could reproduce, and even improve on, the results achieved in the early Richards experiments.

The Main Mill at Beralt Tin and Wolfram, Ltd., was more concerned with the recovery of wolfram than of cassiterite, but in his opinion the general principles involved in the gravity concentration of both minerals were the same. Wolfram had a slightly higher specific gravity than cassiterite, but was even more friable and prone to break up during milling. For that reason the major part of the loss of wolfram in that plant had always been from the section treating the fines. Within the limitation of the existing mill building, that section now followed very closely the system suggested in the conclusions given in the paper. All classifier overflows were collected at the head of the mill and treated in two stages by gravity-fed cyclones, with the underflow from each stage being tabled separately. The results of that operation were, therefore, of direct interest.

The total flow of feed to the fine treatment section was between 250 and 300 gal/min of pulp containing up to 10 per cent by weight of solids. There was a certain amount of coarse micaceous flake material in the feed but the granular solids were nearly all finer than 100 mesh. In line with his theory, the first stage of cycloning was intended to split off the coarser fraction. At first, four 6-in cyclones were used to make the split, working with a feed head of about 6 ft. The cyclones operated well and gave a slime-free spigot product which was tabled on four sand-tables. The results, as shown in Table A, of samples taken at hourly intervals over a week suggested, however, that the split made by the cyclones was too fine.

TABLE A.—*First-stage table operation using 6-in cyclones*

Size mesh B.S.	Table feed Wt. %	Table feed Assay % WO_3	Table tails Wt. %	Table tails Assay % WO_3
+60	6·4	0·042	7·1	0·030
−60+100	12·5	0·046	13·2	0·026
−100+300	53·6	0·312	52·7	0·038
−300	27·5	2·102	27·0	0·748
	100·0	0·754	100·0	0·224

Recovery in the +300-mesh fraction was nearly 90 per cent, while recovery in the −300-mesh fraction was only 65 per cent. Since that latter recovery was very much less than the recovery made by the second-stage tables, where nearly all the feed was −300 mesh, it was obviously better to make a coarser first split and to push more of the −300-mesh material on to the second-stage tables. Tests with a 6-ft diameter hydrosizer had given a coarser but much less precise split and the coarse material in the overflow interfered in the second-stage recovery. That hydrosizer had been replaced with a single 20-in cyclone operating with a feed head of only 5 ft. That gave the much improved results shown in Table B. The recovery in the +300-mesh fraction was well over 90 per cent and the recovery in the now much reduced amount of −300-mesh material had improved to over 85 per cent.

Further work was now being carried out to see if the separation could

TABLE B.—*First-stage table operation using a 20-in cyclone*

Size mesh B.S.	Table feed Wt. %	Table feed Assay % WO_3	Table tails Wt. %	Table tails Assay % WO_3
+60	12·4	0·024	10·1	0·024
−60+100	18·9	0·040	18·8	0·028
−100+300	57·7	0·548	61·7	0·032
−300	11·0	2·602	9·4	0·382
	100·0	0·613	100·0	0·063

be further improved without affecting the recovery in the second stage of tabling. The figures suggested that screening at 200 to 300 mesh would be a better preliminary treatment than classification, and experiments were planned to explore that possibility.

The overflow from the first-stage separation delivered into a head-box from where it was piped to a distribution-box on the lower table floor. That box fed three clusters of 4-in cyclones, each cluster feeding one table. There were four cyclones in each cluster, but the number in use could be regulated to maintain the level in the head-box and keep the full 25 ft of pressure head acting on the cyclones. Usually three cyclones were working in each cluster. Regulation was effected by plugging the cyclone overflow and spigot with wooden bungs whenever the level in the head-box was noticeably low and removing them when the head-box overflowed. Feed to each of the tables was between 200 and 300 kg/h of solids.

TABLE C.—*Second-stage table operation*

Size equivalent wolframite spheres	Table feed Wt. %	Table feed Assay % WO_3	Table tails Wt. %	Table tails Assay % WO_3	Table middlings Wt. %	Table middlings Assay % WO_3	Table concentrates Wt. %	Table concentrates Assay % WO_3
>36	3·8	1·21	3·8	0·047	3·2	1·25	8·1	12·92
<36>27	13·5	1·01	12·5	0·064	13·5	1·32	16·8	19·25
<27>18	33·2	1·24	28·0	0·068	33·6	1·29	31·5	20·50
<18> 9	29·7	1·00	30·3	0·086	39·1	0·85	35·6	13·57
<9	19·8	0·38	25·4	0·228	10·6	0·49	8·0	5·49
	100·0	0·97	100·0	0·113	100·0	1·04	100·0	16·01
Wt. solids in kg/h	240		184		45		11	

Table C showed the result of sizing tests on one of the second-stage tables. Total recovery of wolfram was over 90 per cent and the total recovery of −9-μ wolfram was over 40 per cent.

The diagram showed the total recovery of wolfram in concentrates and middling, also the recovery of wolfram in the concentrates for each size range. Recovery to the concentrates was over 75 per cent in all sizes down to 9μ and even in the −9-μ size range over 20 per cent of the wolfram was recovered in the concentrates.

Those excellent results with a feed of over 200 kg/h to each table suggested that the maximum table capacity in treating the fine sizes was much higher than the figure of 100 kg/h suggested in the paper. Lately

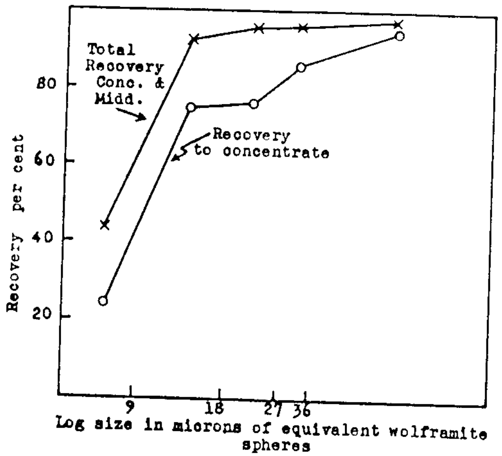


Fig. A.—Recovery of wolfram in second-stage tabling.

they had had feeds of up to 500 kg per table with little reduction in recovery. Two of the second-stage tables were conventional slime tables and one an old fine-sand table. Test results indicated that the slime tables gave a slightly better recovery than the sand-table although the difference was not very great.

Table D showed the size distribution of the solids in the overflow from the 4-in cyclones. There was still some loss of $+9$-μ wolfram and, when space became available, it was proposed to pump the overflow through smaller cyclones to make an even finer split. That third-stage underflow would also be treated on tables and it would be interesting to see if the absence of much material coarser than the 18-μ wolfram equivalent would improve the recovery in the -9-μ size ranges as the removal of the coarse material improved the recovery of the —300-mesh wolfram on the second stage of tabling.

TABLE D.—*Overflow from 4-in cyclone*

Size equivalent wolframite spheres	*Overflow of 4-in cyclone* *Wt.* %	*Assay* % WO_3
>18	4·8	0·37
<18> 9	26·4	0·32
< 9	68·8	0·20
	100·0	0·24

[Mr. Chaston referred to some samples, which he had brought to the meeting, of —300-mesh concentrates, one assaying 44 per cent wolfram and coarser than 40μ, the other assaying 7 per cent wolfram and finer than 10μ.]

A great deal of work had been done in measuring the effect of chemical additions to the surface charge of various minerals, including cassiterite.* That work showed that small additions of reagent could selectively affect the zeta-potential of minerals, but in applying those results in practice he thought the real difficulty would lie in buffering the solutions to give the required low- or zero-potential for the cassiterite in the presence of the usual impurities present in mill water. That was a new field which obviously required a great deal of experimental work.

Dr. C. R. Burch said all must agree that the authors had made an extremely strong *prima facie* case for further work on shaken helicoids, but they had no particular reason to suppose that the Mozley-Sellin helicoid was the best design. The choice of 2-in pitch had been guesswork and they were fairly sure the profile was not the best. In addition they had had no particular reason to believe that 36-in diameter was 'the right size' to make a shaken helicoid. They knew that the 36-in would treat finer material than the 12-in, and would expect a 72-in to treat finer material still. Finally, there were good reasons for believing that a further improvement would be obtained by putting intermediate feed points and tailing bleed points part way down the helicoid. If they wanted to get the best design there was nothing for it but to make at least five large mine-size helicoids—three to establish a 'bracket' on pitch, and at least two more to establish a bracket on size.

He was a little saddened to learn that, although 18 months had elapsed since the authors' first favourable report, Warren Spring Laboratory had not yet made any more shaken helicoids. He was, therefore, all the more grateful to the authors for publishing the paper. That made it pretty certain that other helicoids would be made—if they did not do it first—at least in those countries where technological work was taken seriously.

Mr. P. J. H. Rich said the tests described were very limited and they were, in addition, carried out on a prototype machine the design of which had since been modified. A true valuation of the helicoid's possibilities could not be attempted until all variables, particularly pitch, had been explored. Despite the willingness of Warren Spring Laboratory to carry out a full testing programme, that would not be done until adequate financial support was forthcoming. It was not only the shaken helicoid that was being allowed to die for lack of proper development support.

In view of the difficulties in carrying out experiments without interfering with production, and lack of co-ordination, it seemed an obvious solution to have a central organization responsible for the co-ordination, development and proving of any promising new ideas. Research stations already

*O'Connor, D. T., and Buchanan, A. S. Electrokinetic properties of cassiterite. *Aust. J. Chem.*, **6**, Aug. 1953, 278 93.
Sun, S. C. The mechanism of slime-coating. *Trans. Amer. Inst. Min. Engrs*, **153**, 1943, 479-92.
Gaudin, A. M., and Sun, S. C. Correlation between mineral behaviour in cataphoresis and in flotation. *Trans. Amer. Inst. Min. Engrs*, **169**, 1946, 347 67.
Mattson, S. Cataphoresis and the electrical neutralization of colloidal material. *J. phys. Chem.*, **32**, 1928, 1532-52.

established and well known in Malaya, Nigeria, Bolivia, the U.S.A. and England could be asked to provide facilities if funds were made available, and perhaps a dredge at the end of its life could be taken over for experimental purposes. The 30 to 50 per cent fines losses in the tailings would provide ample scope for research and their recovery help to pay for it.

Mr. J. E. Denyer said the *Transactions* of the Institution published in the first and second decades of the century contained a number of papers dealing with matters bearing on the improvement of recovery in treatment plants on tin mines in Cornwall. It had been realized at that time that recoveries were low, but unfortunately the endeavours of those who had devoted so much time to the problem of improving them bore little fruit for, as a result of the difficulties brought about by the 1914–18 war, and the subsequent economic depression, Cornish mining had fallen into a steady decline and only two mines survived. There were, however, signs that, because of the threatened scarcity of tin and the present high price, there was a renewed interest in the possibilities of Cornwall as a producer of tin.

Some of the early work on the shaken helicoid was done at South Crofty, with which he was connected, and, as had been reported in the paper, much of the test work at Warren Spring Laboratory had been done with material from the South Crofty mill; its performance was compared with that of vanners and round frames in that mill. The results of some of the test work had been revealed to them in confidence over a year ago, and they had naturally given some thought to the question of whether it would be to their advantage to replace some of their machines by shaken helicoids. He would like to explain why they did not contemplate taking any steps in that direction at present.

In the first place the paper describing the helicoid referred to the relatively large quantities of cassiterite normally associated with the slimes which were rejected in the mine tailings and went on to say that to recover the whole or a proportion of those values might have a considerable effect on the present economy of the industry. Those statements were imprecise and in his view somewhat misleading, at least as far as South Crofty was concerned, for only some 5 per cent of the tin in their ground ore was present in the $-20\text{-}\mu$ material on which the shaken helicoid had been shown to be effective. Admittedly the chance of improving recovery in any size range was not to be neglected if it could be achieved economically, but in their view there was at present more scope for increasing production by increasing recovery in the 95 per cent of coarser material and that problem should be tackled first; they also had other ideas for improving the recovery of slimes.

