Investigation and Development of Some Laboratory Wet Gravity Mineral Concentrators

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Report of discussion at June, 1962, General Meeting (Chairman: Mr. J. B. Simpson, President). Paper published in April, 1962, pp. 379–92

Mr. L. D. Muller, introducing the paper, said that he wished to thank the Institution for acceding to the authors' request that the paper, originally scheduled for written discussion only, should be given the opportunity of direct presentation. The reason for that request arose from the fact that during the discussion on the speaker's paper on the micropanner* a member, Mr. C. C. Dell (then with the National Coal Board), had suggested that the use of a ceramic porous deck on the micropanner might enable the bed of particles to be subjected to a *continuous* vertical flow of water with possible beneficial effects.† In reply the speaker had indicated that a continuous upward flow would most likely inhibit stratification and prevent the heavy minerals from making essential contact with the reciprocating deck. However, it had eventually occurred to the authors that a pulsed vertical flow, in place of the continuous one, might well prove advantageous by accentuating the jigging action in normal processes of panning. Thus had been initiated one of the major aspects of the present paper, and he felt sure that once again they would benefit from the help of members present.

Turning to the paper itself, it was, in a sense, an interim report, and much still remained to be done in attempting to apply its findings to full-scale commercial equipment operating on a continuous basis. There were one or two points in the paper that should be emphasized. It had already become apparent that some confusion might well arise from the author's definition of the longitudinal directions of motion of the table and panner decks. With the Wilfley type of concentrating table it was generally accepted that with respect to the reciprocating action of the deck the forward direction or stroke was that direction *away* from the head motion—in other words, the direction in which the concentrate particles were moved; the return stroke was the reverse of this. With panners incorporating a bump in their cycles of operation—such as were described in the paper—the concentrate particles moved *towards* the head motion. If those conventions had been rigidly adhered to it would have meant discussing two forward strokes (and, for that matter, two return strokes) each respectively

*Muller, L. D. The micropanner—an apparatus for the gravity concentration of small quantities of materials. *Trans. Instn Min. Metall., Lond.*, **68,** 1958 59 (*Bull. Instn Min. Metall., Lond.*, no. 623, Oct. 1958), 1–7.

†*Trans. Instn Min. Metall., Lond.*, **68,** 1958 59 (*Bull. Instn Min. Metall., Lond.*, no. 625, Dec. 1958), 95–100.

operating in opposite directions, and confusion would have resulted. The table convention had for that reason been adopted and had also been applied to the panners when describing their stroke directions.

A further small point of difference existed as between tables and panners. With Wilfley-type tables, in any one deck cycle a concentrate particle in contact with the deck received only *one* impulse in the forward direction; that occurred when the deck was sharply reversed into the return stroke. With panners such a particle normally received *two* impulses per cycle in the required direction, first when the deck was sharply arrested at the end of the return stroke, and then, immediately afterwards, as the deck was abruptly reversed into the start of the forward stroke.

Dr. Robinson had already pointed out that the comparative tests described in the paper might well be regarded as incomplete. The test (the results of which were illustrated graphically in Fig. 11, page 389) empirically compared the concentrating efficiency, or performance, as between a laboratory-size Wilfley table, a standard Haultain superpanner, a macropanner—its design being based on that of the superpanner, but incorporating a new design of head motion—and finally the pulsepanner. The latter was the macropanner with a pulsed deck fitted in place of the standard lino-covered deck. The further comparative test that had been suggested was that of the superpanner fitted with a pulsed deck, and that test had now been carried out using the same carbonatite rock previously used as the test material. The results, shown (Fig. A) superimposed on a reproduction of Fig. 11, were very much as might have been expected, and emphasized once again the large increase in performance due to the pulsed deck. By inference that result also confirmed the advantages deriving

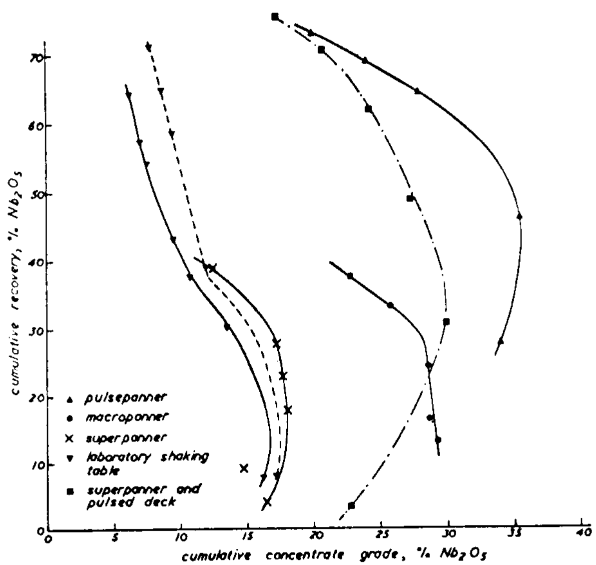

Fig. A.—Recovery of niobium values from carbonatite rock.

from the use of the new head motion, for the superpanner, even when fitted with the pulsed deck, still had not achieved the same performance as the pulsepanner with the new head motion.

Finally, the speaker wished to record an error in Fig. 8, Plate II, wherein the linoleum-covered deck was shown mounted the wrong way round in relation to the macropanner head motion. The pulsed deck illustrated at the foot of the macropanner, was, however, shown in its correct position relative to the head motion of the macropanner.

Mr. J. H. Pownall, adding to Mr. Muller's introductory remarks, said he would like to comment on some other specific aspects of the paper. The first related to the principle of pulsepanning: the pulsed-deck concentrator was attractive because it offered the prospect of development in order to obtain the best features of two types of gravity concentrator. The jig provided an effective method of achieving good vertical stratification of mineral particles. However, in the majority of the designs lateral separation occurring in the jig was dependent upon displacement by new feed entering the system. He did not consider that a very satisfactory arrangement, nor was it very readily controllable. In the case of the shaking table, control over lateral separation was good; however, vertical separation was not so easily controlled since it was dependent upon a number of factors, such as the frictional forces between fluids and particles, and vertical currents induced by riffling and perhaps by Bagnold forces. The separation of particles by an ideal concentrator should comprise two controllable separate mechanisms: a vertical movement followed by lateral separation. The new design offered control of both vertical and lateral particle movements.

One problem had arisen with regard to the pulsepanner as so far developed, namely that the lateral motion did induce a certain amount of uncontrolled vertical stratification as distinct from the pulse provided by the pumping mechanism. However, the authors considered that it was possible to reduce that haphazard effect and to retain control by manipulation of the variables of the pulsing system. That was a fairly naïve picture of what should go on in a gravity concentrator, but, to him, it was quite an attractive one.

The second point to which he would refer was the technique for investigating the motions of the various concentrators. That technique was not particularly novel; it had received a good deal of attention in the field of mechanical engineering, particularly in investigating the motions of aircraft structures, but at the time when the authors carried out their work they had not been aware of its application to mineral engineering problems. Since then he understood that a rather similar technique had been employed at Nottingham University by Dr. Whitmore in investigating the motions of jig mechanisms, and he thought the technique, which had been described in some detail in the paper, was worthy of mention because it offered attractive possibilities in studying a number of pieces of mineral processing equipment.

