Variations in Stroke Waveform in a Laboratory Jig

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Report of discussion at October, 1964, General Meeting (Chairman: Professor J. C. Mitcheson, Vice-President). Paper published in June, 1964 (Transactions, *vol. 73, 1963–64*), *pp. 643–62*

Mr. D. G. Armstrong said that the idea of producing an experimental jig giving different kinds of stroke waveform had come to him many years before, but it was not until he had understood the potentialities of modern pneumatic equipment that he had been able to see the way to develop the kind of device he wanted. Ideas, the basic movement and its subsequent evolution had all taken quite a long time to develop. It was a part-time project which he had had to fit in when other duties allowed and, of course, they had not been working on it all the time. There had been two main stages: first of all, the development of the mechanism to do what they wanted and, secondly, the development, discovery or procurement of something that would indicate what they were doing. The equipment was still far from perfect and a number of improvements could be made in the light of experience.

With the possible exception of coal, the jigging process had been neglected, and he felt that development had been hindered by inadequate terminology. It was difficult to describe just what was meant in English, and in a number of translations, particularly from the German, descriptions were of very doubtful accuracy. There just did not seem to be the right words to describe events in the jigging process. For instance, he was still not happy about 'acceleration'. In the paper he had written (p. 644): 'Acceleration can also be either positive or negative, but negative acceleration is retardation and acceleration on the downward stroke is positive. Acceleration and retardation can both apply to either upward or downward strokes. Since acceleration is defined as rate of change of velocity, a rapid change from high velocity upward to high velocity downward involves high acceleration, but in this sense the word seems to be inadequate.'

Perhaps 'rate of reversal' might better describe that process. One could imagine a car travelling forward at 30 m.p.h., slowing down, coming to rest and then accelerating up to 30 m.p.h. in reverse. A word was wanted to describe the whole operation, and it was rather difficult to find. Obviously, acceleration would not do.

'Bed' was a much misused word. It seemed to cover everything from ragging to the heavy mineral lying on top of the ragging and the whole contents of a cell. They all knew what ragging was. He had referred to the ragging plus heavy mineral overlying it as a bed, but there was the lighter mineral overlying that which needed a name, and he had called it the 'cover'. Although not very satisfactory, he could not think of a better word for it. If they were to talk of the 'bed' and the 'cover', what were they to call the entire contents of the cell? He had called it the 'charge'—again a word he did not like—but one had to have words to explain the process adequately.

Development had also been hindered by difficulty in measuring. It was a problem to measure either the water movement or the mineral movement in a jig. He wanted to make it clear that 'waveform' in the paper referred to water movement, not mineral movement. It was not displacement so much as velocity that required study and velocity diagrams were of the greatest importance.

Quite recently at the New York Congress, Mayer had presented another paper.* The best kind of stroke recommended by Mayer, according to his new theory, was exactly the opposite of what he (Mr. Armstrong) had found to be the best, and it would be interesting to discuss that point. Mr. K. S. Blaskett (personal communication) had said that he was surprised that there had been no reference to the work by Kirchberg and Hentzschell.† That had been an interesting paper, but when someone had asked what the waveform had been Professor Kirchberg had replied that it was a simple harmonic stroke, and had made no further comment. That seemed to be rather like studying flotation without realizing that one could change the quantities of reagent. To develop a new theory without accurately specifying the kind of stroke was most unsatisfactory.

There had been two stages in operation. First of all he had set out to see whether there was any significant difference in results produced by the different kinds of stroke and then, if there were a difference, he proposed to find out which kind of stroke was best and why. If the primary stage had shown that there was no difference there would have been no point in continuing the work, but preliminary results had shown clearly that the different kinds of stroke produced quite different results—one had only to look at the jig in operation to see how a change in waveform affected results. That opened up the way for a very big programme indeed, but one which he had never been able to complete.

He had to emphasize that the kind of information usually given in connection with jigging was not enough. The tests showed very large differences in results obtained with different kinds of stroke with the same feed at the same feed rate, the same feed water and hutch water, the same ragging of the same depth, and the same overall stroke length and frequency. Those were the data usually given when talking of jigging, but they could be identical in two separate cases, except for waveform, and yet results would be entirely different. One therefore had to specify the kinds of stroke and its waveform.

