy ought not to be overlooked. He confessed he was not able to accept the dictum of the writers of the paper that the biological system, or purely sedimentation tank, followed by contact beds, or even by continuous filtration, was sufficient, because in the end it would be found that the sludge difficulty was only apparently disposed of by being transferred from one point to another point of the system. In any case they had to get rid of the sludge in the tanks, and he was surprised at the statement by the authors, as a result of their experiment, that the sludge taken out of the septic tank at Willesden was innocuous, because the results of the experiments at Leicester showed that the sludge in the tanks there was very offensive indeed. But, assuming for argument's sake that the septic sludge was absolutely inoffensive, there were still two practical difficulties for towns to face which had not been adequately considered by the bacterialists, viz., tank sludge removal and filter renewal. In a certain case he knew of, the production of a sludge by chemical means was so superior to that resulting from the biological methods that it obviated all the difficulties of disposal and removal to which he had referred. With regard to the standards if the albuminoid ammonia, as the authors seemed to think, was not a reliable test, the adoption of such a test as that of the dissolved oxygen, suggested by Dr. Rideal, might be an advantage. He quite agreed that something should be done to make the results of analyses comparable. It was an unfortunate thing in regard to standards, that these were so often expressed in different units. It would be an advantage if investigators could agree upon some common form in which to express their meaning, and if some-one would come forward with a unit which everyone would accept, deciding once for all whether it should be grains per gallon, or parts per hundred thousand, or parts per million, much confusion would be obviated.

Mr. A. J. Martin (Exeter) expressed the pleasure with which he had listened to the paper, and said he had seldom heard so much common sense on the "standard" question compressed into such a small space as there was in the latter part of the authors' paper. There seemed to be a little friendly competition as to the best word for describing the continuous method of filtration, and he would suggest that they might call it the "trickling" system. It was rather astonishing to note the divergence in the figures given by different speakers as to the quantity of work that continuous filters could do; and he would point out that in order to make proper comparisons of the amount of work done by "trickling" filters and contact beds respectively, the depth of the beds ought to be stated in each case, otherwise the comparison was misleading. Referring to the remark made by Dr. Rideal as to the contact method being wrong in principle on account of keeping the effluent in the bed out of reach of oxygen, so far as he could remember several analyses of effluents from contact beds made by Dr. Rideal himself showed a large amount of dissolved oxygen in the effluent as it emerged from the filter. That seemed to him to be a good answer to the contention that the liquid is kept in the filter under anaërobic conditions instead of aërobic conditions. In comparing the two systems, they should not lose sight of the fact that the great practical advantage in the contact system introduced by Mr. Dibdin was that it overcame the great difficulty met with in the trickling system, viz., that of distributing the work uniformly over the filter. It effected a perfect distribution of the effluent, and every cubic inch of filtering material was made to do its proper share of work. The point to be aimed at in a contact bed was to get into a given space the largest possible amount of surface; but if this point was pushed too far by having the particles of filtering material too small, the liquid would be held up by capillary action, and they would not be able to get rid of it. Short of this, he agreed with Dr. Barwise that it was desirable to increase the surface so far as possible by reducing the size of the material. The limit to which such reduction could be carried was generally imposed by the question of what is called "sludging up." He was astonished to hear on the authority of the last speaker that a contact bed would sludge up in sixteen months, because he knew of some that had been in work four or five years and had not lost their capacity. After the first year the water capacity of a bed properly worked seemed to become practically constant. The size of the material which might be used was undoubtedly bound up with the question of the amount of suspended matter in the effluent, and he was surprised to hear the authors of the paper speak of such a large quantity as thirty parts per hundred thousand as a normal amount.

Dr. KENWOOD pointed out that the figures mentioned were "from *ten* to thirty parts per hundred thousand."

Mr. MARTIN said that in his experience ten grains per gallon was an outside figure. The prevention of "sludging up," by the elimination of suspended matter, was the most important question in relation to contact beds which remained to be dealt with. He had therefore devoted considerable attention to the problem of delivering to the beds an effluent free from suspended matter, and had devised some very effective means for the purpose.