In conclusion, Mr. Pownall said he would like to thank those members who had shown interest in the development of the equipment, particularly in respect to the pulsed concentrator. The research laboratory found such encouragement very valuable in assessing the desirability of pursuing a particular line of development work.

Dr. C. R. Burch, opening the discussion, said he would like to speak about the paper from differing points of view. As a physicist, he would ask the authors what was the maximum acceleration associated with the knock of the superpanner. It seemed to be about $4g$, possibly $10g$, but that could be estimated more accurately from the original oscillograms. The relevance of that point was that if fluid of kinematic viscosity ν, having in it a particle of diameter D, were accelerated with acceleration A, then the quantity D^3A/ν^2 was a pure number. It was the acceleration analogue of Reynolds number. It told one (to put it simply) whether the particle on being accelerated would think it was a slime particle or a sand particle. If the number were very small it would think it was a slime particle and behave accordingly; if the number were large it would think it was a sand particle and behave like one. He could not calculate the transition range of the acceleration Reynolds number but he would take it as the order of unity. If that were so, the transition acceleration for 10-μ particles in water was $100\ g$. He supposed that, broadly speaking, that was why desliming cyclones used centrifugal accelerations of the order of $100g$. Separation on the pulsepanner would not stop at the transition range; he would expect it to be markedly slower when the number was small than when it was large.

Speaking as an old-timer in mineral dressing, the paper brought to his mind the cynical and slightly bitter comment of a Cornish friend on the Haultain superpanner. He had said, 'It does in half-an-hour what I can do in two minutes by hand with a vanning shovel!' The speaker had wondered to what extent, if at all, that was true of the quartz/riebeckite separation, and he had attempted to find out in an experiment with riebeckite granite. He had armed himself with two vanning shovels and six glass pots. At the end of two hours he was still vanning out riebeckite and he was bound to admit that the authors' pulsepanner did in ten or fifteen minutes what he could not do properly in two hours. He had recovered roughly one-third of the riebeckite—only one-third in two hours!—and he had got it to 65 per cent grade. The pulsepanner, making the same recovery, had achieved 90 per cent grade and if it were set to make the same grade it made 80 per cent recovery. The idea that one could do it as well by hand with a vanning shovel was complete nonsense.

In conclusion, Dr. Burch said he would like to speak about the future. It was quite clear that the pulsepanner would be a valuable tool in any tin-mill assay office. It should end the present fantastic position where they had to calcine slime before vanning it since it was too difficult to van it without calcining, and was too long a process, although the free tin recovered by the burnt vanning assay was less than half the high-grade free tin recoverable by mechanized vanning without burning in the laboratory. It would therefore help the mill superintendent to find out what he really wanted to know—namely, the free high-grade tin in his tails. That work would not, of course, be limited to laboratory analytical devices. There should certainly be a tonnage application, and one could picture several variants of it. It might be possible to get secondary circulation to carry the end layer of heavies away by methods not involving a knock. He believed there was a method (if one pulsed the deck in patches) of developing steady secondary circulation to take the concentrate out on a stationary deck.

He mentioned those aspects only to illustrate that the authors had opened up a large new field of investigation which would take some years to explore. It was really important that facilities should be available for full exploration to be carried out, and in the field of industrial development it was necessary to learn to think in terms of the long view. Instead of being asked, 'Can you straight away make a tonnage concentrator which will beat a table?', the authors should be allowed to follow their own bent and in, say, five years' time they could then be asked, 'What is it going to lead to in another five years' time?' Backing on such a scale would, he was sure, not be regretted.

Mr. H. N. Blyth* said that the efficiency of a shaking table was governed by the propulsive force that the deck could apply to the ore. The further the ore could be caused to travel along the deck at the end of each forward stroke, the better the separation and also the greater the rate of feed that the table would accept at any given frequency. The arrest at the end of the forward stroke could never be too abrupt, so a bumping action must be the best. The early tables of 100 years ago used that principle and he would be interested if any member could explain why it had been abandoned.

The arrest at the end of the back stroke had to be slow enough to ensure that the ore retained its position on the deck. If there were any retrograde movement, even a small one, the separation would be ruined. If the authors' excellent device of pulsing water through a porous deck were applied to a shaking table, it might mean that the ore could be pinned to the deck at the end of the back stroke, thus allowing a higher amplitude and frequency to be used.

He would suggest, however, that precautions were needed. With the early tanker lorries, when they were half full, the fluid banked up at the rear end of the tank while the lorry was running, but if the brakes were suddenly applied, the fluid charged forward, overcoming the effect of the brakes. Similarly, with a porous deck full of water, there would be a tendency at each end of a stroke to eject the water. Strong suction would therefore be needed at the end of the back stroke, or perhaps throughout the back stroke. Pulsion, however, should occur only at the end of the forward stroke, when the particles had acquired momentum. If a bumping action were used, it would probably provide that pulsion.

For their panners, the authors had used a 3-lobe cam. He suggested that a single cam would be better, partly because it would save making three

*Department of Mining and Mineral Technology, Royal School of Mines, London.

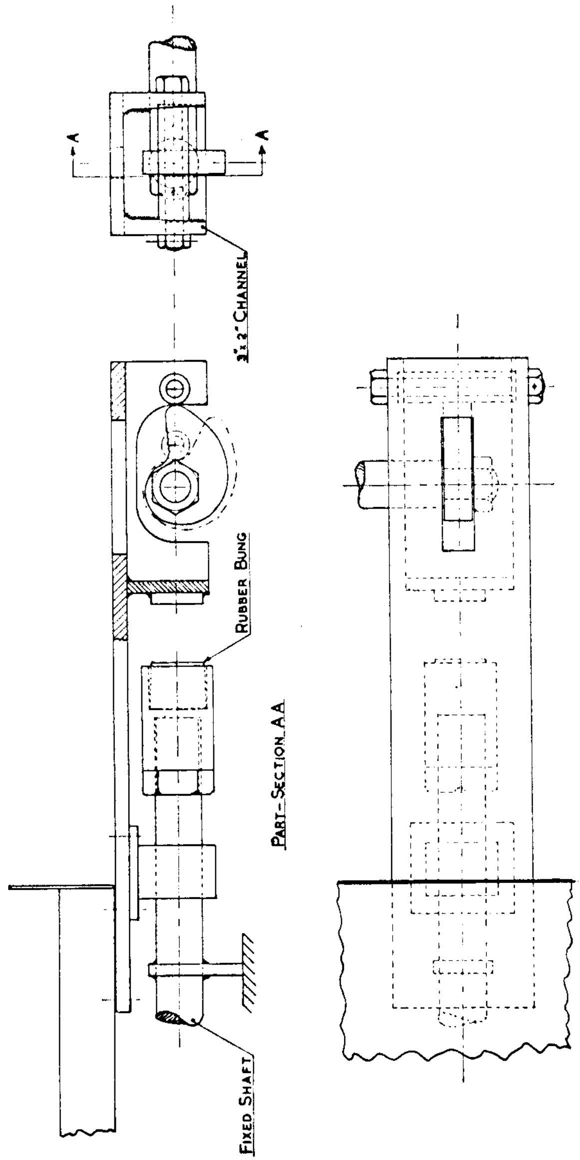

Fig B.—Bumping table head motion.

replicate profiles, also because it would enable the shaft to run three times as fast, resulting in a simpler drive, with pulleys.

