

THE JOURNAL
OF THE
Chemical, Metallurgical and Mining Society
OF SOUTH AFRICA.

The Society, as a body, is not responsible for the statements and opinions advanced in any of its publications.

Reproduction from this Journal is only allowed with full acknowledgment of the source.

VOL. IX.

NOVEMBER, 1908.

No. 5.

Proceedings
AT
Ordinary General Meeting,
November 21, 1908.

The Ordinary General Meeting of the Society was held in the Chamber of Mines, on Saturday, November 21st, Mr. R. G. Bevington (President), in the chair. There were also present:—

38 Members: Messrs. A. McA. Johnston, E. H. Croghan, W. R. Dowling, K. L. Graham, C. B. Kingston, A. Richardson, C. B. Saner, Prof. G. H. Stanley, H. A. White, W. A. Caldecott, W. Cullen, F. Alexander, G. Andreoli, S. Beaton, E. Browning, F. W. Cindel, W. M. Coulter, J. P. Creed, L. Evans, J. C. Greer, Jr., F. G. Guthrie, J. Higham, C. B. Hilliard, G. A. Lawson, J. Lea, C. D. Leslie, G. Melvill, J. T. Milligan, S. Morison, S. S. Osborn, C. F. Parry, C. E. Rusden, A. L. Spoor, A. D. Viney, J. P. Ward, H. Warren, and J. Watson.

6 Associates: Messrs. J. R. Allan, J. Chilton, C. L. Dewar, D. Nicholas, Hy. Rusden, and C. B. Simpson.

10 Visitors and Fred. Rowland, Secretary.

The President: I beg to move that the minutes of the last ordinary monthly general meeting, as published in the October *Journal*, be confirmed.

This was seconded by Mr. C. B. Saner and agreed to.

NEW MEMBERS.

Messrs. F. F. Alexander and H. Warren were elected scrutineers, and after their scrutiny of the ballot papers, the President announced that the candidates for membership had been duly elected, as follows:—

FURNER, EDWARD ARTHUR, Crown Reef G. M. Co., Ltd., P. O. Box 1145, Johannesburg. Mill Manager.
JOHNSON, ARTHUR JOHN, Robinson Deep G. M. Co., Ltd., P. O. Box 1488, Johannesburg. Foreman Amalgamator.

JONES, WILLIAM HARRY RICHARDSON, Public Works Department, Standerton. Civil Engineer.
MARTIN, HENRY STUART, Messrs. H. Eckstein & Co., P. O. Box 149, Johannesburg. Consulting Engineer.
MINARDS, WILLIAM IVEY KENNEDY, Benallack, Grampond Road, Cornwall. Cyanider.
TONNESEN, OTTO STUHAUG, East Rand Proprietary Mines, Ltd., P. O. Box 65, East Rand. Mine Surveyor.
WILLIAMS, RICHARD ST. JOHN BARNARD, Hauser Tribute Co., P. O. Box 160, Johannesburg. Manager.

The Secretary announced that the following gentlemen had been admitted as Associates by the Council since the last general meeting:

GRIERSON, NORMAN VICTOR, Higginsville, Western Australia. Metallurgist and Mining Engineer.
JAMESON, CHARLES GODFREY, Jumpers Deep, Ltd., P. O. Box 1, Cleveland. Mining Engineer.

And as a Student:

BROWNE, CLAUDE MELVILLE, Cinderella Deep, Ltd., P. O. Box 75, Boksburg (Student at the Transvaal University College).

GENERAL BUSINESS.

Mr. E. H. Croghan (*Member of Council*): I should like to test the feeling of our Society on a certain matter, which, though it may not be scientific, would tend to produce a more sociable feeling at our meetings and indirectly benefit all of us, as well as the Society.