Dr. W. BUTLER (London) then replied to some of the criticisms. He thanked the meeting for the way in which the paper had been received, and said there were not many points to answer, but such as there were they were of importance. He was particularly pleased to hear Major-General Carey endorse their remarks as to what is the essential character of a good effluent. A good effluent is that which would not undergo offensive decomposition under any circumstances to which it is likely to be subjected before finally reaching the sea. That was a fact which must always be kept before them, and it was a fact that seemed invariably to have been lost sight of in the past. It was the criterion by which the figures of a chemical analysis must themselves be judged, and it mattered not what these figures were if this condition were conformed to. If that condition, which he had laid down, was conformed to the figures resulting must be the right figures. Had that fact been kept before them he did not think they would have been troubled with the standards which have been a source of difficulty in the past, and if there was one thing which they desired in that paper to do it was, in face of the very astonishing facts which their experiments had disclosed, to dispose once and for all of the use of any standard of albuminoid ammonia, or of any other figure in the chemical analysis, which is to be set up as a measure to which all effluents must conform. Such a standard was essentially wrong in passing judgment upon a sewage effluent, and their experiments had driven them to the position—although when they started they were inclined to attach to it some of the importance which has generally been ascribed to it—that albuminoid ammonia is of no value as a measure of the essential quality of an effluent. Of no value for one reason, that it increased and diminished in the same effluent from time to time, and this had given rise to perhaps the most serious criticism of the evening. Dr. Rideal, Dr. Barwise, and Mr. Scott-Moncrieff had all attempted to explain this change in the action of the albuminoid ammonia in the final effluent. They were informed that the increase in the albuminoid ammonia in the final effluent is associated always with denitrification, and that this denitrification and increase of albuminoid ammonia in a liquid containing carbonic acid, salts, and nitrates, was due to their organisation, to the growth of algæ, &c., which when subjected to alkaline permanganate would yield albuminoid ammonia. To his mind that was a very astonishing explanation, because it was made to cover such a variety of facts that occurred under such a variety of conditions that it would be truly amazing were that the explanation. He could not accept that explanation, and he was sure Dr. Kenwood was with him in this matter. The explanation that they (the authors) offered might not be the correct one, but it was one which seemed best of all to meet the enormous number of facts with which they had to deal in their observations. The observed changes were quite analogous to the behaviour of the oxidisable organic matter, and they were in line with most chemists on the subject in looking upon the oxidisable organic matter figure as merely a phase, a physical condition of the organic matter in solution—an expression really of stability, of its capacity to be attacked by the agent used. Now albuminoid ammonia seemed to be strictly analogous to that. Albuminoid ammonia expressed in terms of nitrogen, or in terms of ammonia, that part of the nitrogenous organic matter present in a state of comparative instability. There was a residue of organic matter which did not appear in their analysis—a residue of stable organic matter which they did not get to-day, which they might get to-morrow, because its stability in the meantime had been shaken. This fact would account for a rise in the albuminoid ammonia, while its diminution was due to its reduction to a further and more ultimate product. That was a very important fact, because when they took that along with the other changes that occurred in the effluent, they would see they did not get in the sewage effluent anything of a final character. A sewage effluent was not a chemical product, but a changing medium which is one thing now, another thing to-morrow, and still another on the day following, and these changes went on until it ultimately approached to what we understand to be pure water, but these changes were prolonged, and continued for a long time. They had kept effluents under observation for twelve and for eighteen months, and they had known effluents not to reach anything like a final stage of purification for twelve months under varied conditions. Therefore they would see they could never hope to have anything in the nature of finality in a sewage effluent which they turned off their farms; and if they were not to do that, then it was easy to say good-bye to final standards as to what effluents should be. Again he repeated that what they wanted was an effluent which would not decompose offensively, but one which was in a condition to undergo those final and ultimate changes of purification without giving rise in the meantime to offence, and if they could leave their effluent in that stage they would have got something that was satisfactory.

Dr. BARWISE asked Dr. Butler if they had noticed growths on the bottles of their samples.

Dr. BUTLER said in most cases they did not get any growth on the sides of the bottles at all, or on the bottom: many of the samples they had were absolutely clear to this day. In any case it was quite inconceivable that algæ should appear within twelve hours of an analysis showing an increase of albuminoid ammonia, and disappear again in another twelve hours leaving no trace of their existence as disclosed by a subsequent analysis. Each term of an analysis was in a state of flux as the same effluent was followed from day to day, but the changes were not of a character to be accounted for by the growth of algæ. With regard to the question of anaërobic or aërobic action, a preference for one or the other, he would like to point out that neither Dr. Kenwood or himself had said anything definite as to whether anaërobic or aërobic treatment is best for the actual suspended matter in sewage; that was an open question at present and he doubted whether anyone was in a position to decide it; but with regard to the actual liquid parts of the sewage they did say there was no evidence to show that anaërobic action was necessary, and there was presumptive evidence that it did harm. Aërobic action was preferable. The object was to get an inoffensive effluent, and anaërobic action must increase the offensiveness without so far as they knew tending with any increased rapidity towards ultimate purification. In their view they were not to aim at this point—not to aim at producing an ultimately pure effluent because that would be pure water, but they were to aim at getting an effluent which was inoffensive, one that would not decompose, and the sooner this was effected the better. Dr. Rideal suggested that there were no aërobic organisms known which would effectually deal with cellulose, paper, fat, straw, and so forth. He was prepared to admit the fact, but he was not aware that it had been shown yet what the organisms are that throw into solution and attack cellulose, paper, and such things. He was not aware that any organisms had been isolated from sewage, whether aërobic or anaërobic, which were known to liquefy these substances. It was an open question and quite undecided by any facts which had come under their observation. It seemed to him a matter to be decided by future experiment. The separation of these solids and their disintegration, liquefaction, or dissolution scientifically observed, would aid in the solution of that part of the problem; but so far as the liquid part of the sewage was concerned, they were satisfied it should be passed on and dealt with as quickly as possible under aërobic conditions.