Secondly, the performance of the shaken helicoid had been compared with that of a vanner and round frames, both of which they were beginning to consider as obsolete. They were at present replacing vanners by slime tables, which gave a much richer concentrate and, they believed, a higher recovery. Moreover, vanners were costly to maintain. One very useful piece of information the paper had given them was that the value of the tin recovered by the round frames, even if they were to assume that none was lost in subsequent concentration, was so small that it was doubtful whether their operation was justified in normal circumstances.

A third factor, which was imponderable but should not be overlooked, was that the tests were done with aged material, while machines in the mill were treating freshly-ground ore. It was now widely held, though it had not as far as he knew been firmly established, that freshly-ground cassiterite, particularly in fine particles, was more difficult to concentrate than aged material, probably because of its surface energy which waned with time. That might explain why tin streamers could make a living by re-treating slime tailings which had had time to age during their passage down the river. It might well be that if the tests described had been done with currently produced material in the mill the results would have been different.

Recent experimental work in the mill at South Crofty had confirmed, as Mr. Chaston had said in his paper, that sand-tables would produce a high-grade concentrate and make a good recovery from very fine material, provided it had been deslimed by cyclones, and they were now taking vanners out of the circuit and replacing them with tables which treated a deslimed feed.

Referring to Mr. Rich's remarks, he mentioned that experimental work had in fact been carried out with cyclones during the past two years on an operating dredge.

Dr. A. J. Robinson said that the National Research Development Corporation had attempted for some time, and so far without success, to obtain the support of equipment manufacturers for the further development of the shaken helicoid. Field testing was an important and essential part of the process of developing new equipment, and Mr. Denyer had made it quite clear that, in the case of tin, testing material in which new surface had recently been produced might give results which differed markedly from those obtained when treating old samples. Laboratory testing was normally conducted under ideal conditions, and it was necessary to convince field engineers that new equipment would perform adequately on plant-prepared feed under normal plant-operating conditions. One of the difficulties of laboratory testing was to know when to stop; it was easy to continue test work and to make minor modifications to suit particular feed material without either altering the basic characteristics of a machine or providing any more convincing evidence for those plant engineers who would ultimately use it. Continued efforts would be made to arouse the support of equipment manufacturers and should it be forthcoming to the extent of co-operation in the development and production of helicoids for plant testing, then Warren Spring Laboratory would be prepared to engage in co-operative experiments in the field.

He found Mr. Chaston's figures illustrating the performance of tilting frames and the effect of classification on the concentration of fine tin extremely interesting, but he could not accept the statement on page 220 that the tilting concentrator was at least as good as other machines in the field. No true comparison had been made. Different feed material was employed in the two experiments, and in Mr. Chaston's work middlings were re-cycled, which was not the case in the work described by Mr. Douglas and Mr. Bailey. It was impossible to determine the size recovery figures relating to tilting-frame new feed from the figures presented. In comparing tilting-frame tests nos. 1 and 2 and tabling tests nos. 1 and 2 it was evident that with the tilting frame, as recovery increased the enrichment ratio also increased; with the table, as recovery increased the enrichment ratio decreased. The latter was in accord with normal operating experience; the former was not. Tilting frame performance was remarkable and the speaker asked if that was a reflection of the effect of the re-cycled middling products.

Mr. Douglas and **Mr. Chaston** replied briefly to some of the points raised in discussion, and agreed to make full written replies for publication later.

The President said that they had had a most interesting discussion but unfortunately not enough time to complete it that evening.

He would like to express on behalf of all present their thanks to the authors and to those who had contributed to the discussion of the two papers.

WRITTEN CONTRIBUTIONS

Dr. C. R. Burch: My tabulation (opposite) gives in column A the profile of the mould on which Dr. Sellin made the helicoid that the authors used—'Sellin's helicoid—after Mozley'. As far as I can recall Mozley himself chose this profile and remade the mould when he had seen the pulp run on his first 36-in fibre-glass helicoid. Column B gives the profile to which Mr. O'Keeffe has now reworked the mould, after seeing the pulp run on Sellin's helicoid. (I ought to have had the courage to suggest that it be lower still near the middle.) Column C gives the profile to which we have remade Mozley's first 12-in helicoid mould: this laboratory model has proved useful to Mr. Rickwood of Bristol University Geology Department and his colleagues, whose researches demanded the separation of biotite, garnet or zircon from schists for geochemical work. Concentrates of about 70 per cent were quickly made from elutriated fractions by repeated passage; these were further worked up by other, slower methods.

I would recommend anyone who wishes to make up experimental helicoids of this type to mould the turns thin enough for the outer part to be slightly flexible, and to provide adjustable edge supports, as this allows one not merely to correct to some extent for irregular moulding errors but also to alter the inward slope of the profile appreciably, near the edge, and so to control the gal/min which the deck will take, and also the pulp size-range for which it is best suited.

Secondary circulation—theory

If one is prepared to treat the pulp as a liquid of uniform viscosity,

Helicoid profile

Distance from axis, in.	A 'Sellin's, after Mozley' height, in.	B O'Keeffe's modification	C Mozley's 12-in, modified height, in.
0			0·000
0·5	0·094	0	0·001
1	0·065	0	0·009
1·5			0·015
2	0·055	0	0·019
2·5			0·022
3	0·014	0	0·028
3·5			0·039
4	0·002	0	0·056
4·5			0·074
5	0·000	0	0·096
5·5			0·120
6	0·001	1	0·137
6·25			0·136
7	0·003	3	
8	0·007	7	
9	0·013	13	
10	0·018	18	
11	0·024	24	
12	0·032	32	
13	0·044	44	
14	0·065	65	
15	0·073	73	
16	0·098	98	
17	0·113	113	
17·5	0·120	120	

a rough idea of the secondary circulation can be obtained by calculating the primary flow as though there were no secondary circulation and the secondary circulation as though its existence did not change the primary flow. This is certainly not strictly justifiable since the secondary circulation will effect not merely a radial redistribution of angular momentum throughout the flow, but also (because its existence implies rising and falling flow in at least some parts of the helicoid) a redistribution in an axial direction. However, it will presumably give a rough approximation. The viscous drag between radially adjacent portions of fluid is also neglected and only the drag between axially adjacent portions, in view of the small thickness of the pulp layer compared with its radial breadth, is considered. It is then supposed that the fluid will flow at each radius as though down a straight-slope tangent to the circumferential descent angle at that point. This angle, α, say, has $\sin \alpha = P/2\pi r$ where P is the helicoid pitch, and r the radius considered.

If the local height above the deck be h, or fractionally, yH where H is the height of the pulp layer, then if velocity at H be V_H, and at h, $= yH$, V, and if ν be kinematic viscosity and g the acceleration of gravity,

$$V = y(2-y)\,\frac{H^2}{2}\,g\,\frac{\sin\alpha}{\nu}; = y(2-y)\cdot\frac{H^2P}{4\pi r}\,\frac{g}{\nu}; = y(2-y)\,V_H \quad (1)$$

The centripetal acceleration of this flow is due to the horizontal component, $V\cos\alpha$, only; it is $V^2\cos^2\alpha/r$, so that

centripetal acceleration =

$$g^2\frac{\sin^2\alpha\cos^2\alpha}{4\nu^2 r}\cdot y^2(2-y)^2H^4, = V_H^2 y^2\frac{(2-y)^2}{r}\cos^2\alpha \qquad (2)$$

Each volume element will be subject to the centripetal acceleration plus a radially-inward acceleration $\frac{g\mathrm{d}Z}{\mathrm{d}r}$ associable with the slope of the free surface, the height of which is denoted by $Z, = Z(r)$ and the viscous drag of the fluid above and below it. If u be the radial inward velocity at fractional height y, and β the angle of inward slope of the deck, these three accelerations must balance. That is

$$\nu\frac{\mathrm{d}^2u}{\mathrm{d}y^2}\cos\beta = g\frac{\sin^2\alpha\cos^2\alpha}{\nu^2 r}y^2(2-y)^2H^4 - g\frac{\mathrm{d}Z}{\mathrm{d}r} \quad . \quad (3)$$

with boundary conditions $u = 0$ when $y = 0$; $\frac{\mathrm{d}u}{\mathrm{d}y} = 0$ when $y = 1$ together with the condition $\int_0^1 u\,\mathrm{d}y = 0$.

These conditions determine u and $\frac{\mathrm{d}Z}{\mathrm{d}r}$, giving

$$\frac{\mathrm{d}Z}{\mathrm{d}r} = \frac{6}{35}g\frac{\sin^2\alpha\cos^2\alpha}{\nu^2\cos\beta}\cdot H^4, = \frac{24}{35}\frac{V_H^2}{rg\cos\beta}, = \frac{24}{35}\cdot\frac{H^4}{16\pi^2}\frac{P^2}{r^3}\frac{g}{\nu^2} \quad . \quad (4)$$

and
$$u = \frac{V_H^2}{r\nu\cos\beta}\cdot\frac{H^2}{210}y\,[7y^5 - 42y^4 + 70y^3 - 72y + 32] \quad . \quad (5)$$

which may be compared with the primary velocity

$$V = V_H\cdot y[2-y], = y[2-y]\frac{H^2P}{4\pi r}\frac{g}{\nu} \quad . \quad . \quad (6)$$

From (5) and (6) can be obtained

$$\frac{u}{V} = \frac{\pi}{36P}\frac{r\mathrm{d}Z}{\mathrm{d}r}\frac{[7y^5 - 42y^4 + 70y^3 - 72y + 32]}{[2-y]} \quad . \quad . \quad (7)$$

The ratio of the two y-polynomials is zero at $y \approx 0{\cdot}57$, and may be expanded in the two infinite series

$$\frac{u}{V} = \frac{16\pi}{36P}\cdot\frac{r\mathrm{d}Z}{\mathrm{d}r}\left[1 - 5y + \frac{5y^2}{2}\text{ etc.}\right] \quad . . (y \text{ small}) \quad . \quad (8)$$

and

$$\frac{u}{V} = \frac{-5\pi}{36P}\frac{r\mathrm{d}Z}{\mathrm{d}r}\left[1 + 2(1-y) - \frac{23}{5}(1-y)^2\text{ etc.}\right] \quad . . ((1-y)\text{ small}) \quad (9)$$

That is to say, near the bottom, the layers will flow inwards at angle θ_i from the tangent, where

$$\tan\theta_i = \frac{16\pi}{36P}\frac{r\mathrm{d}Z}{\mathrm{d}r}; \quad . \quad . \quad . \quad (10)$$

the upper layers will flow outwards, at θ_o, given by

$$\tan\theta_o = \frac{5\pi}{36P}\frac{r\mathrm{d}Z}{\mathrm{d}r} \quad . \quad . \quad . \quad . \quad (11)$$

Our helicoid has $P = 2$ in. deck surface inward slope, β, about $0{\cdot}016$ near $r = 13$ in.; if it is assumed that the pulp thickness H is uniform in this region we shall have $\mathrm{d}Z/\mathrm{d}r = 0{\cdot}016$ and

$$\left.\begin{array}{l}\text{Inward } \tan\theta = 0{\cdot}145\\ \text{Outward } \tan\theta = 0{\cdot}0454\end{array}\right\} \quad . \quad . \quad . \quad (12)$$

In order to obtain the azimuthal angle, ϕ, which particles in the lowest layers must traverse to get from the edge to the concentrate zone, near the centre, let t be time, $\frac{r\mathrm{d}\phi}{\mathrm{d}t} = V$, $\frac{\mathrm{d}r}{\mathrm{d}t} = u$, so that

$$\text{Lowest-zone } \phi = \int_{r_1}^{r_2}\frac{V\mathrm{d}r}{ru}, = \frac{9P}{4\pi}\int_{r_1}^{r_2}\frac{\mathrm{d}r}{r^2\frac{\mathrm{d}Z}{\mathrm{d}r}} \quad . \quad . \quad (13)$$

If $\frac{\mathrm{d}Z}{\mathrm{d}r}$ is constant,

$$\phi = \frac{9P}{4\pi}\frac{(r_2 - r_1)}{r_1r_2}\frac{\mathrm{d}r}{\mathrm{d}Z} \quad . \quad . \quad . \quad . \quad (14)$$

If it is supposed that the value $\frac{\mathrm{d}Z}{\mathrm{d}r} = 0{\cdot}016$ holds from $r = 18$ in. to $r = 3$ in., then $\phi = 12{\cdot}4$ radians, $= 1{\cdot}97$ turns.