As illustrated in Fig. B, there was room for a 14-in diameter pulley on the cam shaft, which would give a useful gyro effect to steady the whole machine. It was important that the head motion should be steady since, if it were not, the calculated movement would not be imparted to the deck because the head motion itself was moving.

The shaft carrying the cam remained horizontal. The tilt of a table deck seldom reached and never exceeded 5°. That amount of tilt on the follower roller would not matter as the cam would act on it in the same way.

The gap between the rubber bung and the boss on the end of the follower box would be so adjusted that the roller would never reach the position shown on the drawing, because the reversal would be effected by the rubber bung. The roller would meet the cam somewhere between a quarter and three-quarters of a revolution after the moment of release, depending on the amplitude.

An air-cylinder, made on the principle of the Westinghouse vacuum brake, that was to say without a piston, could take the place of a spring for the return stroke. The pressure in the cylinder would control the violence of the blow.

He congratulated the authors on the device they had used to follow the motion of the deck. For any motion other than a bumping motion, a useful graph could be drawn. The periphery of the fly-wheel or the driving pulley could be divided into a series of equal arcs. Supposing there were 24 arcs, then each would subtend an angle of 15°. If a clock gauge were placed against the feed end of the deck which was then brought to the beginning of the forward stroke, the clock set to zero, turned through one arc, re-set to zero and turned through the second arc and so on until the cycle were completed, travel per arc could then be plotted as ordinate against the arc series as abscissa to give the ordinary s against t graph found in the text-books on mechanics (Fig. C).

The distance of any point on the curve from the line representing the

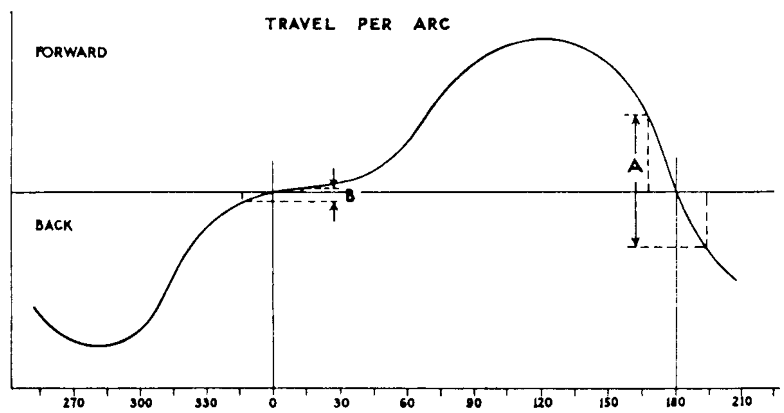

Fig. C.

change of direction was a measure of the speed of the deck at that point in the cycle and the slope of the curve at any point was a measure of the rate of change of speed.

The ratio A/B afforded a strict comparison between one head motion and another. The bigger it was, the better the head motion.

Mr. Frank Yeates said his comments applied equally to the paper under discussion and to the paper by Messrs. Douglas and Bailey, and that by Mr. Chaston. The three papers were concerned with improvements in gravity concentration. All improved designs of concentrating machines were welcome, but the main thing needed, in his view, was preparation of the feed by hindered-settling classification,* i.e. in a column of teetering particles, or a quicksand condition. In that way the size ratio of equal-falling particles was increased, with benefit to subsequent separation. He would like to touch briefly on the theory in order to emphasize his point.

When particles fell in a fluid the force producing acceleration was quickly counterbalanced and the particles then fell with a constant velocity called the 'terminal velocity'. But such velocity, though constant, was not the same for all particles but varied with their size and specific gravity. Thus, it was a matter of experience that a large particle of quartz would fall alongside a small particle of galena. The ratio of the diameters was important in the subsequent concentration operation. Such ratio varied inversely as the densities of the particles less the density of the medium in which they fell. If D_L were the diameter of the light particle and δ_L its density, and D_h, δ_h the diameter and density respectively of the heavier particle, then

$$\frac{D_L}{D_h} = \frac{\delta_h - 1}{\delta_L - 1}$$

for fall in water of density 1.

For cassiterite of sp. gr. 7 and a gangue mineral of sp. gr. 2·65 the ratio D_L/D_h was 3·64. If the density of the medium were increased, however, the ratio would also be increased. A quicksand had a density of about 2. In a hindered-settling classifier, such as a hydrosizer, the material was probably looser than in a true quicksand, and he thought it could fairly be taken as 1·5. Using that value, he found that the above ratio D_L/D_h would be 4·8.

He felt that the main factor in concentration was the streaming water itself. The higher a light particle projected into the swifter surface currents the quicker it travelled down a tilted plane and the farther was it separated from its equal-falling heavy particle under a shaking impulse imparted to the plane. He himself would never engage in a gravity-concentration operation without preparing the feed in a hydrosizer. The effect upon a feed by so doing was strikingly illustrated by Table A opposite, copied from his own contemporary notebook when he was operating a six-pocket hydrosizer handling a tin-bearing sand dredged from the sea. The cassiterite in it had been washed into the sea from dressing-floors on land. The feed to the machine had already been classified in sea-water and it might have been supposed that no change could be made in the grading, but they would see for themselves how great a change had in fact been made. They would note the concentration of coarse material in the first few pockets and of the fines in the last. An important feature of that classification was that more than 31 per cent of the cassiterite was in the first pocket and over 23 per cent in the second, and so was associated with good granular material from which it could be cleanly separated.

*TRUSCOTT, S. J. *A text-book of ore dressing* (London: Macmillan & Co., Ltd., 1923).

TABLE A.—*Grading of samples taken on afternoon of 26.11.1929*

Mesh I.M.M.	Spigot 1	Spigot 2	Spigot 3	Spigot 4	Spigot 5	Spigot 6
	%	%	%	%	%	%
+ 20 . .	4·50	0·34	Trace*	Small trace*	0·07*	Nil
+ 40 . .	22·50	13·10	7·86	2·17	1·65	0·11*
+ 60 . .	41·10	40·28	36·09	31·00	18·30	6·32
+ 80 . .	18·10	20·28	26·20	30·20	26·60	16·20
+ 120 . .	9·40	18·39	19·97	23·30	30·25	39·22
+ 200 . .	1·40	7·45	10·05	12·68	21·90	35·99
− 200 . .	Nil	0·16	0·33	0·65	1·23	2·16

*Very micaceous.