[The speaker then showed slides of displacement, velocity and acceleration diagrams for a simple harmonic and a uniform velocity stroke; Fig 5 (p. 650) (with an explanation of the working of the mechanism); the pneumatic valves; and two photographs of the complete jig at an earlier stage of development.]

*Mayer, F. W. Fundamentals of a potential theory of the jigging process. *VII International mineral processing congress* (New York: Gordon and Breach, 1964), 87–94.

†Kirchberg, H. and Hentzschell, W. A study of the behaviour of particles in jigging. *Pap. Int. Mineral Dress. Congr., Stockholm*, 1957, no. 11 : 3, 193–215.

The author said that he had some Vibrograph strips which showed the stroke waveform. That instrument seemed to be the only one which could do the job satisfactorily. It contained a reel of waxed paper which travelled through it at constant speed and a stylus which scraped a line on the wax, giving a trace of the stroke.

He realized that as he had not been able to continue the work the results in the paper were too brief, but he thought it clear that the subject was well worth further study on the lines indicated. He thought that the results, incomplete though they were, were worth putting on record and continuation of the work might show a way to increasing the capacity and efficiency of jigging.

Mr. D. J. Ottley said that the paper provided a great deal of useful information to both jig manufacturers and operators. It clearly confirmed the complexity of the jigging and suggested lines for further investigations. It was unfortunate that the work could not be continued. Any further studies could be directed either towards the design and development of an industrial-type jig, using the very flexible pneumatic system for the control of the waveform, stroke and frequency, or using the laboratory test assembly with modifications, to study basic aspects of jigging especially the interrelationship, for example, between waveform, bed depth and cover, feed rate and feed characteristics.

Were it possible to incorporate a flexible pneumatic system, it should be possible to design and construct a jig to suit a particular material or optimum operating conditions. That presumably could be achieved conveniently and cheaply. Most mineral treatment equipment at the moment was standard and usually not very flexible either in design or operation.

Laboratory jigs were usually scaled down and very much simplified versions of the full-size machine with but limited means of controlling and measuring the parameters used or of observing what took place. The author's design demonstrated what could be done there and what benefits could be derived from having a more sophisticated laboratory machine. It was unfortunate that manufacturers were not interested in developing and making more useful laboratory machines for industrial laboratories and pilot plants.

The author had pointed out very clearly the ambiguities existing in jigging. There was obviously an urgent need for a comprehensive glossary of terms used in the whole of the mineral proceedings and related disciplines and he would like to see the Institution initiate or support the preparation of such a comprehensive work.

He doubted whether the laboratory jig was a realistic scale-down of a full-size diaphragm-type jig. Standard Bendelari jigs, for example, were between 26 in. by 26 in. and 42 in. by 42 in., with depths of bed between 3 in. and 6 in. The laboratory jig was geometrically dissimilar to the

Bendelari jig and it might thus be unwise to compare the laboratory jig performance with what one might expect from an industrial-size machine.

On page 652 laboratory jig feed rates were given as equivalent to about 3·8 t/24 h/ft² of jig area and were stated to be similar to those used in practice. He said that he had found a large number of examples where the feed rate was not similar to that used in commercial practice. Feed rates of between 20 t/24 h/ft² were more common for Bendelari jigs.

On page 654 the author mentioned that mineral movement was the most important factor in jigging, both for stratification and for subsequent removal of the products. Thus it did not follow that waveform was necessarily the controlling factor. One could change the screen area, the type of screen and the percentage of open area and get different performance without altering the actual waveform.

The duration of the tests, reported on pages 652 and 657 was between 10 and 15 min. From Fig. 7 (p. 660) it was noticed that bed equilibrium was not reached even after 24 min. It would appear that all the tests had been run under non-equilibrium conditions, which made any interpretation of the results very difficult and misleading. In Table I (p. 653) values reported for recovery did not mean very much because what had been recovered might have come from the new sample fed in or from a previous sample because of the building up or depletion of values in the jig bed.

In view of the improved control which the laboratory jig offered, an interesting programme of test work might be directed to determining whether different waveforms and other influential parameters could be selected to treat particles finer than 150 mesh B.S.S. efficiently, which would be particularly valuable and interesting to those treating alluvial tin ores. Another prospect was whether the jig could separate materials of a density difference of 0·5 or less.