The outward ϕ is seen to be $\frac{16}{5}$ times longer—i.e. $6{\cdot}3$ turns.

One should hesitate to regard this as anything other than an order-of-magnitude calculation, for it treats the pulp as a uniform fluid with viscosity ν. But it quite obviously is not uniform in viscosity from the centre to the edge. Neither, indeed, is it uniform axially. I think the calculation can be said to show that 3 is a reasonable number of turns—perhaps a bit on the small side. It is not, of course necessary, in order that the edge flow should be barren of recoverable mineral, that it should be composed only of fluid which has migrated from the central region: it is only necessary that the mineral in the edge flow should have dropped into the lower layers and been carried away from the outer region; this part of the flow may then be bled off. Concern is thus more particularly with the inward ϕ rather than the outward ϕ. I would suggest for the next helicoid at least 6 turns with edge-bleed and intermediate radius feed at the end of the second and fourth turns.

The inward velocity as a function of fractional height, y, is the next consideration. This function, y times the polynomial in brackets in eq. (5), rises to a maximum of about $3\cdot6$ near $y \doteq 0\cdot25$; falls to zero at $y \doteq 0\cdot57$ and sinks to -5 at $y = 1$. It is particularly interesting to know the total inflow rate per turn, i.e. $2\pi rH\int_0^{0\cdot57} u\mathrm{d}y$, because this inflowing quantity must rise inside radius r; it allows the mean rising velocity inside r to be deduced:

$$2\pi rH\int_0^{0\cdot57} u\mathrm{d}y = \frac{\pi V_{II}^2}{\nu}\frac{H^3}{105}\int_0^{0\cdot57}[7y^5 - 42y^4 + 70y^3 - 72y + 32]y\mathrm{d}y \quad (15)$$

$$= \frac{1\cdot376\pi}{105}\cdot\frac{V_{II}^2}{\nu}H^3$$

$$= \frac{1\cdot376}{1680\pi}\frac{H^7P^2}{r^2\nu^3}g^2 = \text{Inflow at } r \quad (16)$$

The corresponding primary flow outside r is

$$\int_{r_1}^{r_2}\mathrm{d}rH\int_0^1 V\mathrm{d}y, = \frac{Pg}{6\pi\nu}\int_{r_1}^{r_2}\frac{H^3}{r}\mathrm{d}r \quad (17)$$

If for simplicity it is assumed that H is constant, the primary flow outside r thus becomes

$$\frac{Pg\,H^3}{6\pi\nu}\log r_2/r_1 \quad (18)$$

Combining equations 16, 18 and 4,

$$\frac{\text{Inflow through } r}{\text{Primary flow outside } r} = \frac{1\cdot376\pi^2}{12}\frac{r}{P}\frac{\mathrm{d}Z}{\mathrm{d}r} \quad (19)$$

This inflow must rise between r_1, the inner edge of the pulp band, and r. Therefore the mean rising velocity in this zone is

$$\frac{1\cdot376\pi}{12}\frac{r\mathrm{d}Z/\mathrm{d}r}{P(r^2-r_1^2)}\times\text{Primary flow outside } r \quad (20)$$

Suppose in our helicoid 2 gal/min, = 150 cc/sec, flows outside $r = 9$ in. The resulting inflow must give a rising velocity inside 9 in., by eq. 20, of $0\cdot45$ cm/min—corresponding to the sinking rate of 5μ cassiterite.

This, broadly speaking, is the theoretical justification for expecting that a reasonable recovery of 5μ cassiterite can be made. The criterion may even be too severe, for the maximum rising rate is not developed until

$y \doteq \frac{1}{4}$ and it may reasonably be hoped to carry cassiterite settled in the outer regions (where the fluid is actually falling) at values of $y < \frac{1}{4}$—and so to carry it safely underneath the region of maximum rising rate into a zone of lower rising rate in which it is trapped.

Next it is noticed that

$$\frac{1}{2\pi r}\frac{\mathrm{d}}{\mathrm{d}r}(\text{inflow at } r) = \text{rising rate at } r \quad (21)$$

I am not completely happy about using our approximate expression 16 in the formally exact equation 21, for since the approximation inherent in eq. 16 is admittedly a rough one it does not of necessity follow that its derivate with respect to r will be even a rough approximation in all parts of the range. However, the result of using eq. 16 in eq. 21 is an expression which seems to me reasonable, even when r is made to tend to zero. I am inclined to think that the use of eq. 16 with eq. 21 is permissible.

With this note of caution, from these two equations is obtained

$$\text{Rising velocity at } r = \frac{1\cdot376}{3360\pi^2}\frac{P^2g^2}{\nu^3 r}\frac{\mathrm{d}}{\mathrm{d}r}\left(\frac{H^7}{r^2}\right) \quad (22)$$

It is reasonable near the axis to design the helicoid for a uniform rising velocity irrespective of r. This implies

$$H^7 = A^3r^2(r^2 - r_1^2) \quad (23)$$

where A is a constant having the dimensions of length.

From eq. 4 and eq. 23

$$\frac{\mathrm{d}Z}{\mathrm{d}r} = \frac{3}{70\pi^2}g\frac{P^2}{\nu^2}A^{12/7}r^{-13/7}(r^2 - r_1^2)^{4/7} \quad (24)$$

Z may be obtained as a function of r by graphical integration of the right-hand side of eq. 24 with respect to r; since H is known from eq. 23, $Z-H$ can be found—which is the deck profile. For a certain ratio r/r_1, depending on the choice of A and on r_1, H will become as large as is reasonable—say about $0\cdot2$ or $0\cdot3$ cm max. Outside this region, then, H must be chosen by a different criterion—say, so that $V_{II} \geqslant 30$ cm/sec—and will not have a constant rising velocity. Towards the edge of the helicoid the deck should continue to rise slowly: there should *not* be an upturned lip: experience shows that undesirable standing waves, with a strong secondary circulation of their own, are generated if the outer edge of the pulp runs against an upturned lip. For this reason, Mozley turned the edge of his profile *down*—into a narrow deep gutter, which served the double purpose of containing an emergency overflow and stiffening the edge of the turn.

If r_1 is set at 0 the expression for $\mathrm{d}Z/\mathrm{d}r$ is integrable in finite terms, giving

$$Z = \frac{3}{14}\frac{gP^2}{\nu^2}A^{12/7}r^{2/7} \quad (25)$$

and

$$H = A^{3/7}r^{4/7} \quad (26)$$

with the deck profile, as always, $Z-H$.

This deck profile dips down vertically infinitely near the axis, as it must. H does drop to zero as the axis is approached, and so does the inflow at r. I do not, of course, suggest that this deck profile is usable to $r = 0$: $\cos \beta$ would then be 0, and has in the formulae been replaced by 1. But I do suggest that it may be usable to smaller values of r than one might think—where $\cos \beta$ departs appreciably from 1—say to $\beta = 10°$–$20°$. If equations 25 and 26 are used rather than eq. 23 and graphically integrated eq. 24, H should then become finite, with a finite radial inflow at the inner edge of the pulp band. Physically, this implies a continuous inward bleed of concentrate into a steep gutter adjacent to the central shake shaft (or to the axis, if no central shake shaft is used—one can make the out-of-balance weight in the form of a ring outside the helicoid, at the height of the centre of gravity, supported on three or more parallel cranks, only one of which needs to be driven).

If continuous bleeding in this way is desired, without the use of wash water at the inner edge of the deck, it may be necessary to bleed off a rather larger fraction of the pulp than usually taken as concentrate—one hopes, with an increased recovery.

I think experiments of this type, with repeated modification of the deck shape, after trial, with a view to pushing the concentrate zone ever nearer to the middle, may well prove rewarding.

Bagnold forces

In the preceding discussion the problem has been simplified by regarding the pulp macroscopically as a Newtonian viscous fluid—of viscosity perhaps varying from centre to edge of the helicoid, but still a viscous fluid. At the same time, it is known that the pulp is not turbulent (in the proper sense of the word), and yet it does not settle out on the deck when this is shaken. If even a modest understanding is to be claimed of how shaken helicoids work, the forces by which the pulp is held in suspension cannot be ignored—the Bagnold forces, so called in honour of R. A. Bagnold, who first made detailed experimental measurements of the pressure and traction which are developed across the plane of shear when a suspension of particles in a viscous liquid is subjected to continuous shear. The derivates perpendicular to the plane of shear are the forces, due to shear, which act on each infinitesimal volume element: these (per unit volume) are the Bagnold forces.

Bagnold worked with a gravity-free suspension—a suspension of particles of density 1·000 in water. It may be asked—Can the laws deduced for this suspension apply to quartz in water? Will not the fact of density difference alter the laws? Bagnold himself has shown, in a masterly paper,* that the application to quartz sands, both in water and in air, of the expressions he deduces for pressure and traction across a shear plane, does in fact explain quantitatively a very large number of observations which hitherto could not be explained. He also gives theoretical justification of a quite general character for the use of his gravity-free results, with slight

*Bagnold, R. A. The flow of cohesionless grains in fluids. *Proc. roy. Soc.*, **A 249**, 1956–57, 235–97.

intelligent adjustments to bring in particle and fluid densities in the case of quartz.

Bagnold's formulae can give the explanation of why a classifying cone for coarse sands must have steeper sides than one for fine sands, if it is not to bank; of why the tailing flows off a round frame instead of settling 'according to the principle of area'; of how it is that a non-turbulent slurry can flow, without settling, in a launder, and of why it is that many shaken-bed operations go better in the absence of superfines. Mineral dressers are forced to learn to think in terms of Bagnold forces.

For the comprehensive discussion of steady flow, reference should be made to his paper. Here I am concerned principally with discussing whether, in this case, the effects will be size-dependent or not.

In Bagnold's terminology

C = volume concentration of solids

C_* = maximum possible value of C, $\approx 0{\cdot}74$

$$\lambda = \frac{\text{grain diameter}}{\text{mean radial separation distance}} = \frac{1}{(C_*/C)^{1/3} - 1}$$

D = particle diameter

$\dfrac{dU}{dy}$ = rate of grain shear, radians/sec [Here y is absolute height—for horizontal shear—not fractional height]

σ = particle density

η = fluid viscosity

P = normal stress

T = tangential stress

To quote Bagnold (p. 242):

'All the experimental values of the grain stresses T and P for $\lambda < 14$ obtained by varying λ, η and the rate of grain shear dU/dy were found to conform to a pair of single-valued relationships between two dimensionless numbers

$$N = \frac{\lambda^{1/2}\,\sigma\,D^2\,dU/dy}{\eta} \quad \text{and} \quad G = \frac{D}{\eta}\sqrt{\left(\frac{\sigma}{\lambda} \times \text{grain stress}\right)} \tag{27}$$

. . . N and G have forms analogous to velocity and stress Reynolds numbers. . . . at high rates of shear in terms of N the effects of grain inertia at the encounters make the stresses follow the square law; whereas at low rates of shear they follow the linear law. Within the experimental range of λ the empirical expressions for T in the two extreme regions are

$$T_{\text{inertial}} = 0{\cdot}013\sigma\,(\lambda D)^2\,(dU/dy)^2 \qquad T_{\text{viscous}} = 2{\cdot}2\lambda^{3/2}\eta\,dU/dy\text{'} \tag{28}$$

The lower limit of the transition in terms of $G^2{}_T$ and $G^2{}_P$ is about 100; the upper limits differ, being about 3000 for $G^2{}_T$ and 1000 for $G^2{}_P$.