Dr. H. R. KENWOOD (London) also touched on some of the points which had been raised in the discussion, and which Dr. Butler had not dealt with. With regard to the changes in the effluents which had so puzzled them, he was bound to say that the explanations offered that evening did not satisfy him, and he remained puzzled. Anyone present could examine the samples which they had in the Public Health Laboratories at University College, there being some hundreds of them; and they would not find any evidence of algæ in a large proportion of them, even at the present time (the middle of April). He was certain that the growths of confervæ had nothing whatever to do with the varying figures of analyses which they had obtained, for the sufficient reason that the large majority of the effluents were entirely free from any such growths at the time of analysis. He agreed that such growths were capable of seriously affecting the albuminoid ammonia and oxygen absorbed figures, but he was surprised that the matter should have been raised in connection with the facts dealt with in their paper. Almost the whole of their experiments were conducted in the winter six months, a period in which algoid growths are conspicuous by their absence; and, as a matter of fact, the two illustrations of the varying results of daily analyses of the same effluent which are given in the paper, are selected from analyses performed towards the end of November and the commencement of December—a period of the year during which he could scarcely bring himself to believe that Drs. Rideal and Barwise, or Mr. Scott-Moncrieff, had experienced difficulties from the growth of algæ. Did these gentlemen, moreover, seriously suggest that algoid growths were capable of causing the *daily* fluctuations in albuminoid ammonia—fluctuations which one day lead to an increase and the next day to a decrease? Dr. Barwise had quoted an instance of an excellent water which, when kept in the sun for about a fortnight, would yield great increase in albuminoid ammonia owing to the confervoid growths on the sides of the bottle, and he holds that these growths are responsible for the "fatal and contradictory results" we obtained by keeping our samples under observation. What is the logical conclusion to all this? He advocates a limit of ·1 part per 100,000 of albuminoid ammonia, and he acknowledges that the estimate of this ammonia may be considerably affected by the growth of confervæ. Suppose then we took an effluent which shortly after its collection furnished ·1 of albuminoid ammonia, and allowed it to sweeten "in the sun for a fortnight," then the effluent, if analysed after its fortnight's further self-purification, might at certain times of the year show an increase in albuminoid ammonia due to algæ, and the "fatal result" would be that it would be pronounced unsatisfactory. They had been told by Dr. Barwise that when advising bodies of business men they must adopt certain arbitrary standards. He did not agree. He and Dr. Butler had advised many public bodies, but they did not say you must have your effluent come up to ·1 per 100,000, for the simple reason that they knew that they *might*, under certain circumstances, get an infinitely better effluent, which on analysis would furnish as much as ·4 part per 100,000. In conclusion Dr. Kenwood congratulated the meeting on the circumstance that some of the first authorities on this difficult subject had contributed to the discussion on the paper that evening, and both he and Dr. Butler thanked them for their criticisms. Such discussions materially aided in bringing us nearer to the goal they were all anxious to reach—the goal of a sound and scientific grasp of all the facts which are bound up in the subject of the biological purification of sewage.

THE CHAIRMAN proposed a vote of thanks to the authors of the paper, and said he had hoped they would have made some reference to the question of the influence of light on sewage and sewage effluents. Experiments he had made some years ago showed that nitrates were extraordinarily increased by being kept in the dark rather than in the light, and he believed that a great deal of the success of the so-called anaërobic treatment was due to the absence of light. With regard to standards he thought a sewage sample ought to be analysed directly it was received, and afterwards incubated for a number of hours and then analysed again, and if they all did that, at the same time reducing the samples to identical alkalinity, they might probably get more uniform results. He quite agreed that at present there was great confusion. He thought the process of Dr. Adeney and Mr. Parry seemed to have been much neglected in this country. One of the curious results of the discussion to him was the general agreement, possibly with one exception, that of the gentleman who referred to the chemical treatment of sewage—it was a pity that they had not this latter aspect of the matter more fully represented that evening—but so far as the discussion had gone on biological grounds the speakers were in general agreement, and he was certain that the report of the meeting would be found to be most interesting when it appeared in the Journal of the Institute.