It would be very interesting to establish the influence of waveform on jig performance for feeds of a wider size distribution than those reported in the paper. In the preliminary test results shown in Table I, were the sizes of particles recovered in the concentrate, or lost in the bed or tailing, determined? Harris* had reported the phenomenon of losses of a particular particle size. Could waveform be used to explain or overcome the loss of a particular size of valuable mineral ?

He hoped that the results reported would stimulate others sufficiently to continue the work, with the objective of ultimately developing an acceptable theory of jigging.

Mr. F. B. Michell said that he was a little doubtful as to the value of the term 'true bed'. To use the author's terminology, in some cases there was no distinction between the 'bed' of heavy mineral resting on top of the ragging and the remainder of the 'body', 'charge' or 'cover'. If one

*Harris, J. H. Serial gravity concentration: a new tool in mineral processing. *Trans. Instn Min. Metall.*, **69**, 1959–60 (*Bull. Instn Min. Metall.* no. 637, Dec. 1959), 85–94.

examined the charge in a rougher jig operating in the alluvial field one found no bed in the sense used in the paper. The change in character was gradual and it had been shown that most of the separation penetration of the high specific gravity minerals occurred between the particles in the upper part, simply because there was no true lower part other than the ragging. There was an upper mobile bed which had a certain amount of transverse flow and also a lower zone of some graduated specific gravities and of varying dilation because it was not a homogeneous bed. Examination of successive layers in a commercial jig showed that there was no appreciable accumulation of high specific gravity minerals on the ragging.

A lot of conflicting statements had been made about jigging. In Taggart's *Elements of ore dressing** one read that with a material finer than 14 mesh, dilation started at the top—a statement which he himself had never understood because he had never seen a bed dilate from the top. It always dilated from the bottom, except in the extreme case of very coarse material, with a layer of very light fine material above it. If an excessive stroke were used in such conditions enough velocity was created to disturb the upper part of the charge and he supposed that it might then be said to dilate from the top. He would like to hear the author's views on that subject.

He was glad that the author had drawn attention to the question of the ratio between the screen area and the area of the diaphragm or plunger because frequently one saw figures quoted of stroke length for a given purpose, but nobody had taken the trouble to point out that the plunger or diaphragm area was not the same as the screen area or to give the diaphragm/screen area ratio, which could cause a considerable difference. Not only was that so, but jigs had been made which, if operated at a short stroke, tended to boil in the middle while around the periphery dead spots could be seen. That was usually because the ratio between the diaphragm and the screen area was too great, and the distance between the diaphragm and the bottom of the screen provided insufficient space for the water to take up an even flow, with the result that all sorts of currents were present.

The author had also drawn attention to the importance of lowering the initial velocity on the upstroke. That should lift the particles *en masse* and then provide enough space for adequate dilation as the upstroke approached the top of the curve. The separation then took place at the end of the upstroke and during the first part of the downstroke. If the upstroke accelerated too rapidly at the beginning, it tended to lift small high specific gravity minerals in the interstices before the lower part of the 'charge' started to open. Later in the cycle the lower part of the bed was starting to open; therefore the interstices were larger and an increase in the velocity of the rising water could then be tolerated.

He had to admit that the work at Camborne in the last two years had always been done using a simple eccentric, but the state and duration of the dilation appeared to be the major factor affecting the penetration of the dense particles. The degree of dilation varied during the cycle: with the

*Taggart, E. S. *Elements of ore dressing* (New York: John Wiley, 1951), p. 203.

greatest dilation the larger particles penetrated rapidly, but as the dilation decreased the interstices were reduced in size and the smaller high specific gravity particles penetrated relatively unhindered. One could, in fact, liken the interstices to a rubber tube that was expanding and contracting; when it was expanded, the larger particles passed more rapidly and the smaller ones when it was contracted. The difficulty was to get the expansion and contraction of the 'tube' to be of the right duration to fit the size ratio of all the particles to be concentrated.

In the experiments described the time interval for the upstroke and the downstroke appeared to be almost equal and one might expect an advantage in increasing the time taken for dilation. If the dilation time could be increased without extending the lifting time, a greater frequency should be possible for any given amplitude. That, he knew, led to the very practical question of whether it was easier to provide more screen area or a more complicated mechanism to get the same throughput.