The stress ratio T/P approaches 0·32 for fully inertial shearing and 0·75 when viscous effects dominate. The corresponding limits for N are about 40 and 450.

Considering the values N may take in our case, it may be supposed that $\lambda = 1$, i.e. $C = 0{\cdot}09$, i.e. 24·3 per cent w/v for quartz pulp; $\eta = 0{\cdot}010$

(our concern is with the viscosity of water, not pulp), in eq. 28. Supposing a primary flow of 2 gal/min = 150 cc/sec between $r = 9$ in. and $r = 18$ in., and guess $\nu = 0{\cdot}04$ for the viscosity of the pulp in eq. 18—then $H = 0{\cdot}32$ cm; from eq. 1 the shear rate near $y = 0$ is $\frac{HP}{2\pi r} g/\nu$; that is at $r = 9$ in. the shear rate dU/dh is about 276 radians/sec, which gives $N \approx 8{\cdot}5$, if $D = 100\mu$. The shear due to primary flow would therefore not be expected to produce a marked size-dependence of lift. The surface velocity of the primary flow is 44 cm/sec at $r = 9$ in.

The part of the shear due to the shake is next considered. Shear waves of pulsatance $p = 2\pi \times$ frequency f attenuate upwards as $e^{-\alpha h}$ where

$$\alpha = \frac{+P}{\sqrt{2\nu}}(1+i);\ i \text{ being } \sqrt{-1}.$$

Thus, if the deck amplitude is Re^{ipt} in the Argand diagram, the velocity is $ipRe^{ipt}$ and the shear rate near the deck surface is $-\,ipR(1+i)\sqrt{\frac{P}{2\nu}}\,e^{ipt}$,

the modulus of which is $Rp\sqrt{P/\nu}$. In our case $p = 20\pi$, and if we take $R = 0{\cdot}15$, $\nu = 0{\cdot}109$, corresponding to $C = 27$ per cent, the shake shear rate near the deck is about 211 rad/sec and the steady-flow shear rate is 145 rad/sec, giving a maximum instantaneous shear rate of 356 rad/sec, or $N \approx 19$. The size dependence of T and P is small for this value of N.

This explains the fact that with the Crofty slime, the larger gangue particles tend to report in the concentrate. That they should so report may be a valuable feature of the helicoid in that the concentrate should be well suited to further concentration by tabling.

The 12-in laboratory helicoid has been used on material of rather more than 100μ; with the Gwithiam beach sand ($0{\cdot}6$ per cent cassiterite locked to hematite and to the usual gangue minerals) the gangue ranges up to 500μ. The shake speed may be as high as 900 rev/min. The largest particles of gangue in this sand report at the outside, and the innermost concentrate fraction contains only fine gangue. In this case N may be 300 or even more.

Messrs. Douglas and Bailey point out that forward velocity and shake velocity are comparable, and they suggest this may explain the concentrating action. This is certainly part of the story: it may accentuate the throwing and washing action which tends to move concentrate inward, due to the forward slope of the deck surface, and consequently the existence of a shake component normal to the deck surface.

(To appreciate the throwing action, lay a penny on a large book: tilt the book surface forward 10°–20° and move the book in a horizontal circular clockwise orbit, increasing the speed until the penny moves. It moves not directly down slope, but obliquely, moving always towards the right.)

The component of the shake normal to the deck is always far below g in amplitude: the component parallel to the deck is about $\frac{1}{2}g$ for 600 rev/min and $R = 0{\cdot}15$ mm. It is not, therefore, a question of the normal component of shake motion throwing the particles up through the water. If they

rise from the deck, this must be due to Bagnold forces. These can be large. In the case $C = 0{\cdot}27$, $\lambda = 2{\cdot}6$, $\eta_{\text{liquid}} = 0{\cdot}01$ the effective viscosity of the pulp is $0{\cdot}109$. (Bagnold justifies the expression $\left(2{\cdot}2\lambda^{3/2} + 1 + \frac{5}{2}C\right)\eta_{\text{liquid}}$ for the effective viscosity.) Bagnold forces on the particles due to shake alone are about $\frac{2}{3}g$; when the steady flow is added to the shake, the Bagnold force-field rises practically to g.

It is quite possible, therefore, for the pulp near the deck surface to dilate and cease to bear on the deck surface about the time when it has shake instantaneous velocity directed towards the axis, and not to recontact the deck until the deck axis had moved orbitally towards it. This means that pulp near the deck surface may travel more directly towards the deck axis than the steady flow of combined primary and secondary circulation. I have seen the rather obvious migration of the largest concentrate particles to the inner edge of the pulp zone. One's impression is that they are being thrown through the water: a throw of velocity V can carry a particle which sinks in free fall at F cm/sec, a distance of the order of F/V cm. For 100-μ quartz or 50-μ wolfram, $F \approx 0{\cdot}7$ cm/sec. The shake orbital velocity is 9 cm/sec: the shake by itself then can hardly throw these particles more than $\frac{9\times 0{\cdot}7}{g}$ cm per cycle—say 70μ per cycle—or $0{\cdot}7$ mm/sec through the water. But once the slip starts, a Bagnold force field is operative on these particles and this field can fall as low as $g/7$ (in the example considered) or rise almost to g. I cannot calculate the rate of dilatation of the bed under these conditions, but I can well imagine that the resultant *washing* in (not throwing of) the particles (because they are lifted into stronger inward currents at a favourable time in the cycle) may be much more important than throwing through the water, and I am indebted to the authors for drawing my attention to the fact that shake velocity and forward velocity are comparable, and that this could be important.

Dr. E. J. Pryor: My immediate reaction on study of the paper by Messrs. Douglas and Bailey is one of caution. Are likes being compared when performance of a Frue vanner in plant operation is noted against that of Dr. Burch's shaken helicoid under laboratory conditions? In this the question of the colloidal content of the water used in the two cases might be significant. Cornish tin ores on grinding release iron minerals which rapidly impregnate the pulp with somewhat gelatinous iron slimes. This is one of the factors leading to the success of tin streamers in making further recovery of cassiterite from tailings discharged into the local rivers. Even today, the ochreous stain carried down from the Red River to the outlet at Gwithiam shows for miles, despite the reduced volume of tailings from post-war mining. I have repeatedly observed the importance of copious use of clean water in preparing slimed tin for recovery by gravity treatment.

A purely gravitational approach to recovery of this -40-μ cassiterite at the research level seems, on the whole, likely to be unrewarding. The upgrading produced in these tests, even if it could be repeated when using contaminated mill water, cannot lead to a break-through in this problem of improving efficiency. Leaving out alluvials, with which the paper is not concerned, higher recovery appears to me to be attainable by the following further study:

(*a*) closer attention to comminution of the ore; this would aim at staged classification and reduction of overgrind, since the simplest way to avoid slimes losses is to avoid making slimes;

(*b*) flotation research aimed *either* to float cassiterite *or* to float gangue from a low-grade concentrate which had been stage-produced during staged classification;

(*c*) intensive study of simple buddling, in the light of modern knowledge of surface physics and chemistry;

(*d*) further exploration of the possibilities of chemical extraction, applied to low-grade concentrates.

From such a broad study-front might emerge a modified grinding plan combined with staged removal of gangue until a concentrate carrying from 5 to 10 per cent of tin had been made. This would give a possible economic feed for a high-recovery use of well-known methods of chemical extraction.

However ingeniously mild centrifugal force is applied to -20-μ particles, and however carefully hill-and-valley developments are minimized in the sluicing system, we are up against an 'either/or' incompatibility in gravity treatment. Either we can have high recovery on an uneconomically small tonnage or low recovery on a substantial feed. Only by getting away from this by finding an approach which will avoid sliming at the earliest stages of upgrading can we examine the whole problem afresh. Economics are interlocked here with techniques, and the side-effects of the iron hydrating into the mill water may well prove a major obstacle, rather than the specific gravity–size relationships of the minerals concerned.

One feature of the flotation process not sufficiently recognized is that by its tendency to remove slimes with the froth it makes possible the recovery by tabling of minerals from the flotation tails which would not have been recovered from the heads. It is only necessary to watch a miniature table at work monitoring the quality of recovery from the tailings end of a bank of cells to see the change in behaviour of the minerals—a change mainly due to the greater freedom of movement they possess once the 'felting' effect of their entangled slimes has been reduced.

In making this contribution I cannot foresee these suggestions having much value in slime recovery from alluvials where the head value is too low to encourage the extra cost.

Mr. Donald Gill: My discussion of the paper by Messrs. Douglas and Bailey is limited to the numerical results presented and is not to be construed as in any way critical of the apparatus. Some of the numerical results are hard to understand and it is hoped that the authors may be able to give supplementary information in their reply. In view of the great interest in the use of gravity concentrators for fine pulps, it is important that the numerical results presented should be firmly established.

Sampling.—It is supposed that the large samples of material tested (e.g. vanner feed and tails, and round-frame feed and tails) were sent to Warren Spring in steel drums. Since the sampling of such material can be a tricky operation, could the authors put on record the precise method used for obtaining 'laboratory size' samples for assay, sizing-assay tests, etc.?

Assaying.—In the last two lines of Tables I, II and III (pp. 650, 651, 654) there are differences between 'assay head' and 'calc. head'. As quite small assay differences may be of importance when dealing with such low-grade material, it would be helpful to know, first, whether all the assay results appearing in the paper were done in the same laboratory under similar conditions and, secondly, what differences the authors would expect between replicate assays of the same sample of the approximate grades concerned (say, between 0·3 and 0·6 per cent Sn).

Sizing-assay tests.—Are the sizing-assay tests, as done by the authors, of a 'reproducibility' as good as those cited by Mr. Chaston (p. 218)?

Vanner feed and tail (*Table I*).—I have shown the size distribution of the feed and tail, taken from Table I, in Fig. A. It is evident that the tail is a finer product than the feed, to an extent considerably greater than would be expected from the relatively small amount abstracted as concentrate—say, 5 per cent or less.

If any reliance can be placed on the sizing tests, one is driven to the conclusion that the operation of the vanner was not stable during the period of sampling; that is, coarse material was building-up on the vanner belt, and the period of sampling was not long enough to 'iron-out' such changes in coarseness (and, no doubt, assay also) of the bed of material on the belt. For how long a period was the sampling continued to obtain the samples represented in Table I? In this machine what is the time taken by the belt to travel upward from the point of tailings discharge to the feed point? In other words, how many times during the sampling was the material on the vanner belt 'turned-over'?

Round frame feed and tail.—Fig. C shows the size distribution of feed and tail, taken from Table III. In this case it is the tail which is (or appears to be) a significantly coarser material than the feed, at all sizes above about 15μ. There is no question of bed changes in this machine. Again, for how long a period was the sampling continued to obtain the samples represented in Table III, and how many revolutions of the machine does this period cover?

Another very curious thing about this table is that two of the tail fractions, namely the $-36 +24$ and the $-12 +6$-μ fractions, are very notably richer than the corresponding feed fractions. I find this difficult to accept if the sampling was reliable!

Helicoid feed and tail.—Fig. B shows the size distribution of feed and tail, taken from Table II. It will be noted that (probably within the limits of experimental error?) the two graphs lie very close together and may well be quite sufficiently accurate representations of the two materials.

If the authors do not accept my suggestion of inadequate sampling

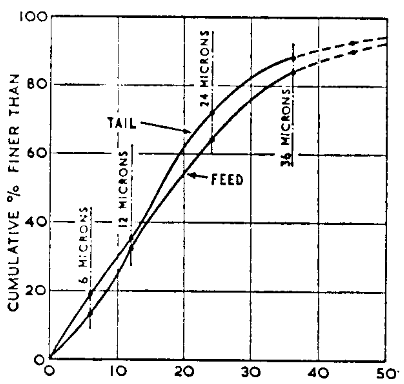


Fig. A.—Size distribution of vanner feed and tail (from Table I).