Mr. S. R. LOWCOCK (Birmingham), who was unable to attend the meeting, sent the following remarks upon the subject of the paper:

I have read with much interest the copy of the above paper which you were good enough to send me, and in the main I quite agree with the authors' conclusions.

My experience is that except under very exceptional conditions it is cheaper and easier to produce an equally good effluent by means of sedimentation tanks and bacteria beds than by treatment on land, whether the sewage is applied in a crude state or previously strained or subjected to chemical treatment.

In addition, the biological process is far more certain in its results, can be kept under much better control, and can be more readily adapted to deal with the varying qualities of sewage which we are called upon to purify.

I have also found that it is essential to sediment the sewage before applying it to the beds, and I am very pleased to find that the authors' experiences confirm what I have long contended, that the best results are obtained from the intermittent application of sewage to at least two sets of beds, the outlet pipes of which are always open and which are thoroughly aërated, thus providing for the effluent in thin films being constantly in contact with air. This is aproaching what I consider the ideal of a system of sewage treatment, which is, a sedimentation tank and at least two sets of bacterial beds, followed by land on to which the nitrified effluent can be turned when required by the crops, and when not so required can be sent directly into the river without nuisance. I have not, however, been able to obtain, in permanent working, the purification of such large quantities per square yard per day as are given by the authors.

This circumstance may be accounted for by the difference in qualities of the sewages dealt with, but my experience is that in round figures we cannot rely upon dealing with more than from 200 to 250 gallons per square yard per day as a regular permanent average rate.

I have filters which have been at work for five years at these rates, and they are giving very excellent results, and show no signs of deterioration or loss of capacity.

The question of the total quantity per day is not the only question to be considered in connection with bacteria beds, as the question of *rate* is of very great importance.

A common method in making experiments is to take only a portion of the daily flow of sewage arriving at the outfall works and treat that by means of biological tanks or bacteria beds of various sorts, and thus the quantity treated and the rate of flow is practically uniform, so that, as in the example of the working of contact beds given by the authors, these can be worked with absolute regularity, being filled in say one hour, rested full for two hours, emptied in one hour, and allowed four hours aëration. When, however, the process comes to be applied to the whole of the sewage reaching the works, the conditions are entirely altered owing to the varying rates at which the sewage reaches the works at different periods of the day.

To reproduce the experimental conditions therefore would require a very large and costly tank to provide for storage and equalization of the flow, but the working of the beds, whether contact beds or intermittent aërating beds with open outlets, can be made practically uniform by working them in groups as shown by a set of diagrams which I communicated to the "Surveyor" in September last.

This is as I have already said, a point of great importance, and I am now superintending the construction of several sets of sedimentation tanks, contact beds, and aërating filters, each set being arranged on different lines, but in such a way that they will all be fed with the same sewage in the same proportional quantities as it comes down to the works, and I hope that in a few months the results of these experiments will be available.

I am glad to see that the authors have touched upon the question of distribution, as this is really the chief difficulty in connection with beds with open outlets. So far I have been able to get over the difficulty most satisfactorily by flushing the effluent on to the surface through flat troughs with perforated sides placed fairly close together.

Just one word as to a standard of purification: As the authors point out, the purification of sewage is a matter in which, to obtain really satisfactory results, the biologist, the chemist, the engineer, and the medical officer of health must work in cordial co-operation, but as far as a standard of purity goes the engineer has only to carry out the works necessary to produce an effluent, complying with the standard set up by the members of the other three professions.

There appears, however, to be very great difficulty in agreeing upon such a standard, or indeed upon the terms in which such a standard can be expressed.

If such could be agreed upon, and sufficient money be placed at the engineer's disposal, I do not think there would be any great difficulty in producing an effluent to meet the requirements.

I do not believe, however, that it is possible or even desirable to fix any hard and fast limit of impurity, as no two set of conditions are ever exactly similar, and a standard which would be very desirable in the case of a city or town discharging its effluent into a small stream, the waters of which are subsequently used for manufacturing or other purposes, would be quite unreasonable and unnecessary where the effluent is discharged into a large volume of water not subsequently used for any such purposes.

It appears to me that every individual case ought to be judged by itself, and a standard fixed in relation to the circumstances existing in such case, taking into consideration the quality and quantity of the sewage and the effect of the effluent on the stream into which it is discharged and the purposes for which the water in the stream is to be subsequently used, I therefore entirely agree with the authors' conclusions in this respect.