The author appeared to connect the rate of concentrate withdrawal with the depth of the 'bed' and the feed rate, but surely it depended on the proportion of 'concentrate' in the feed. With a small proportion, such as was found in most alluvials, the additional amount of high specific gravity mineral with increased feed rate was small, and the 'bed'—if there were any—did not not increase appreciably. If one watched such a machine it was perfectly obvious that if the load were increased by increasing the feed rate, the total depth of charge increased. Furthermore, the slope between the feed point and the tailboard also increased and, since the amplitude remained constant, the dilation was decreased. If the amplitude were increased the bed would become completely dilated again. That rather supported the author's suggestion that the depth of the screen was a basic parameter.

He would also like clarification of a point in connection with the hutch water. The author stated (p. 654): 'A constant flow [of cell or hutch water] will displace the base line in the velocity diagram', but it seemed to him that neither was a constant volume of water admitted by a simple orifice nor was there a constant withdrawal from a spigot discharge, since both depended on the difference in pressure across the orifice at any given point in the cycle. Only when the water pressure was very high did one get a more or less constant inflow of water. If the water pressure were comparatively low there was a varying amount of inflow.

In conclusion he felt that there was a need for a great deal more research to follow up the author's work on the effects of waveform.

Mr. J. H. Harris said the ability to observe the effect of change of waveform on concentration performance so readily was a useful advance in experimental technique. It was possible that the performance characteristics of different types of jig varied owing to the stroke waveforms introduced by the design. Such factors as the method of introduction of hutch water and the inertia of the pulsating mechanisms would have an effect on rate of reversal. In some large jigs the actual flexing of the body of the jig during operation would also affect the waveform.

From the results presented it appeared that variation of waveform affected (*a*) the rate at which the experimental jig recovered concentrate; (*b*) the venue of the concentrate, i.e. on or through the screen; and (*c*) the grade of concentrate. The author pointed out that the results shown in Table I did not necessarily indicate that the *AR* stroke was superior to the *RA* stroke. They showed that the *AR* stroke resulted in recovery of the concentrate in the hutch while the *RA* stroke collected more in the bed. The *RA* stroke appeared to give the better grade of concentrate, as shown in Table III (p. 658).

	Tests 64 and 69 AR	*Tests 70 and 71 RA*
Average heavies in feed	1580	1596
" " " concentrate	1381	871
" " " tail	205	471
" " retained in bed	− 6	+ 254
" grade of heavies, %	78·9	98·7
Loss in tail as percentage of feed	13	29·5
Recovery in hutch + bed, %	87	70·5

The summary of Table III showed the retention of heavies in the bed in the *RA* tests. Since none of the concentrate collected in the bed was drawn off through a gate, some might have been carried over mechanically and contributed to the loss in the tail. The total potential recovery in both cases might therefore not be too dissimilar.

The comparison with the Bird and Mayer cycles was interesting. The former investigators were concerned with coal washing where, perhaps, 75 per cent of the feed would also be light and 25 per cent heavy, as in the present investigation. The 'light', however, would be the values (coal), required to report in the overflow, whereas the heavies would be tailing, required to report in the bed, i.e. the reverse of the requirement for heavy mineral concentration.

Bird and Mayer had somewhat different approaches and it seemed that Bird was satisfied with the *AR* stroke, which gave quick stratification and hence enhanced machine capacity, while Mayer preferred the *RA* stroke, which gave a cleaner separation.

On the other hand, Mayer did suggest* that it would be preferable to reach the plateau in his displacement time curve by a series of short pulses so that he too, arrived at an average waveform very similar to *AR*. It seemed possible that the *RA* stroke may be preferable when jigging coarse material for recovery 'over the screen', provided that the mineral was effectively withdrawn through a gate.

At the same time, the *AR* stroke appeared to give quicker stratification with the sizes tested and certainly gave a quicker recovery 'through the screen', although at a lower grade. Hence the *AR* stroke might be preferable for the recovery of fine-grained mineral, especially when present in low percentages in the feed.

*See reference 4 on page 662 of the paper and footnote reference on page 144.

Mr. I. R. M. Chaston said that the author had emphasized the dual function of a jig and pointed out that the primary function of a jig was stratification and the secondary function was the removal of the stratified mineral. He then, however, judged the effectiveness of his waveforms by assessing the performance of the jig in that secondary function. Surely the most important function of a jig was to produce stratification and the true measure of the effectiveness of a jigging operation was the speed with which stratification was produced and the success with which that stratification was maintained. As Mr. Harris had pointed out, one of the waveforms considered in the paper had produced a concentrate on the screen and the other waveform had brought that concentrate through the screen. In some circumstances the first result, using a gate and dam for concentrate removal, might be preferable to the second. It was, however, not possible to tell from the results given in the paper which of the waveforms had actually resulted in the jig performing its primary function most satisfactorily and which, therefore, was really the best waveform to use in jigging.