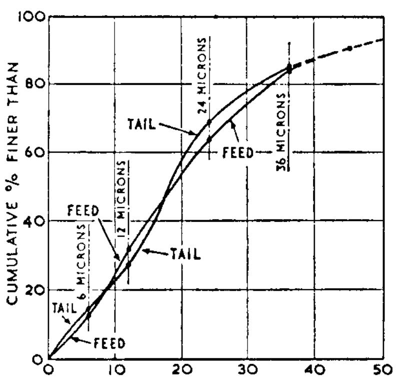


Fig. B.—Size distribution of helicoid feed and tail (from Table II).

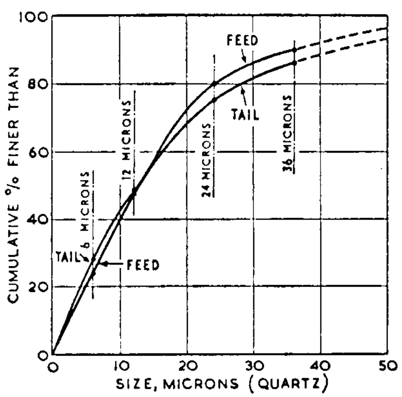


Fig. C.—Size distribution of round frame feed and tail (from Table III).

periods for the samples of Tables I and III, can they suggest any manipulation that the relevant samples could have suffered to account for the differences in coarseness between feed and tail—*in different directions in the two tables*?

Performance graphs at 1-μ intervals—Figs. 19 (vanner), 21 (helicoid) and 22 (round frame) (p. 652).—The authors state (p. 651) that 'these results have been used in constructing Fig. 19' and (bottom of p. 653) 'these results have been used to plot tin distribution curves (Fig. 22) for the feed and concentrate from this machine', these two quotations referring to the vanner and round frame respectively, and to results from Tables I and III respectively. It is not definitely stated how Fig. 21 is derived, but the implication is that it is derived from Table II. A more detailed explanation of the computations leading to the construction of Figs. 19, 21 and 22 would be welcomed. If the three diagrams are derived solely from the data of Tables I, II and III, then, surely, *perfect sampling, sizing and assaying* are a pre-requisite? In view of the discrepancies noted above, do the authors still maintain the correctness of these graphs, especially, of course, the lower graph in each figure, relating to the concentrates?

Batch treatment (Table IV, p. 655).—What was the dry weight of feed processed in the rougher stage (stage 1) of this test? In other words, at the normal rate of feed of about 110 lb/h what was the duration of the rougher stage? In the cleaning stage (stage 3), where the feed was only about 13 per cent by weight of the rougher feed, what was the duration of the test?

Can the authors offer any explanation for the difference between the 'Rougher head assay' (0·41 per cent Sn) and the 'Calculated rougher head' (0·45 per cent Sn)? Could it be due to the formation of the bed of relatively low-grade 'silt' on the deck of the helicoid (p. 648), no account of which is taken in Table IV?

In the cleaning stage (stage 3) how was the bed of 'silt' formed? Was it 'taken over' from a previous stage or was it formed from the material under test? In either event, with a relatively small quantity of material involved, it could have affected the result very significantly.

Tables V and VI (p. 656).—There is a misprint in Table V, where the units for cleaner concentrate should be 0·059 and not 0·59 as printed.

In Table VI the authors present a rather optimistic picture, by which the overall recovery is raised from 13·1 to 38·9 per cent without any change in concentrate grade, merely by recirculation of the cleaner tail. It is evident from the last four lines of page 655 that there are no experimental data to support this estimate.

Mr. F. A. Williams: It appears that for the recovery of fine cassiterite the shaken helicoid will have to compete with tilting concentrators or fine sand tables, although when the valuable mineral is magnetic, e.g. wolframite and columbite, the Jones wet magnetic separator might prove to be competitive with all three. Furthermore, it might be found that low-grade concentrates from one of these machines might best be up-graded on one of the others or by a different process altogether, such as the use of TBE.

Messrs. Douglas and Bailey were restricted to a limited test programme, but I do not think that that has done full justice to the potentialities of the shaken helicoid. I think that the tests should now be repeated with feeds which have been deslimed by cycloning.

On page 640 of their paper the authors state: 'Throughout these tests, knowing the limitations involved, chemical assays have been used to determine the values in the various samples'. I would like to discuss this aspect first.

Physical and chemical assaying.—In developing the shaken helicoid, before handing it over to Warren Spring Laboratory for independent testing, Dr. Burch and his associates used a physical method for analysing the samples, Dr. Burch contending that, in this type of research investigation, the physical separation of a concentrate from the sample followed by chemical assay to check its grade is more usefully informative than the direct chemical assaying of the original samples. I certainly agree. All that chemical assaying of the original samples does is to determine with great accuracy the information which is *not* required, i.e. the amount of a particular element present irrespective of the mineral form in which it occurs or the degree of release of the mineral. The samples of Hawk's Wood wolframite slimes chemically assayed in the test programme at Warren Spring must have contained intergrown grains over the whole specific gravity range 2·7 to 7·4, but chemical assays make no distinction between locked and released mineral. The need for this distinction also crops up on pp. 224 and 225 of Mr. Chaston's paper, where he refers to locked tin.

In Nigeria centrifuging in bromoform has been used for the separation of —300-mesh deslimed samples. At Warren Spring Laboratory accurate gravity fractionation of samples of fine-grain size from the helicoid investigation might have been undertaken by centrifuging in a series of heavy liquids. The size–density fractions could then have been chemically assayed. By this combination of physical and chemical methods wolframite-bearing gangue too light for gravity concentration could have been eliminated and the nature of the middlings could have been more adequately studied.

It is important to be able to distinguish the relative extent to which samples of middlings consist of (*a*) a mixture of free grains of light gangue and fully released mineral representing imperfect plant performance which can be tackled without regrinding, and (*b*) intergrown grains rightly reporting in the middlings because of their intermediate specific gravity and for which regrinding is necessary. Chemical assaying without physical assaying is responsible for much unnecessary overgrinding in many mills.

Applications.—I am interested in trying to assess the prospects for the profitable application of the information contained in both of these papers to the treatment of a number of different ores, particularly the intensely decomposed columbite-bearing granites of Nigeria from which cassiterite, xenotime and a magnetic zircon are also produced. The methods of sample valuation originally used have already been described.[1] The problems of recovering wolframite and columbite are very similar. Both minerals are magnetic. Wolframite readily cleaves into flakes and the habit of much of the columbite is tabular and acicular. These shapes adversely affect recovery by gravity concentration. In the following tabulations I have assembled some data regarding grain-size range and throughput capacities, which have an important bearing on the selection of the most suitable concentrator.

[1], etc. See list of references at the end of this contribution.

Application of the Jones wet magnetic separator for feebly magnetic minerals to some Canadian mineral dressing problems[3]—*Grain size range*

Experiment no.	*Mesh size of feed*	*Separations effected*
1	−28 +65	Garnet from quartz
2	−28 +150	Iron oxide stained quartz from sandstone
3	−20, 9% −325	Green mica including grains with inclusions from granite
4	−48, 36% −325	Basalt and magnetite from zeolites
5	−65 +100	Iron oxide stained flakes from graphite concentrate
6	−200	Magnetite and ilmenite from apatite
7	10% +100, 53% −325	Biotite from kyanite
8	−28 +400	Iron-bearing minerals from mixture of quartz, kyanite and feldspar
9	6% +65, 33% −200	Hematite and ankerite from quartz
10	−100 to 5μ	Hematite and magnetite from quartz
11	99·9% −325	Beneficiation of talc by removal of iron-bearing fraction
12	95% −325	—ditto—

Throughput capacities

Concentrator	*Capacity, lb/h*
Shaken helicoid, single	70
Shaken helicoid, 17-machine unit	900
Tilting concentrator	4500/1800
Fine shaking table	220
Jones wet magnetic separator, machine comprising four units	13 000

Intensely decomposed columbite-bearing granites.—Three companies are actively engaged in working intensely decomposed columbite-bearing granites on the Jos Plateau, Nigeria. All use plants incorporating hydrocyclones, jigs and shaking tables, and, although differing considerably in detail, for the purposes of discussion the plants can be represented by the left-hand half of the generalized flowsheet shown in Fig. A. It is the overflow of the secondary hydrocyclones, at present going to waste, which might now be considered for further treatment. This overflow will contain most of the ultra-fine slime, as well as fine 'sand' in the original feed to the plant, in the form of a fairly dilute pulp. It may or may not be valuable enough to justify the cost of pumping it through tertiary hydrocyclones, but if it is, then data available from the original valuations of the granites suggest that there is a high probability that the underflow would prove to be worth concentration.

In this connexion I would like to ask Messrs. Douglas and Bailey the approximate cut off recovery grade for a vanner feed. According to their paper a shaken helicoid gives an appreciably better performance than a vanner. Table I (p. 650) shows a vanner feed containing 0·41 per cent Sn yielding a tailing still containing 0·33 per cent Sn. This is presumably a payable recovery or else the mine would not have been treating this material. Available data indicate that the recoverable monetary value of columbite and associated saleable minerals in the tertiary hydrocyclones underflow might be up to ten times that of the tin recovered by the vanner. This is encouraging, but the plant-scale dressing of this complex concentrate at such a fine size range would probably be difficult.

Mr. Chaston used a 6-in hydrocyclone to deslime the feed to the tilting concentrator. Van der Spuy[2] has described the successful plant-scale use of a battery of 50-mm porcelain hydrocyclones fed at 24 lb/sq.in to obtain a fine split in a modernized mill for the concentration of lode tin ore in South Africa. In Nigeria hydrocyclones of 75-mm diameter fed at 8 lb/sq.in and 30-mm diameter fed at 40 lb/sq.in for desliming alluvial bore samples have been used. The latter gave a split which was rather too fine even for superpanning, but this split is probably not too fine for recovery in a Jones wet magnetic separator. This is indicated by the entries 11 and 12 in my first table compiled from a paper by Stone,[3] the following being an excerpt from the concluding remarks in a later paper by the same author[4]: 'It has been shown that the Jones separator constitutes a major breakthrough in magnetic separation in general. . . . In addition to being applicable to iron ore, the Jones separator can concentrate a wide range of other minerals down to the micron size range. These include, garnets, uranium, germanium, ilmenite, chlorite, mica, marmatite, columbite, wolfram, manganosiderite, iron-bearing silicates, pyrrhotite, etc.'

The types of concentrator which might be considered for dealing with the tertiary cyclone underflow are shown in Fig. A. In making a selection between the first four, a number of factors, including capital cost, space occupied, capacity, specific power consumption, maintenance and recovery performance, would have to be taken into consideration. In my second tabulation some comparative data on capacity are shown and the final flowsheet might include more than one of the machines from this selection.

The results listed in my first table, when compared with recovery data given in the papers under discussion, suggest that effective wet magnetic separation might extend to rather finer grain sizes than gravity concentration. However, the magnetic concentrate is likely to be of rather low grade, because, in addition to the four valuable minerals columbite, cassiterite, xenotime and zircon and associated resistant heavy accessory magnetic minerals, it would also contain a large excess of biotite and particles of quartz and feldspar with inclusions of magnetic minerals or just iron-stained particles. If this heavy low-grade concentrate could not be up-graded satisfactorily on any of the three gravity concentrators listed, which, for tilting concentrators, is rather suggested by Mr. Chaston's experience with the sulphide-rich ore, then cycloning in TBE might have to be considered to achieve some upgrading by rejecting a light fraction.