I.M.M. Mesh	*Feed* %	% cum
+ 20	0·68	0·68
+ 40	7·46	8·14
+ 60	30·41	38·55
+ 80	25·95	64·50
+ 120	21·34	85·84
+ 200	13·18	99·02
− 200	0·98	—

Another feature of the operation was that nearly all the products were discharged with clean water, making their behaviour on the tables easy to observe. The slime was carried right through the machine to the cone overflow at the end, and that was a point Mr. Chaston might like to note, for in the synopsis to his paper he said: 'the shaking table gives the best recovery of cassiterite in sizes down to 10μ, provided that the feed is sufficiently slime free.'

Hindered-settling of the feed ought also to improve the *concentration criterion* by increasing the term Δ_f in the ratio.

$$\frac{(\Delta_h - \Delta_f)}{(\Delta_L - \Delta_f)}$$

where Δ_h is the sp. gr. of the heavy mineral, Δ_L that of the light, and Δ_f that of the fluid medium.*

*PRYOR, E. J. *Mineral processing* (London: Mining Publications, Ltd., 1960), pp. 305–6.

Mr. D. G. Armstrong said he felt he ought to challenge the authors' ideas presented at the beginning of the paper. They started by talking about the vertical stratification within the particulate bed and the lateral separation of the stratified layers and then went on to say that the former resulted from the longitudinal motion of the concentrator deck. Surely that was not so. He agreed that one must first stratify and then extract. That had to be done in the jig as well, although in the jig what was best for one was not necessarily best for the other. On the table, stratification was surely by streaming and extraction was by shaking. The shaking motion was for extracting the stratified heavies but the shaking movement contributed very little, if anything, to the stratification. He had always thought that the table deck was essentially something on which settlement could take place; it definitely was not a machine designed to give upward and downward movement. It was settlement that was aimed at. On the slime table there were large pools in which minerals could settle, and then they could be moved along the deck by the shaking movement. He thought that the table motion was designed for the single purpose of extraction—not for stratification.

If a jigging movement were applied to a table, he would regard that as basically a jig with shaking extraction and it was quite wrong to view it as a pulsed-deck table. Rather should they consider that the main movement was jigging, with the secondary movement the shaking, by which means the mineral was extracted.

In discussing concentrator motions, the authors said that a uniform acceleration was required, followed by uniform retardation. They went on to say that the rate of reversal of the deck at the end of the forward stroke must also be more rapid than at the end of the return stroke. The speaker thought that the phrase 'rate of reversal' needed to be defined. He thought the end of the stroke was the most important part of it, both in jigging and tabling. What happened in between was not so important as what happened at either end.

Mr. Muller had made some attempt in his introduction to clarify his definition of 'forward stroke' given in the footnote (p. 380), but as far as the speaker was concerned it only confused the matter a little more. He thought the definition wrong as given in the footnote because, after all, the mechanism of a table or a panner could just as well be at the other end of the machine. Mechanically it did not matter. The physical position of the mechanism had nothing to do with the direction of movement. Surely the proper definition of 'forward stroke' was 'the direction in which the heavy mineral travelled'. He would like to substitute the word 'forward' for the word 'return' at the beginning of the second paragraph under the heading Concentrator Motions (p. 380), as he thought that would make it read more easily. The bump at the end of the panner forward stroke then took the place of the smooth but sharp reversal at the end of the table stroke. He thought that a sudden stop was hardly a modified form of a smooth reversal, which was what the authors were saying. One might just as well say that going up was a modified form of coming down. Following on that, he felt that Fig. 1(*b*) on page 381 should be the other way up, if one had in mind the direction of travel of heavy mineral. There should be a sharp peak at the top and a smooth curve at the bottom. If one visualized the sharp peak shown at the bottom right-hand side as appearing at the top of the figure one could then compare it with the rounded peak at the top of Fig. 1(*a*). Mr. Blyth had touched on that. It was the sudden stop that was wanted, but the mechanical linkage of table head motions did not give a really sharp stop although they came near it, as shown in Fig. 1(*a*).

Returning to the definition of 'rate of reversal', what one saw at the top of Fig. 1(*a*) was surely a retardation at the end of the forward stroke followed by acceleration at the beginning of the reverse stroke. It was not a rate of reversal one was talking about, but a retardation at the end of the stroke. He thought that with those thoughts in mind pages 380–1 could be clarified and in that way the processes could be more easily understood.

The authors had described the differential transformer used for indicating the movement of the deck. Differential transformers were made in America in a large range and sold in the United Kingdom, intended for movements of 0·005 in. up to 6 in., and were tremendously versatile. With moving equipment of that kind very good pictures of what was going on were obtained. In his own jig work the speaker had not in fact used the differential transformer but had used a German instrument, a hand-held vibrograph, which put the stroke-trace on paper, giving a useful record. It did suffer from the drawback of having a spring in it which had to be pressed slightly against the moving object, and if the object were quite light, that did have some effect.

Turning to the pulsepanner—or should one call it a shaking jig?—the speaker said that the kind of up-and-down movement would prove to be more important than the side shake. The same kind of comments and arguments applied to the stroke characteristics up and down as applied to the longitudinal movement. There again what happened at either end was more important than what happened in the middle.

The authors had said that development work was being carried out on continuous pulsed-deck concentrators. Was he right in assuming that that was a mechanical engineering development? If the work had got to the stage of operating on such a large scale, he was sure members would be most interested in hearing some results.

Mr. E. Douglas said he thought the authors had shown quite clearly the advantages to be gained—whether one termed it jigging or pulsepanning did not matter a great deal—by combining the correct head motion with the pulsed or jigged deck.

He was interested to note the time/displacement pattern traced for the Wilfley table (Fig. 7, page 386), but was somewhat surprised to learn that that trace, which was asymmetrical, was not in accord with the symmetrical pattern shown by Taggart and others.

The authors explained that difference on the assumption that their table was not giving the trace it should according to the actuating linkage. He wondered whether that explanation was necessary. If the linkage of the Wilfley table was considered (Fig. D), it would be seen to be made up of a fixed pivot, *a*, and an elbow-type linkage which allowed horizontal

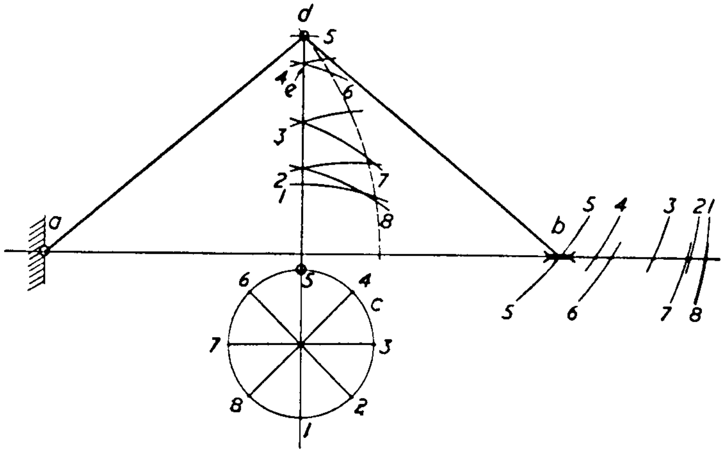

Fig. D.

movement of point b. An eccentric, or a crank, c, rotated and was connected by means of a pitman to the floating pivot d. Consequently point b would take up a horizontal position according to the angular displacement of the eccentric. The various respective positions for c, d and b were indicated by means of reference numbers in Fig. D.