So far as the operation of jigging was concerned, one part of the jigging cycle which had received a great deal of study in another field was the dilation part of the stroke which was allied to the concept of fluidization. Fluidized beds had been the subject of a great deal of published work—some of which could well be considered in relation to jigging. The author had used grids above and below the screen to minimize turbulence and cross-currents below the screen and to prevent migration of the ragging above the screen. From his own experience with many forms of jig the speaker had come to the conclusion that that form of grid played a more important role than just holding the ragging in place, and that view was upheld by a paper on fluidization by Hall and Crumley.* They had investigated the effect of baffles in a deep fluidized bed and had found that segregation was promoted by the baffles and also that the presence of baffles enabled the bed to become properly fluidized at low gas-flow rates. Without baffles the bed tended to 'slug' violently, i.e. the whole bed would lift instead of dilating quietly. Such 'slugging' had been described in the textbooks as the normal effect of the rising current on a jig bed, but in his opinion it was undesirable and better jig performance could be achieved by installing baffles in the jig to prevent the 'slugging' and to promote the smooth dilation of the jig bed during the upstroke of the jig.

The effect of baffles on a fluidized bed of used catalyst of varying specific gravity was apparent from the tabular matter (p. 151) taken from the paper by Hall and Crumley.

It could be seen that where there were no baffles mixing in the bed was good and virtually no segregation took place, but the presence of the baffles was effective in promoting considerable segregation. It should be noted that the catalyst was − 72 mesh, compared with the 10-mesh screen baffles.

*Hall, C. C., and Crumley, P. Some observations on fluidization as applied to the Fischer-Tropsch process. *J. Appl. Chem. (Lond.), 2nd Suppl. Issue* **1**, 1952, S47–S55.

Type of baffle	*None*	*10 mesh steel gauze*			*Brass-strip cross-piece*
Distance apart, in.	–	6	4	2	2
Bulk density, g/ml					
Top of bed	0·88	0·86	0·76	0·67	0·92
Centre of bed	0·91	0·91	0·90	0·84	0·96
Bottom of bed	0·80	0·07	0·98	1·22	1·02
Max. difference	0·03	0·21	0·22	0·55	0·10

Fig. 2 on page S53 of the paper by Hall and Crumley showed the size and density distribution in such a baffled bed. The fresh catalyst was of uniform density and only showed segregation by size. The used catalyst, however, showed segregation mainly by density with coarse and fine low-density particles being held up in the bed.

Those fluidized conditions only represented part of the jigging cycle, but it would be of great interest to develop a jig along those lines since they would seem to result in good segregation. Such a jig would have a 'charge' several feet deep instead of the few inches usual in present-day jigs. It would have several horizontal screen baffles set in the 'charge' to prevent re-entrainment of segregated material, which apparently occurred in the present form of jig.

It was common knowledge that the bulk of the recovery in a jig was made in the first cell and it seemed probable that segregation in the jig actually occurred within the first few inches of the feed point and that the subsequent length of the jig was solely concerned with re-segregating mineral which had been swept out of the preliminary segregated layer by eddy currents in the jig. By preventing that desegregation, and by designing the the jigging operation to give the maximum rate of segregation to start with, it should be possible to increase the capacity of a given size of jig greatly. The point might be reached where tin dredges would only need one or two of those deep primary jigs in place of the large number of the present type of jig used on dredges. That could lead to a much reduced water consumption on the dredge and to a much reduced size and cost in building new dredges.

Mr. Armstrong said that he would deal with the questions posed during the discussion in writing at a later date.

WRITTEN CONTRIBUTION

Mr. D. M. Rowe*: The paper deals with research which is closely related to work that I am engaged on at Bristol and, on the whole, I am able to confirm the results obtained on the effect of water stroke and consequent bed stroke waveform on the jigging process. There are, however, a few points which may be of interest to the discussion.

*H. H. Wills Physics Laboratory, University of Bristol.