Cassiterite/wolframite lode ores.—The essential similarity between concentrating comminuted cassiterite/wolframite ores and the disintegrated

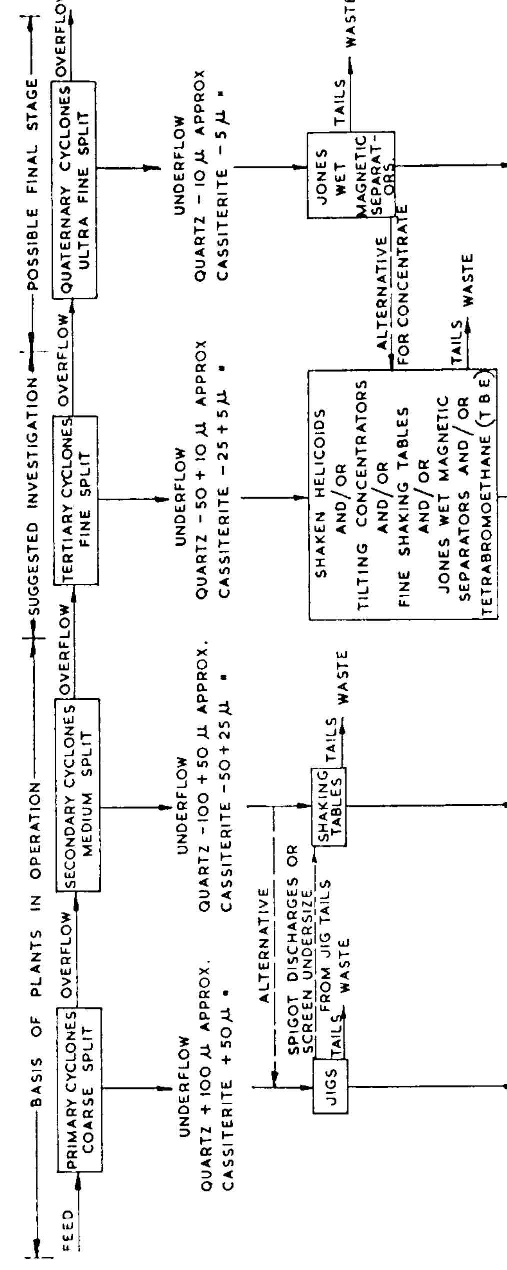


Fig. A.—Generalized flowsheet for decomposed columbite-bearing granite.

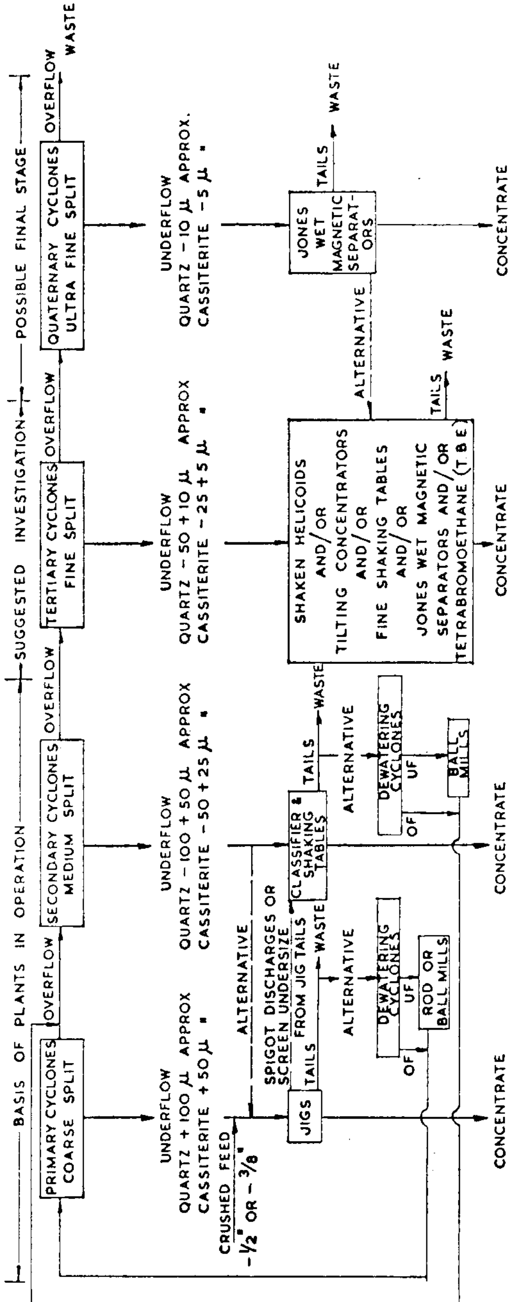


Fig. B.—Generalized flowsheet for crushed cassiterite/wolframite ores.

decomposed granite can at once be seen from Fig. B. There are considerable differences between existing wolfram mills, particularly in the survival of outmoded practices which lead to overgrinding. Modernization is towards the type of flowsheet shown in the left-hand half of Fig. B. This circuit minimizes overgrinding. Nevertheless, because wolframite cleaves so readily and cassiterite is rather brittle, a considerable proportion of each is usually present in the so-called slimes discharged to waste, and this still constitutes a major problem in the wolframite mining industry.

Cassiterite lode ores.—By deleting magnetic separation the generalized flowsheet in Fig. B becomes adapted to a crushed ore containing only cassiterite as depicted in Fig. C. The modernization of a tin mill in South

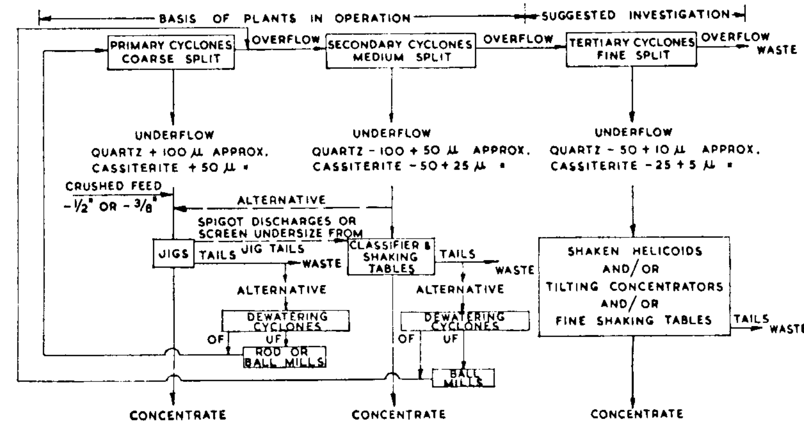


Fig. C.—Generalized flowsheet for cassiterite ores.

Africa along similar lines has already been mentioned.[2] The capacity of this mill was increased by 27 per cent, recovery by more than 10 per cent, and the number of round frames was reduced from 13 to 3, and these might conceivably be eliminated by installing shaken helicoids.

Alluvial tin deposits.—The original Nigerian plant, which eventually incorporated hydrocyclones, jigs and tables, began operating with decomposed granite in July, 1953. In November, 1956, after the bottom had temporarily fallen out of the columbite market, this plant was simplified for treating material from a nearby alluvial lead. I have already described these modifications.[5] The dual cyclone system was retained, but, in order to reduce operating costs, the secondary cyclones were re-erected at a lower level so that they could be fed by gravity. It was found that there was not sufficient cassiterite (and columbite) in the secondary hydrocyclone overflow to warrant the use of tables. Revaluation of many alluvial deposits in Nigeria by reliable physical methods has indicated that, although there is much more fine cassiterite (and columbite) in the ground than was formerly known, it is not sufficiently fine to warrant the use of shaken helicoids and rarely even shaking tables. A high recovery can be made with hydrocyclones and jigs alone.

In 1961, Sheahan[6] proposed that a dual cyclone system, also with gravity feed to the secondary cyclones, should be used on tin dredges in Malaya. As an alternative to jigging the secondary cyclone underflow he proposed the use of spirals. I suggest that, subject to the results of further tests and provided there is enough very fine cassiterite in the ground, shaken helicoids might be considered as an alternative to spirals, although they might prove to be too sensitive to slight tilting for use on dredges. These alternatives are shown in the generalized flowsheet for alluvial tin deposits in Fig. D.

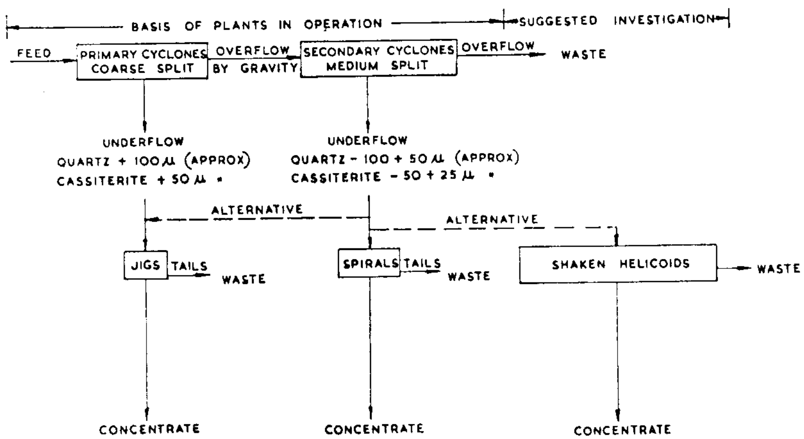


Fig. D.—Generalized flowsheet for alluvial tin deposits.

On the basis of chemical assays Sheahan[7] has recorded: 'The true size distribution of free cassiterite has been determined for a great many samples of Malayan material. Peak values usually lie in the 30–100-mesh B.S. range with secondary peaks sometimes found around 100–200 mesh and -300-mesh$+20\mu$ ranges frequently present.' This has yet to be checked by a comparable number of physical assays of samples. If confirmed, the -300-mesh$+20\mu$ fraction would present a potential need for shaken helicoids.

Research.—To a greater or lesser extent, all the major tin-producing countries of the world are faced with the problem of the loss of very fine cassiterite and any valuable associated minerals. The tin-mining industry might, perhaps, seek solutions to this problem by supporting the necessary research on an international co-operative basis. At the Tin Research Institute the programme is directed entirely to developing the market for tin but as yet there is no corresponding support on an international basis for research on the recovery of the original cassiterite.

REFERENCES

1. WILLIAMS, F. A. The identification and valuation of decomposed columbite-bearing granites of the Jos-Bukuru Younger Granite Complex, Nigeria. *Trans. Instn Min. Metall., Lond.*, **65**, 1955–56 (*Bull. Instn Min. Metall., Lond.*, no. 591, Feb. 1956), 169–79.
2. VAN DER SPUY, R. C. M. The treatment of tin ores in the Rooiberg concentrator. *J. S. Afr. Inst. Min. Metall.*, **61**, May 1961, 468–88.
3. STONE, W. J. D. Wet magnetic separator for feebly magnetic minerals. II. Application to some Canadian mineral dressing problems. *International Mineral Processing Congress, 1960* (London: Institution of Mining and Metallurgy, 1960), 733–43.
4. STONE, W. J. D. Application of the Jones wet magnetic separator to the beneficiation of iron ore. *Trans. Canad. Inst. Min. Metall.*, **64**, 1961, 404. (*Canad. Min. Metall. Bull.*, no. 593, Sept. 1961, 686.)
5. WILLIAMS, F. A. Performance analyses of screens, hydrocyclones, jigs and tables used in recovering heavy accessory minerals from an intensely decomposed granite on the Jos Plateau, Nigeria. *Trans. Instn Min. Metall., Lond.*, **67**, 1957–58 (*Bull. Instn Min. Metall., Lond.*, no. 613, Dec. 1957), 89–108.
6. SHEAHAN, P. M. A proposed dual cyclone system for Malayan dredges. *Min. J.*, 256, Feb. 10 1961, 146–7.
7. WILLIAMS, F. A. Recovery of fine alluvial cassiterite: correlation of bore valuations with plant-scale recovery. Contributed remarks to discussion by P. M. SHEAHAN, *Trans. Instn Min. Metall., Lond.*, **70**, 1960–61 (*Bull. Instn Min. Metall., Lond.*, no. 651, Feb. 1961), 317.

Mr. I. R. M. Chaston: I note from the paper by Messrs. Douglas and Bailey that the feed rate was kept constant at 8 l/min and with very low pulp densities this would mean that the rate of solid feed to the helicoid would be very small. Could the authors say if the same type of bed built up on the helicoid with the low pulp density feeds as occurred in the other tests? The surface presented by a slow-moving bed of material would certainly give very different concentration conditions than would the actual fibre-glass surface of the helicoid and any change or discontinuity in the sand bed surface could be responsible for the anomalous recovery figures for the dilute pulps.