For symmetry, positions 4 and 6 of the eccentric should result in one common position for b. Reference to Fig. D showed that that condition did not obtain.

The derivation of the symmetrical form by Taggart had been achieved by assuming the locus of d to be on a straight line joining d to the centre of the eccentric. In that case, for both positions 4 and 6 of the eccentric, d would move to point e (i.e. the intersection of arcs $4d$ and $6d$) and b would be displaced the same amount in both positions.

In practice, the intersections between arcs $4d$ and $6d$ and the true locus of d (shown dotted in Fig. D) are displaced relative to each other, thereby producing an asymmetrical movement which, when plotted for comparison with the authors' trace, gave the characteristic shown in Fig. E.

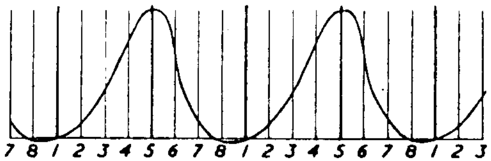

Fig. E.

He felt that that illustrated that the authors' method of obtaining the trace had given an exact interpretation of what the Wilfley-table linkage did and, in fact, what it was designed to do.

He considered that the authors' results were correct and that an asymmetrical form was the true form for the Wilfley head motion; he asked if, in that instance, Taggart was wrong.

Professor B. W. Holman said the paper was an excellent example of modern methods of scientific research applied to an old subject, water concentration tests—a subject on which the speaker had contributed a paper some 30 years ago.*

The two main processes, as stated by the authors, were still vertical stratification and lateral separation of the stratified layers. Various additional methods were promoted then disappeared, for different reasons, chiefly financial. One particular table with which the speaker had great success with a very difficult heavy gangue tin ore was the Sen Pan table, in which the table surface had an up and down movement as it imitated the motions of a vanning shovel. The corners of the rectangular table were supported on four steel spheres, each of which travelled in a closed stationary groove of varying depth. The plan and depth of the groove was determined from motion studies of men using a vanning shovel. A reciprocating motion was transmitted to the table by a connecting rod and an eccentric, but the table surface also moved up and down as the four steel balls travelled in the grooves. By causing a backward and forward motion one got the surface of the table moving sideways, lengthways and vertically in each completed stroke, to describe a motion of the vanning shovel.

Another apparatus was the Buss or Humbolt table, which was supported on very long slats, each of which was designed to give a lift to the ore. Owing to the length of the slat, 15 or 18 in. long according to the ore, one had a considerable element of lift. The vertical motion of the particle was a motion along an arc of a circle. Those tables were used some 30 years ago in Italy, France and Germany on a large scale but were hardly ever used in the United Kingdom. Some were quite simple, and he had on one occasion, with the aid of mine carpenters, made a couple of tables which did quite satisfactory work in separating a lead ore—not an easy proposition. In that way the speaker said he obtained a very simple and cheap table. Most people had found that for many obscure reasons one sometimes found one good table giving better results than another on a given ore.

Another factor in testing laboratory wet mineral concentrators was the determination of the actual degree of release of the minerals in the crushed samples tested. In some ores the gangue and the mineral were very intimately mixed, and in others they were not. That could be approached by elutriation, heavy liquids, dielectric separation and the use of the microscope.

Mr. Pownall, replying to the discussion, said that there were points he would like to mention briefly, although others would have to be considered at greater length.

*HOLMAN, B. W. Water concentration tests. *Trans. Instn Min. Metall., Lond.*, **39**, 1929–30, 426–72. (*Bull. Instn Min. Metall., Lond.*, no. 312, Sept. 1930, 1 47.)

In regard to Dr. Burch's contribution, the speaker said he had not the figures available at that moment, but could give an estimate of the accelerations during the panning cycle after examination of the motion diagram photographs. It would be interesting to see whether they matched up with the proposition Dr. Burch put forward. The concept of an acceleration number to Reynolds number had not occurred to the speaker before. Dr. Burch had also made an interesting point in regard to the production of secondary circulation by vibrating the tables to different amplitudes along its length. The idea of a resonant vibration was an interesting thought in that connection.

Mr. Blyth had asked why the bumping table was abandoned. It must have happened very many years ago, but he understood it was for economic reasons. One reason why the authors were hesitant in advancing the proposition of a large-scale tonnage-treating panner rather than a shaking table, was that they thought maintenance might be heavy; however, they were open to correction on that point.

Regarding the use of a three-lobe cam, that was the smallest number that could be conveniently incorporated into the mechanism without encountering design difficulties. The design which Mr. Blyth had shown appeared to eliminate that difficulty, although he was not entirely clear about the mechanism for varying the stroke of the table. The authors' concept of the macropanner included the desirability of having some adjusting linkage so that, whatever the amplitude of the stroke, the characteristic of the motion remained identical. That did not happen on the superpanner because as its throw was reduced part of the motion cycle was eliminated.

Mr. Yeates had made the point about classification of the feed for which he agreed there were many strong arguments. One set of variables had to be removed from the system in order to exploit another and classification of feed was a very good and popular method of doing that. Preparation of feed material was not discussed in the paper since the primary purpose was to investigate the mechanism of motion. The authors had regarded any materials they had put over the deck as a method of measuring the performance of a particular motion rather than as a concentration operation *per se*.

Mr. Armstrong had raised several points which would have to be considered carefully at their leisure. He had said that stratification occurred mainly by streaming, which was indeed true, but what was streaming? It was a vertical stratification directed by mechanical means by the forces associated with fluid flow and friction. He still felt that the pulse principle offered a more controllable means of achieving the same ends. Mr. Armstrong had suggested that a pulsepanner ought to be a jig, but the speaker tended to regard any lateral extended concentration surface which was pulsed to a certain degree as a pulsed-deck concentrator, whereas any deep-bed concentrator which incorporated a relatively small degree of lateral separation could be regarded as a jig. There were such devices as the Halkyn jig, which moved in a lateral direction but which was not a pulsed-deck concentrator.

In reply to Mr. Armstrong's statement that the rate of reversal needed defining, he thought that that was a question of terminology; rate of reversal could be expressed in terms of acceleration. With regard to the definition of direction and the arguments advanced by Mr. Armstrong, he suggested that he had fallen into some of the confusion that had confronted the authors. They had found it necessary to adhere to one convention of direction, otherwise it was easy to be confused. Mr. Armstrong had proposed—the reverse to their convention—that the forward motion should always relate to the direction of the heavy material. As the direction of the heavy material could be either way, that would be rather confusing. Mr. Armstrong had also said that his definition had validity in that the mechanism could be at either end; in terms of a shaking table Mr. Pownall agreed that that could be so.