It seems to me that clarity should not be sacrificed for brevity and in my nomenclature the insertion, as appropriate, of water, plunger or bed, before upstroke or downstroke removes any ambiguities. From my own results I am able to verify that the water stroke waveform is important, but not necessarily *most* important. In Table I (p. 653), while examining the effect of water stroke waveform on performance, and again in Fig. 4 (pp. 648–9), no mention is made of the frequency. I raise this point because I do not think that performances can be compared unless one standardizes the frequency.

I have carried out experiments similar to those of the author using a 'pulsed dense column'. This consists of a parallel-sided column containing the charge. It is subjected to a slow upward flow of water, which controls the degree of compaction of the bed, and a pulsating flow which passes through the perforated base. Its action differs from the normal jig in that visual dilation and recompaction is apparent only along a small fraction of the charge depth ($< 1/10$). The 'pulsed dense column', due to its extreme depth, is essentially a batch device and little can be said of its commercial prospects without further investigation with the model.

The results I have obtained indicate that an accelerated water upstroke followed by a retarded water downstroke is preferred. Jig performances, however, cannot be evaluated or compared without due consideration being paid to the bed stroke length and the frequency of the bed pulsations, which is intimately related to the size of particle being jigged.

No reference has been made to the examination by Rafales-Lamarka* of dimensionally various mathematical relations which he obtained in connection with a simplified model of a jig. He then went on to show that providing conditions of dynamic simularity exist between the laboratory model and its commercial counterpart (predictions arrived at for this model can be applied to the other). He also goes a long way in appreciating the significance of various parameters such as particle size, frequency and amplitude of water pulsation. As the present author points out, the jigging process is indebted to variations in water velocity, but attention must be paid to a quantity, the jigging parameter, defined by Rafales-Lamarka. In it the importance of optimum water pulse frequence for correct performance of the jig, i.e. separation according to specific gravity, is stressed. Ignorance of this parameter can lead to separation according to size, and in extreme cases reversal in size gradation. Although not pointed out by Rafales-Lamarka, further examination of the jigging parameter reveals that his results are applicable for any waveform. We can thus extend Rafales-Lamarka's theoretical arguments to include the results obtained by Armstrong when utilizing a non-sinusoidal water waveform.

From Fig. 4(*a*) (p. 648) the water pulse retardation is about 3 g, which is much more than the downward force normally encountered. Would this result, when using a natural feed, in particles which normally remain in

*Rafales-Lamarka, E. E. The hydrodynamics of gravity concentration processes and the modelling of these processes. *Sb. Trudov Akad. Nauk URSR, Lab. Hidravlichnykh Mashyn.*, no. 6 1956, 142–58. (Russian text.) English transl. No. 61–13984. (Nov. 1960, 24 p.), available from Library of Congress.

suspension and pass out with the overflow water undesirably passing down through the ragging?

I think that frequency should be added to the already disclosed important parameters—depth, screen depth and waveform—and would be interested to hear why the particular value of 100 strokes/min was taken as standard. Perhaps the author would also elaborate on his suggestion that the results indicate the effect in the case of alluvials.

As stated, I feel that the study of the waveform as an entity in itself will not add to the understanding of the phenomena involved although, as the author points out, its effects are very noticeable. The waveform is only one of a number of inter-related and inter-dependent parameters and, without a mathematical theory predicting their behaviour, extrapolation or attempts at predicting the jig's performance, remains purely speculative.

In concluding, I would like to congratulate the author on the ingenious design of his reciprocating mechanism, with its close control on waveforms. That the author is no longer in a position to continue his research is a great pity, especially as the problem of constructing the apparatus has been overcome while the accumulation and explanation of results remain hardly touched.

Theory of Thickener Design Based on Batch Sedimentation Tests

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Report of discussion at October, 1964, General Meeting (Chairman: Professor J. C. Mitcheson, Vice-President). Paper published in July, 1964 (Transactions, *vol. 73, 1963–64), pp. 729–59*

Mr. A. G. Moncrieff said that in presenting his paper he realized that he was inviting considerable criticism. He had attempted to make a critical analysis of existing theories on thickening and had suggested that they needed revision. He had, however, done that without providing any new experimental evidence. He appreciated the dangers of so doing and thanked the Institution for publishing the paper.

Many useful papers had been published on the subject, including several since his own paper had been written. One of those recent papers*

*Robins, W. H. M. The theory of the design and operation of settling tanks. *Trans. Instn Chem. Engrs*, **42,** May 1964, T158–63.