The tests comparing the operation of the helicoid under laboratory conditions with the machines used in practice in Cornwall are interesting. I assume that the size fractions represented in Tables I, II and III (pp. 650, 651, 654) were obtained by some form of elutriation and that the sizes refer to equivalent quartz spheres. Perhaps the authors would describe the exact sizing method employed. The tables would be much more interesting and useful if they had been extended to include the concentrate sizing and assays. These additions might go some way to explain the obvious anomalies between the size and analyses of the feeds and the tailings in the separate tests, which, while they might be expected in the samples taken from operating machines, are rather surprising in careful laboratory tests performed in closed circuit. These somewhat haphazard sizing tests are the more remarkable when they are compared and, indeed, used to construct Figs. 19, 21 and 22, and I am eager to learn how the authors sized the cassiterite in these low-grade concentrates micron by micron with such accuracy that they can indicate that there was no recovery in the 9-μ size for the vanner concentrate or at 10 or 11μ for the helicoid concentrates. Some indication of the actual test results for sizing would be of great interest.

Mr. F. B. Michell: The helicoid performance results presented by Messrs. Douglas and Bailey are certainly a considerable improvement on those quoted for vanners on the same material, but I am perturbed as to the influence of possible dissimilar conditions in the two operations. Small variations in conditions can affect recovery, for example, the circulation of the pulp for three-quarters of an hour may cause attrition, producing either more near-colloidal material or altering the surface of the mineral particles. Temperature also has a marked influence on separation. If a flowing film is considered and attempts are made to calculate the distance needed for a particle to reach the concentrating surface, it will be found that it requires twice the distance at 5° C than at 35° C. Was the pulp temperature approximately the same in the plant and in the tests? In addition, were other conditions identical? For example, was the pulp sample taken with the appropriate amount of accompanying liquid or was it thickened for transport and re-diluted at the laboratory? If there were any difference in the pH value conditions could be markedly changed and 'aging' of the 'pulp' could have an effect.

I cannot agree that the difference in the pulp densities of the two operations can be disregarded. In the case of the helicoid, the pulp appears to contain between 10 and 11 per cent solid (Fig. 20, p. 652) and the operation is compared with a vanner having a feed containing 25 per cent solids. According to Fig. 11 the enrichment on the helicoid with a feed containing 25 per cent solids is about 3·4 compared with some 6–7 at 10 per cent solids. Recovery, however, is shown to be little affected. If either the concentration efficiency or the concentration index is calculated by a standard method,* the following figures are obtained which shows the helicoid to be markedly inferior at 25 per cent solids.

Machine and pulp density of feed	*Recovery* %	*Concentrate* %	*Enrichment*	*Concentration efficiency*	*Concentration index*
Vanner, 25 per cent .	19	4·95	8·5/1	1·16	1·42
Helicoid, 10 per cent .	30	3·77	6·5/1	1·34	1·65
Helicoid, 25 per cent .	31	1·97	3·4/1	0·62	0·745

Note: Feed is assumed to contain 0·58 per cent Sn and the maximum grade of concentrate to assay 72 per cent Sn.

It is true that the authors argue (p. 653) that the relatively low pulp density of the helicoid is offset by the wash-water requirements for the vanner. I cannot agree, however, since 500 lb/h at 25 per cent solids is represented by 2000 lb of pulp, while at 10 per cent solids the amount is 5000 lb of pulp. A vanner might use a maximum of about 1·25 gal/min or 725 lb/h (the actual consumption on the remaining vanners at South Crofty mine is 0·21 gal/min), but it is most unlikely to consume 5 gal/min which would be necessary to provide a total flow of 5000 lb of pulp per

*Concentration efficiency $E_c = \frac{c-h}{c_{max}-h} R$ and $I_c = \frac{c}{h} - 1 \frac{R}{100}$

STEVENS, J. R., and COLLINS, D. N. Technical efficiency of concentration operations. *Colo. Sch. Mines Quart.*, **56**, no. 3, July 1961, 483-508.

hour. It is possible, therefore, that the recoveries on a vanner and helicoid are really comparable. This of course does not detract from the obvious merits of the helicoid in respect of power and space requirements as well as the provision of greater shear which should aid separation when there is a high percentage of near-colloidal material.

Turning to the figures in Table I (p. 650) and the curves on page 652, a difference of 0·02 per cent Sn in the assays can make a considerable difference to the slope of the curve and when there is little rate of change this affects the first differential curves very appreciably. Was the method used for assaying the product of an accuracy adequate to show up such differences?

With regard to the round frame performance, I presume the samples were taken from the concrete fixed-bed machines. If so, it is only fair to point out that these frames 'channel' and are less efficient than the revolving round frame, and the results are not representative of the efficiency of a round table.

It would seem that one of the merits of the helicoid lies in its ability to separate near-colloidal material by virtue of the shear forces, whereas the removal is much less complete when using a vanner. This effect can be seen when using a vanning shovel. 'Slime' separation takes place during the circular motion and in subsequent dilution and decantation, after which subsequent concentration, when the high sp. gr. minerals are 'thrown up', is carried out with greater facility.

The value of prior removal of such near-colloidal material by using cyclones can be marked. In 1957 I reported a test, carried out at Geevor mine* a few years earlier on a feed containing 38·4 per cent −25μ and 2·6 per cent +300 mesh in which 74 per cent of the tin reported in the underflow which carried only 6·8 per cent of −25μ (quartz). Concentration of this underflow yielded recoveries ranging from 69·8 to 77·7 per cent with enrichments of 25/1 to 17/1. Neglecting the tin in the overflow, the tin recovery is about 55 per cent.

Similarly, on another mine, desliming in front of conventional round frames showed *overall* recoveries of 49–50 per cent after desliming, as compared with 33–35 per cent without desliming. In this case the feed was treated in 50-mm cyclones when 50 per cent was rejected as overflow. The feed assayed 0·47–0·6 per cent Sn and contained 27 per cent −4μ and 16 per cent above 20μ.

The lesson is surely obvious—the colloidal slime must be removed. In fact I suggest that the steep slope on the fine limb of the first differential curves for a round frame (Fig. 22, p. 652) may be due solely to the interference of near-colloidal material.

Turning to Mr. Chaston's assertion that the double-recovery phenomena are not found with a table, our observations also confirm this fact. I think it likely, however, that a lower penetration rate of middling size particles might be observed in a deep bed within the riffles, although I am aware

*MICHELL, F. B. The concentration of fine cassiterite by gravity methods. *Trans. Cornish Inst. Min. Engrs*, **12**, 1956–7, 56–77.

that Kirchberg did not find it. Since it is extremely rare to find a truly fully riffled deck, I think this may account for a predominance of the effect of a flowing film and deck acceleration on the resulting concentration with an absence of the double-recovery effect. I note the author says the table used was a fully riffled one. This is unusual and even a James table shows a small unriffled area owing to the diagonal shape, while the riffles are extremely thin and widely spaced relative to the depth. In fact, on a slime deck, a double concentrate band is not uncommon and Mr. Suvarnaplatip has recently shown that the table concentrate exhibits a single peak in the weight of tin per micron against tin size curve, but that the double peak is found in the middling. This leads one to think that the double-recovery effect only occurs where there is a thicker bed, more shear in the film and less flowing film stratification.

Turning to the possibility of altering the surface charges on particles, I think this has distinct potentialities and, indeed, recent research at the Camborne School of Mines has shown that a pronounced effect is brought about by adsorption on both mineral particles and the concentrating surface. Mr. O'Keeffe has found that when he measured the effect of a flowing film on certain small mineral particles, changes in pH and the presence of metal cations had a pronounced influence. It would appear that best recoveries are obtained generally when the repulsive forces are small, that is, when zeta-potential values are low. The tendency to flocculate reduces the efficiency of concentration in a flowing film but unfortunately deflocculated conditions are often synonymous with high zeta-potentials. Consequently, although the enrichment may be improved, the recovery of the finer size particles suffers. It must also be remembered that high zeta-potentials produce electro-viscous effects which probably hinder the separation.

Aging of concentrating surfaces has been observed to influence efficiency and this is also probable dependent on the sorption of cations from the pulp. In view of these observations and since 'slime coating' is bound to affect mineral surfaces, it would appear theoretically most desirable to eliminate near-colloidal slime and then proceed to effect concentration of the 'deslimed' pulp under optimum conditions for this material, a practice which is in agreement with observations in the plant.

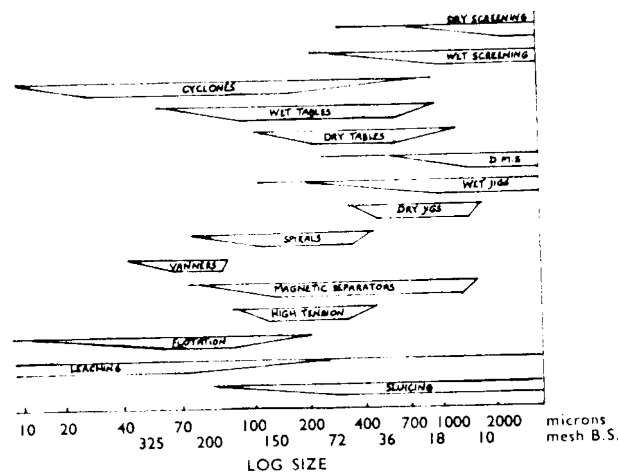
Mr. F. Hutchin: During the past 35 years I have seen the decline in the number of tin streams operating on the Red River (from Camborne to Gwithiam), leaving only two working today. The overall recovery made by the tin streamers, or the slimes plant of a tin mine, is low. The feed to a slime plant is low in value, consisting of cassiterite in the range of 100μ down to 10μ, and less, with fine sand, arsenopyrite, chlorite, iron oxides and large quantities of colloids. The tin streamer, nevertheless, by his skill in dressing, takes material from 0·5 per cent Sn up to a saleable concentrate, so that any device that can help him is worthy of consideration. Many types of surface, on an inclined plane, have been tried for the retention of finely-divided cassiterite, such as, wood, glass, rubber, linoleum, concrete, as also has the pre-conditioning of the pulp by adding chemicals, with some slight measure of success. With the shaken helicoid fibre-glass is

used. However, the concentrates recovered from the helicoid at 9·2 per cent Sn or from the Frue vanner at 4·95 per cent Sn, have still to be upgraded to a saleable concentrate of 30–50 per cent Sn, and here again losses will occur.

The feed to the vanner contains a large amount of colloidal material which is detrimental to the recovery of cassiterite; the grains of cassiterite are coated with either limonite, hematite or clay. Also the colloidal suspension prevents the free gains of cassiterite from settling on the inclined surface, and they are carried off in the wash water into the tailings. The coating of limonite or hematite round the cassiterite grains prevents it from adhering to the surface and they roll off the table into the tailings. From experiments carried out at South Crofty on slimes it has been found that by cycloning the pulp the colloids and iron oxides are sheared off the cassiterite grains, and the underflow of the cyclone gives a cleaned feed which can be tabled on a James slime table, from which concentrates are recovered assaying 35–40 per cent Sn, and the tailings of the table, at 0·25 per cent Sn, are lower than those from a Frue vanner treating uncycloned feed. The ratio of upgrading has by this means been greatly increased in one operation.

The shaken helicoid has on the test run given a slightly better performance than the Frue vanner, and it has the advantage of taking less space, but in my view it does not yet equal the James slime table or the Denver-Buckman tilting frame.

Mr. M. P. Jones: Since the war the concentration of fine-grained cassiterite has been (wholly, or in part) the subject matter of ten papers read to the Institution. Nine of them dealt with various forms of gravity treatment and one—the earliest—advocated the use of flotation. The diagram below shows the effective size-range of various mineral dressing



Size range of application of various mineral dressing methods.

processes and it can be seen that 'fine-grained' cassiterite, i.e. $-30\text{-}\mu$, is too small for efficient gravity concentration. I am surprised, therefore, that so much of the recent work should have been devoted to gravity methods.