Mr. Douglas had produced some most interesting points, and the speaker felt he was probably quite right. The asymmetry of the Wilfley-table motion had been observed by the authors and it had puzzled them. They had wondered at first whether the measuring apparatus was incorrect, but had come to the conclusion that it was not and that that was a genuine effect. He thought that Mr. Douglas's explanation appeared a good deal more likely than the rather tentative one the authors had suggested. That point served to accentuate the desirability of using some dynamical method to investigate deck motions, rather than a graphical method derived from the mechanics of the system.

Professor Holman had described a most interesting mechanism, the vanning table. A table which imparted an upward throw to the minerals on the table deck was an attractive proposition but, once again, would seem to lack controllability, such as was achieved by a water pulse applied to the deck.

Mr. Muller said he and his co-author had asked for the help of members and they had certainly received it, and for that he was very grateful.

Dr. Burch's vanning results were of interest; he had, in fact, analysed Dr. Burch's products for him and hoped that it would eventually be possible to re-combine those products and run a comparative test on the pulsepanner. Dr. Burch had also mentioned that other methods of moving the heavy minerals in a longitudinal sense might be possible and he hoped he would have the opportunity of discussing that aspect with Dr. Burch at some future date.

Mr. Armstrong had discussed the question of defining the direction of deck motion of tables and panners. They had given their definition in the paper and he had emphasized, when introducing the paper, the reasons for their choice of definition. Two approaches to that problem were possible and Mr. Armstrong preferred the opposite to that chosen by the authors. He was in complete agreement with Mr. Armstrong as to the importance of the form and application of the pulse and suction strokes to the deck. They were only at the beginning of the problem and it was one which would certainly have to be investigated.

Mr. F. D. L. Noakes said that he felt that some of Dr. Burch's remarks about the future development of the pulsepanner might be capable of misinterpretation; it was clearly not intended to suggest that commercial development should be allowed to lag five years behind any further scientific development work. Would it not be possible for simultaneous commercial development to be accelerated by using something very similar to the pulsepanner that might be already available? For example, perhaps a conventional dry gravity separating table could quite simply be adapted. Would it not be fairly simple experimentally to take such a table and convert the continuous upward flow to synchronized pulsations, first of all with air and then with water? There would be the danger that one negative result might throw back the commercial development of such an apparatus, but he felt that that risk would be justified by the advantages to be gained in having a British idea developed in Great Britain.

Dr. Burch apologized for not expressing himself more clearly, because there was absolutely no disagreement between himself and Mr. Noakes. All he had meant was that they should not demand of the authors that they must make the commercial application in any particular way succeed at once—'or else'. They should make it clear to the authors that they had long-term backing if they needed it.

The President said that he was sure that everyone was pleased that the paper had been discussed orally and also that the authors had been present to introduce it. He expressed thanks to them and to all who had contributed to the discussion of the paper.

[At the conclusion of the discussion the authors gave a demonstration of an electronic method of studying micropanner motions.]

WRITTEN CONTRIBUTIONS

Mr. J. S. Jacobi: It is gratifying to observe that work on improving the Haultain superpanner continues. In a sense the wheel appears to have turned full circle: by imparting an upward impulse movement to the panner the authors have now arrived at a motion not unlike that recommended for the old miner's pan, but there is of course a very marked difference in that the new vertical pulsepanner is a precision-built tool based on a full analysis of the component movements.

Looking at developments in mineral dressing from the remoteness of the Andean Sierra one is struck with the impression that we are experiencing a marked renaissance in the study of gravity separation, the techniques of which were being neglected a generation ago when people's minds were occupied with flotation and with flotation alone. By contrast, the last

ten years have witnessed notable developments in non-flotation techniques such as the heavy media cyclone, the commercial development of TBE, vastly improved slimes tabling methods and the wet magnetic separation of sub-sieve materials. This work has been accompanied by more rigorous laboratory analysis and testing equipment of which the present paper gives us a fine example. It is likely that metallurgists will increasingly turn to gravity separation methods in the micron range to complement or supplant froth flotation to a marked degree.

I trust the present paper may be considered as one of a whole series. The innovation disclosed is most welcome, but some of the underlying test work seems a little limited in scope. Several suggestions for further study present themselves as no doubt the authors fully realize.

Most mineral dressers using shaking tables prefer a feed deslimed by classification, while the authors undertook their test work on a closely sized feed. Classification is not only more practical in the size ranges under discussion but also it is based on a sounder principle because shaking tables are designed to separate equal-falling particles rather than particles of equal size.

By using artificial mineral mixtures in which the heavy constituent is fully liberated the authors have eliminated a variable which tends to defy precise analysis but which also is inevitably present in industrial operations, namely a true middling. Not only does a middling lend stability to the table operations but it is sound practice to liberate a great deal of the values at the coarsest possible particle size followed by a re-grind of a middling of locked particles. A fully-liberated feed is an overground feed which spells excessive sliming and unnecessary consumption of energy.

One cannot help feeling that, in their tabulations of comparative results, the authors have done less than justice to the shaking table. Unlike the superpanner and its recently derived versions, the shaking table is meant to be operated under steady-state conditions. By recirculating the table products in the way described in the paper the authors have indeed attempted to simulate a continuous operation but with the significant difference that concentrates are gradually withdrawn from a recirculated batch sample. Under such conditions one would not expect to obtain best tabling results.

In Fig. 10 (p. 389) the authors show a table recovery of 37 per cent riebeckite at a 53 per cent concentrate grade. I would venture to guess that with a closely graded feed, fully liberated and having a concentration criterion of 1·47, most practical table operators could obtain vastly better results. If the authors would care to send me a 20-kg sample of their artificially prepared quartz-riebeckite blend I would attempt this myself on a 40-in by 18-in Wilfley laboratory table.

However, it matters little whether some of the test results reported by the authors could be improved upon or amplified. The positive result is that a useful new laboratory tool has been developed which will bring us a step nearer to defining the ideal separation performance feasible under a given set of conditions. More important still, we may look forward with lively anticipation to the promised development of an industrial-scale pulsed-deck concentrator.

Mr. I. R. M. Chaston: The authors describe a very interesting possible development in gravity separation equipment. Their pulsepanner idea seems to operate on a principle somewhat similar to the Hooper vanning jig of the last century. This moving-sieve jig had a central feed and a horizontal, as well as a vertical, motion. The horizontal component caused the concentrate on the bed to move continuously to the higher end of the screen where it overflowed and was collected, while the tailings were continuously rejected at the other, lower, end. A great many problems would need to be overcome in applying this principle to a full-size shaking table in the way of giving an even pulse over the whole table area and of preventing clogging of the porous surface. To appreciate some of the difficulties to be faced one has only to recall the continual vigilance which has to be exercised in most mills to prevent clogging, with pipe scale and other foreign matter, of the much larger apertures supplying wash water into the tables.

As the authors suggest in their paper (p. 391), the results of so few tests as are reported must be considered sceptically. One odd point which the authors may be able to explain is that they seem to suggest by the way they have drawn the curves for the pulsepanner results in Fig. 10 (p. 389) that the recovery is somehow limited to a figure of not much more than 80 per cent. In this I feel that they have been unconsciously influenced by their statement in the discussion that the best result would be shown by a rectangular plot. This is not to say that their results for the superpanner, micropanner and pulsepanner are not indicative (the inclusion of the laboratory shaking table with its 40 times greater loading cannot really be called significant in the context of the paper) and they certainly support the decision to continue the work.