There appear to be four ways of treating the cassiterite problem:

(1) Treat only coarse-grained minerals and ignore the fine-grained material; this is the policy generally followed in alluvial tin mining;

(2) Endeavour to catch the fine-grained material by a series of gravity treatments; this is the Cornish method;

(3) Treat the fine-grained cassiterite by non-gravity methods;

(4) Minimize the production of fines during treatment and so reduce the problem of their recovery.

The first method is often very effective, as the proportion of fine-grained cassiterite in alluvial deposits is frequently small. This method can also *seem* to be successful because of inefficient prospecting methods which do not always disclose the total amount of fine-grained mineral in a deposit.

The second has been popular recently, but, despite continued improvements, cannot possibly provide the final answer.

The third method has attracted attention in the U.S.A. but no treatment has yet been carried to the plant stage. Flotation has never been successfully adapted to run-of-mine ores,* but I feel that the full potential of this method has not been exploited. It is true that flotation of cassiterite *is* difficult, but it may not be as difficult as some tin miners imagine. Does not the cassiterite problem deserve a further concerted effort, using the most modern techniques and new reagents? Several recent American papers deal with the treatment of run-of-mine ore and low-grade tin concentrates by chemical attack. Such a method has the major advantage that it is admirably suited for treating fine-grained material and it may also eliminate a smelting stage. Among its many disadvantages, perhaps, would be the strong 'sales resistance' it is likely to provoke in the tin-mining community. Laboratory tests on the chlorination of run-of-mine ore at 550° C show a recovery of over 95 per cent of the total tin although 30 per cent of the cassiterite was -325 mesh. Although this process is successful with run-of-mine material it would obviously be an advantage to pre-concentrate the ore, but this pre-concentration need not be extended to the finest fractions.

Method 4 has received less attention than the others. If gravity methods are still to be used it is essential to liberate the cassiterite at the coarsest possible size. Grinding circuit control or the use of new comminuting methods may offer greater advantages to the tin-dresser than improvements in the performance of buddles or frames.

This period of high metal prices is surely a good time for a re-appraisal of tin-dressing methods and a good opportunity for supporting research along novel lines.

*PRYOR, E. J., and WROBEL, S. A. Studies in cassiterite flotation. *Trans. Instn Min. Metall., Lond.*, **60**, 1950 51 (*Bull. Instn Min. Metall., Lond.*, no. 532, March 1951), 201 37.

Professor Maurice Rey: Some calculations based on the data given in Mr. Chaston's paper appear interesting and it would be helpful if the author would confirm or correct them.

From the data given in Table II (p. 220) it appears that the sulphide flotation concentrate removes 17 per cent and the desliming step 45 per cent of the weight of the thickener underflow. In test 1 the grade is increased from 0·70 to 0·81 per cent Sn by the flotation step, so that it can be supposed that in test 2 the grade is increased by flotation from 0·84 to 0·97 per cent. Desliming next increases the grade from 0·97 to 1·82 per cent and it is supposed that the tonnage of slimes removed is $45/(100 - 17) = 54$ per cent of the flotation residue. The tin content of the slimes is thus 0·25 per cent, a figure which seems reasonable when the grade of the fine size shown in Table I (p. 218) is considered.

From the data given it is possible to compute the combined result of desliming plus Buckman-table concentration in test 2. Table A below gives the results. The figures not taken from the paper, but calculated, are indicated by an asterisk.

TABLE A

	Test 1 without desliming	Test 2 with desliming
Desliming step		
Flotation residue, per cent Sn	0·81	0·97*
Slimes—weight, per cent		54*
Slimes, per cent Sn		0·25*
Slimes loss in metal, per cent		13·7 *
Concentrating step		
Feed, per cent Sn	0·81	1·82
Cleaned concentrate, per cent Sn	4·01	13·05
Residue, per cent Sn	0·53	0·89
Concentrate—weight, per cent	8·05*	8·15*
Concentrate recovery, per cent	40·0	55·0
Combined results		
Slimes plus table residue, per cent Sn	0·53	0·53*
Combined recovery, per cent	40·0	47·5 *

The striking fact is that the combined slimes plus table tailings show the same tin assay as the tailings obtained when desliming is not practised. Therefore the improvement in recovery on the fine sizes due to desliming does not appear very important. The main effect of the slimes on the tilting tables is to contaminate the concentrate and lower its grade.

The author gives the grade of final concentrate and recovery (Table IV, p. 223) but not the table tailing assay. This can be calculated, the results being as in Table B.

TABLE B

	A	*B*
Feed, per cent Sn	4·01	13·05
Concentrate, per cent Sn	41·40	49·29
Tailing, per cent Sn	1·20	0·80
Recovery	75·5	94·0

It thus seems that, in case *B*, treating a higher-grade feed and obtaining a higher-grade concentrate than in case *A*, the tailing is of lower grade.

This may be due to the fact that the tonnage treated on the table is much less. Computed from data in the paper, it is 160 kg/h in case *A* and only 65 in case *B*. A 'normal' tailing for case *B* would appear to be 2·40 per cent Sn, in which case recovery would be 85·7 per cent, a figure which is still remarkably good.

Mr. N. H. Monro: Mr. Chaston makes a plea for the re-definition of 'slime', but surely a true slime is a colloid—'that which approaches molecular dimensions'. I assume that there is no such thing as colloidal tin; all cassiterite particles are crystalline, so that 'slime' must be removed before satisfactory recovery can be made.

There is sometimes confusion in practice between classification and concentration. In treating fine tin the former is essential before the latter can operate. Only by good classification can a good recovery be made by concentration, a fact often forgotten in practice.

It is far harder to catch fine tin than coarse; the first principle must therefore be not to create fine tin in so far as this is possible, and the whole process of grinding should be aimed at this: reject the ground particles from the grinding machine as soon as possible and regrind as little as possible.

A great improvement in recovery would be made if the smelter would accept a lower-grade concentrate because regrinding to free the cassiterite so often reduces the tin particles to such a small size that it is uncatchable by known methods. What is really needed is a chemical method of recovering tin which is commercially economic. The whole grinding process can then be reversed and the Rand method known as 'all sliming' can be adopted.

Mr. E. Douglas: It was refreshing to note Mr. Chaston's new slimes designation, -10μ, and to note further the impressive results achieved by concentrating a feed which has been deslimed at this standard. Also, his explanation of double recovery, in which he relates the indifferent concentration at a 'middle size' to interstitial freedom, appears to be entirely feasible and most probably accounts for large proportions of middle size losses in many systems.

I feel, however, that an additional factor has been introduced into the work described. Fig. 7 (p. 219) shows the size recoveries for an unclassified feed and for a cyclone deslimed feed. An alternative explanation for the differences in these characteristics may be in the method of classification. From Figs. 5, 6 and 7, it appears that the d_{50} for the cyclone is between 13μ and 18μ and consequently the responses to separation of the underflow and overflow fractions, from this particular size cut, might be expected to be similar to one another and to the same fraction in the head. In confirmation, almost identical recoveries are shown in this size range for both characteristics presented in Fig. 7. Only small quantities (less than 10 per cent) of the fractions finer than 13μ reported to the cyclone underflow—this material must be highly susceptible to gravity separations (otherwise it would have passed to cyclone overflow) and consequently one would expect the relatively high recoveries which were actually achieved in the -13μ size range of the classified feed, *vide* Fig. 7. The improvements in recovery of sizes greater than 18μ may be due to the greater freedoms resulting from slime extraction.

Dr. Robinson has already commented on some of the discrepancies contained in the comparisons presented in Table III (p. 220). It should also be recorded that the concentrates have been cleaned only in the cases of the tilting concentrator. On the basis of rougher concentrates, the resulting enrichment ratios are approximately 4.

The outstanding tabling performances reported in Table IV (p. 223) are extremely encouraging and demonstrate the effectiveness and practicability of this method for recovering near-slime values. In relation to these results, Mr. Chaston's conclusion that tabling does not produce the double recovery characteristic may not be entirely valid as, in these tests, the feeds comprised tilting-table cleaner concentrates.

It can be asked what type of characteristic would result if the original deslimed feed were treated. It could be argued that it would be according to a product combination of the recoveries in *B* (Fig. 8, p. 224) and test 2 (Fig. 7), in which case a double peak would result.

Mr. D. N. Moir: Mr. Chaston shows in Table II (p. 220) the effect of removing slime from the feed to the Buckman concentrator; as a result of slime removal, however, the throughput of the table has dropped by more than half. It would have been interesting to have found out the effect of a throughput of 2·0 t/h of deslimed feed on the recovery and grade figures. I appreciate the fact that the overall recovery of Sn has risen from 38·6 to 45·4 per cent, but the question then arises as to whether the throughput over the concentrates could be increased by say 30 or 50 per cent, still retaining the same recovery. This would imply in plant practice that a reduction could be made in the number of units required to treat a given tonnage and as the author points out these are fairly expensive items. I wonder if the author has any data on increased feed rates to the Buckman concentrators.

In the section on tabling the size of table used in the experiment is not stated; I think this would be of interest, as the author emphasizes that the feed rate was low at 220 lb/h.

In order to get a better idea of the efficiency of the integrated operation of Buckman concentrator and table, the feed rates over the table should be in proportion to the output from the Buckman concentrators; this would imply a feed rate of 320 lb/h for 'undeslimed' material and 124 lb/h for the deslimed ore. Even had this been done, however, it would have been interesting to know just what throughput the tables could handle under these conditions in order to ascertain the reduction which might be effected in the number of units.

Finally I would agree with the author that selective interstitial hold-up of particles in the bed may have been responsible for the so-called double recovery effect.

Mr. D. N. Collins: I am surprised to see that separations have been achieved down to 9μ particle size on such a very simple form of gravity concentrator as the tilting frame. The residence time of particles on the helicoid are from three to six times higher than those of the Buckman tilting frame for equivalent linear fluid velocities. I feel that enrichment ratios obtained at different sizes should have been plotted along with the figures on p. 219 of Mr. Chaston's paper; recovery figures alone do not mean very much unless related to some form of selectivity expression.

I am entirely in agreement with the author in the use of desliming prior to treatment. The fact that desliming leads to improved sulphide flotation makes me wonder why the procedure is not as widely adopted in sulphide flotation as it is in non-sulphide flotation.

In gravity concentration it is standard practice to classify the feed prior to treatment. The removal of the fine slimes has a two-fold effect, serving to classify the feed and, in addition, removing the very fine material which does not respond to gravity concentration at all. This material would report in equivalent proportions with the water and would thus dilute the grade of the final product. Its hindered settling and viscosity effects would result in lower Sn recoveries.

The corresponding results obtained for classified and non-classified products make interesting reading. I have calculated overall recoveries and corresponding grades and enrichments for the products given on p. 220.

	Per cent recovery Sn		*Per cent Sn*		*Enrichment ratio*	
	Test 1	*Test 2*	*Test 1*	*Test 2*	*Test 1*	*Test 2*
Thickener underflow .	100	100	0·70	0·84		
Feed to separator .	96·4	82·6	0·81	1·82		
Cleaner concentrates .	38·6	45·4	4·01	13·05	5·7 (5·0)	15·5 (7·2)

The most remarkable result is that although the classified feed contains nearly 14 per cent less Sn values than that in the undeslimed feed, the overall recovery in the cleaner concentrate is 7 per cent higher. The overall enrichment is three times as great with the deslimed feed, but the enrichment obtained by the tilting frames alone is also higher.

In conclusion I feel that slime separation of tin is a misnomer, since real slimes cannot be separated under gravity conditions with present technological knowledge. It should therefore be the aim to produce better separation of the fine tin, i.e. in the $-53+10\mu$ range and if possible to extend this range lower. This can only be done by classification of the feed and/or desliming prior to treatment.