The actual mechanism of concentration under the pulsed-deck conditions provides a considerable field for speculation. The timing of the pulses with relation to the horizontal motions of the deck will presumably have a considerable effect on the action and, even at this early stage, it would be useful if the authors could give us some indication of this relationship in their tests.

I wonder if a sharp up-and-down motion of the superpanner might not give something of the same sort of jigging action as the water pulses. The effect would not be nearly as positive as the water pulses but it would be a good deal easier to apply.

Mr. A. L. Stewart: I have always considered the superpanner and micropanner as mineralogical tools, and the Wilfley laboratory table more qualitative than quantitative even when used for mineral-dressing tests. From the point of view of making separations for mineralogical purposes where one of the minerals has a specific gravity of less than 3 (as in the examples given in the paper) it is doubtful whether these machines can compete with heavy liquids. The real range of the superpanner is for separations of minerals with higher specific gravities for which the use of heavy liquids tends to be expensive, or the liquids are poisonous or need inconvenient thermal control, but even so it is limited to a concentration criterion of about 1·5 when using water, so that many problems are difficult to handle.

Such a problem was the separation of corundum and zoisite, the measured specific gravities of which were 3·89 and 3·34 respectively, giving a concentration criterion of only 1·24 in water. However, for a liquid with specific gravity 2·3 the criterion becomes 1·53 and it was found possible to make a good separation in a prospecting pan using a mixture of TBE and Shell mineral turpentine, some specific gravity being sacrificed for improved viscosity.

I would suggest that the range of usefulness of such machines could be greatly extended by the substitution of one of the cheaper heavy liquids for water, and I should be interested to know if the authors have any information of such applications.

Mr. F. A. Williams: The mineralogical school of thought in so-called metallurgical accounting and control is already deeply indebted to Mr. Muller for an earlier paper* describing some new items of equipment for the physical assaying of samples in terms of minerals. The list comprised a micro-volumenometer, a picker belt, a rotarys ampler and the micropanner. The paper at present under discussion carries one of these developments several steps further. To the micropanner is now added the macropanner and the pulsepanner—all descendants of Haultain's original superpanner.

Whereas it was formerly difficult and time-consuming to wash the slimes out of samples before panning without loss of fine heavy minerals, any values washed out can now be effectively and quickly retrieved by means of small hydrocyclones as I have described elsewhere.† I would like to ask the authors if they have carried out any test work on the macropanner and pulsepanner with heavy minerals finer than the sizes −60 +100 and −60 +200 mesh B.S.S. mentioned on page 388. The recovery of fines is a very important aspect of the development of mechanical panners for laboratory work.

The influence of hydrocyclones on plant layout

When the only classifiers available for use in ball-mill closed circuits were of the mechanical type, the space in which jigs could be inserted to scavenge heavy minerals as soon as released was quite inadequate. In consequence the degree of overgrinding was considerable and, with tin ores, the number of sand tables, slimes tables, vanners, etc., required was excessive.

With the modern practice of installing pumps and hydrocyclones instead of mechanical classifiers, much more space becomes available for installing

*Muller, L. D. Some laboratory techniques developed for ore dressing mineralogy. *International Mineral Processing Congress 1960* (London: Instn Min. Metall., 1960), 1047–57.

†Williams, F. A. Recovery of fine alluvial cassiterite: correlation of bore valuations with plant-scale recovery. *Trans. Instn Min. Metall., Lond.*, **70**, 1960 61 (*Bull. Instn Min. Metall., Lond.*, no. 648, Nov. 1960), 49–69.

jigs in closed grinding circuits. As in alluvial mining practice one or even two stages of primary concentration with ample jigging area can now be used followed by a clean-up jig. This system provides the combination of a good recovery and a high-grade concentrate. The recovery of chats is also optimized.

Applied to ball-mill circuits another great advantage of this combination of hydrocyclones and jigs is that it minimizes overgrinding. Furthermore, by extending recovery in jigs to much finer sizes it offers good scavenging before flotation or even, in some cases, an alternative to flotation. This aspect of the revolution in ore dressing made possible by the advent of hydrocyclones is particularly attractive when the valuable minerals are friable and/or when the cost of effective flotation is inordinately high. These conditions are typical of many oxidized lead/zinc ores.

Lower plant operating costs

Although shaking tables are very effective for the recovery of fine heavy minerals, particularly when, as shown by Mr. Chaston,* the feeds are adequately deslimed and suitably sized by means of hydrocyclones, the cost per ton of throughput is considerably higher than with jigs. Furthermore tables occupy much more space in relation to tonnage treated.

On page 392 of their paper Messrs. Muller and Pownall announce that development work is in progress on continuous plant-scale pulsed deck concentrators in which a jigging type of pulse is to be added to a shaking deck action. I would ask the authors whether, in their opinion, these concentrators could be expected to have a greater capacity per unit deck area than conventional sand tables and whether the design might lend itself to multi-deck construction.

Jigging is the cheapest mechanical means of mineral recovery. This has been amply demonstrated by the use of jigs on bucket dredges. The published costs of some of the large tin dredging companies in Malaya are only about 6d. per cubic yard. A cubic yard of ground dug yields not more than about a ton of sand to the jigs. Jig operation, including the low-pressure pumps supplying the hutch water, and the additional pumps circulating the hutch discharges, amounts to only about a sixth of the total cost of dredging. So in this application jigging an unsized feed costs only about one penny per ton.

The average feed rate on tin dredges is about 20 tons of sand per hour to four jig cells in series, each cell having a screening area of about 40 in. by 40 in. Douglas and Moir† have recorded a feed rate of over 60 tons an hour of comminuted gold ore to two such cells in series. The cost per ton will vary inversely with the rate of feed.

Single-deck tabling is usually considered to cost about 6d. per ton. It

*Chaston, I. R. M. Gravity concentration of fine cassiterite. *Trans. Instn Min. Metall., Lond.*, **71**, 1961–62 (*Bull. Instn Min. Metall., Lond.*, no. 662, Jan. 1962), 215–25.

†Douglas, J. K. E., and Moir, A. T. A review of South African gold recovery practice. *7th Commonwealth Min. Metall. Congr. 1961, South Africa and Rhodesias* (Johannesburg: South African Institute of Mining and Metallurgy, 1961), 28 p.

could reasonably be inferred that triple-deck tabling would cost appreciably less. Perhaps pulsed concentrators will bridge the gap between the operating costs of jigs and tables per ton treated.

The challenge to flotation

[redacted]w that the combination of hydrocyclones, jigs and tables has been [redacted]d to extend the recovery of heavy minerals down to about the same size limit as flotation, it begins to appear that gravity concentration may soon stage a comeback and challenge flotation in many of its established fields of application, particularly in primary concentration. It is now only a matter of comparative costs. In view of the frequent high total cost of the chemicals consumed in flotation, the logical development in the processing of ores in which the valuable minerals are heavy and coarse enough for gravity concentration would now be, in many cases, to restrict flotation t[redacted] the processing of mixed mineral gravity concentrates.

This approach has already provided a solution to the difficult probler[redacted] of processing a pyrochlore-bearing carbonatite in Canada.* Triple-deck tables are used to get rid of most of the calcite. A rather expensive sequence of flotation processes is then justifiably applied to the small bulk of gravity concentrate.

With the introduction of the Imperial Smelting furnace described by Morgan,† which will take mixed lead-zinc concentrates, differential flotation of lead-zinc ores is no longer essential. Either bulk flotation or modern gravity concentration will now suffice and whichever is the cheaper should be used. The replacement of flotation by modern gravity concentration would appear to be particularly attractive in the case of oxidized lead-zinc ores rich in zinc. Galena PbS and sphalerite ZnS can be floated fairly cheaply especially if differential flotation is not needed. Sulphidization is necessary before the cerussite $PbCO_3$ can be floated and this adds to costs. But the even more expensive amine flotation is necessary for the recovery of calamine $ZnCO_3$. Usually any pyromorphite $PbCl(PO_4)_3$, anglesite $PbSO_4$ or willemite $ZnSiO_4$ present is not recovered at all. In current flotation practice with such ores the flotation of 'oxide' zinc is often omitted because of this high cost of chemicals and the poor recovery resulting from overgrinding. Up to 5 per cent 'oxide' zinc is often discharged in the tails. Modern gravity concentration with hydrocyclones, jigs and tables might replace flotation in the processing of such ores. As a geologist I am more particularly interested in the prospects for developing some oxidized lead-zinc orebodies with a high zinc ratio which previously had been considered to be unpayable.

At Ruwe in the Katanga Province of the former Belgian Congo an intensely-decomposed metamorphic rock containing malachite was mechanically excavated, disintegrated with monitors and concentrated in jigs.

*Concentrating a Canadian pyrochlore. *Min. Mag., Lond.*, **106**, no. 4, April 1962, 245–6.

†Morgan, S. W. K. The production of zinc in a blast furnace. *Trans. Instn Min. Metall., Lond.*, **66**, 1956–57 (*Bull. Instn Min. Metall., Lond.*, no. 609, Aug. 1957), 553–65.

During a tour of the Copperbelt of Northern Rhodesia I was able to study the problems associated with recovery of cobalt as a by-product on one of the copper mines. Similar copper-cobalt sulphide ores occur in the Congo. There was a considerable loss of very fine sulphide cobalt in the flotation tails due apparently to overgrinding of the softer cobalt minerals, and this appeared to me to present a case for research work in the use of hydrocyclones and jigs to scavenge out a mixed concentrate for separate differential flotation.

O'Meara* has described numerous examples on the Copperbelt where the use of mineral analyses of samples involving grain counting under the microscope, instead of chemical assaying, was essential for the solution of ore-dressing problems.

For lode mining, as in alluvial mining, the analysis of samples directly in terms of minerals is a basic necessity. The work of Messrs. Muller and Pownall described in the paper should prove to be a very useful contribution to the technology of sample valuation.

Performance of a Shaken Helicoid as a Gravity Concentrator

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D. L. R. BAILEY, B.Sc.

Further contributed remarks on paper published in August, 1961 (Transactions, *vol. 70, 1960–61) pp. 637–57, and on joint discussion published in April and June, 1962* (Transactions, *vol, 71, 1961–62) pp. 397–436 and 547–50*

Mr. Donald Gill: I wish to thank the authors for their very careful interim reply to my contribution (p. 416) to the discussion and especially for the helpful description of the method by which the micron-by-micron recoveries illustrated in Figs. 19, 21 and 22 (p. 652) of their paper were derived.

Vanner and round frame.—The samples of the feeds and tails from these machines were taken mainly for the purpose of supplying bulk material for tests on the helicoid. The samples were taken under all the difficulties inseparable from work in an operating mill, under conditions of obviously fluctuating feed and, moreover, in neither case were the feed and tail samples taken over the same period of time. In these circumstances it is evident that the sampling was not sufficiently reliable for use in 'deriving' the performance graphs (Figs. 19 and 22) for the two machines. The authors admit this in their reply but I consider that they should go further and withdraw both these figures and the conclusions based on them: there are too many known inaccuracies for the conclusions to be valid.

Helicoid test on vanner feed.—This test is illustrated in Table II (p. 651) and Fig. 21. I have prepared Table A to show the weight balance between feed, tail and concentrate, assuming a recovery of 5·3 per cent of rougher concentrate, which appears probable from Table IV (p. 655) of the paper, for the various size fractions between +36 and −6μ (quartz). If it is assumed that the decantation tests were generally fairly reliable my table shows, without any equivocation, that during the 'run-through' test on the vanner feed the bed of silt on the helicoid deck must have been unstable. There was a notable *loss* of material from the silt bed to the tail in the range −24 +12μ and an approximately equal *accession* of material to the silt bed from the feed in the range −12 +6μ. The amounts of these transfers are notable, because they each represent between 8 per cent and 9 per cent of the feed during the run-through test.

TABLE A.—*Weight balance for 100 lb of feed. From Table II (p. 651) of the paper, assuming 5·3 per cent of concentrate to be made*

Size (quartz)	Feed	Tail		Concentrate (by difference)	
	lb	%	lb	+	−
μ				lb	lb
+ 36	16·0	15·5	14·7	1·3	
− 36 + 24	20·0	15·5	14·7	5·3	
− 24 + 12	31·5	42·5	40·2		8·7
− 12 + 6	19·5	12·0	11·4	8·1	
− 6	13·0	14·5	13·7		0·7
	100·0	100·0	94·7	14·7	9·4
				− 9·4	
				5·3	

Now, the computation of the performance graph (Fig. 21) depends upon there being only three entities concerned, namely feed, tail and concentrate—that is to say the computation depends entirely upon an assumption of the stability of the silt bed during the test. Since instability of the silt bed during the test is demonstrated by my Table A, it is clear that Table II cannot be regarded as a reliable basis for computing Fig. 21.

If, on the other hand, it is assumed that something went seriously wrong with one of the decantation tests in Table II, by way of wrong allocations of weights in the −24 +12 and the −12 +6μ fractions, the same arguments apply.

There is, in fact, no *real* evidence presented in the paper for failure of the helicoid to recover tin in the range 8–13μ (cassiterite spheres). There is no *real* evidence for what it is now the fashion to call 'double recovery'.

Appeal to the concentrates.—In a written contribution to the discussion of Messrs. Douglas and Bailey's paper Mr. Chaston said (p. 427): 'The tables would be much more interesting and useful if they had been extended to include the concentrate sizing and assays.' I entirely agree.

In the cases of the vanner and round frame sampling, the taking of

*O'Meara, A. E. A mineralogical approach to some Copperbelt metallurgical problems. *7th Commonwealth Min. Metall. Congr. 1961, Northern Rhodesia Section,* paper no. 8, 1961, 49 p.