As a Society, good work is carried on, but it seems to be in the hands of only a few. This is a very bad state of affairs, and to my mind is primarily due to a lack of intercourse not only amongst our members individually but also between the members and those controlling the Society's affairs. A society should be like a club where everyone is on an equal footing, and no one should feel that by virtue of his worldly position he is debarred from discussing a subject, no matter if trivial, with those with whom he desires to. Under our system of ordinary monthly meetings, it is difficult for it to be otherwise, in view of the short time at our disposal. We do have as many excursions as possible, but I wish to go some steps further. Sport has done much in

cementing the British Empire, well, why should it not be entertained in connection with science, locally at any rate. It would be indirectly a means of interchanging ideas in a more casual way, some of which could be as usual contributed officially to the Society. I feel assured many sleeping geniuses would then better develop, and come a little more into daylight, thus promoting our mutual interests. The idea could be extended still further by inducing other technical societies to enter the arena; we might also arrange smoking concerts, as I am sure we are endowed with plenty of talent. Certain annual functions might be held by the combined technical societies, we could have, say, a combined banquet or dinner, ball or dance, and athletic sports, trophies for which, I feel sure, would be given by our mine owners and others.

In conclusion, though this is an extremely unusual subject to mention at such a meeting, I think you will pardon me, as it is the only definite opportunity one has of bringing such matters forward.

The President: This is certainly a new idea of Mr. Croghan's for providing for social intercourse between the members of the Society, and as he has launched the bolt, I think it is up to any member of the Society to follow it up for further consideration or otherwise. Would anyone like to make any remarks?

PROPOSED LOCAL SECTIONS.

The President: There is, at the end of the agenda paper, a notice of a Special General Meeting to be called for the purpose of considering some proposed additions to the constitution and rules. It may not be known to many of you why these additions are wanted, but I may tell you that our members are so widely scattered in various parts of South Africa, and other countries, that suggestions have been made with regard to the formation of local centres or sections. We have had correspondence from some of our members in Rhodesia who wish to be able to form a local centre there, in order that they may meet one another and discuss matters connected with their mining operations. Some members also have an idea of forming a branch in another country. Therefore, your Council considered that in order to place it in a position to deal with the formation of local sections, your approval should be asked of the proposed additions to the constitution and rules. This matter will come up at a Special General Meeting on the 19th of December, at 7.30 p.m., and I trust that all members will take the matter into consideration and will be present

and vote either for or against, so that we may know the feeling of the Society. Suppose the additions are made to the constitution and rules it will not, at any rate, hurt anyone. We may never have to avail ourselves of these additions, but in order to prepare for such a case—and I think we should be prepared for it, as a feeling does exist that local sections may be formed—we propose, with your sanction, to make these additions to our constitution and rules.

THE SILVER COATING OF AMALGAMATING-PLATES.

Mr. W. A. Caldecott (*Past-President*): Whilst electro-plated copper plates have never come into general use on these fields, and whilst some of the virtues attributed to them may have little better foundation than the fond fancies of the old-time millman, there is yet no doubt that the percentage recovery by amalgamation in the early stages of starting a new mill with plain copper plates is by no means satisfactory. When more stamps or tube mills and shaking tables are installed on a plant already in operation, a little amalgam can usually be spared from existing plates to set the new plates, but even so, greenish stains and patches for some time offend the amalgamator's eye and defer the day when he can view with satisfaction a uniform silvery surface persisting from one dressing till the next.

The use of silver amalgam for setting purposes has frequently been recommended, and I have experimented upon it in various ways, dating from early but unsatisfactory attempts, which involved much labour in filing silver coins into powder. The simple method finally adopted was based upon obtaining a pure silver amalgam of buttery consistency containing the silver in the finest possible state of division. If such an amalgam is applied to plain copper plates after the usual scouring some two or three weeks before the plate is put into commission and the plate frequently dressed with the same amalgam during this period, the silver amalgam is given the opportunity to become thoroughly incorporated with the surface of the plate, with consequent benefit to amalgamation when the stamps or tube mills are started.

Silver amalgam is now commercially obtainable, but if preferred, it can be prepared in the manner described by Louis, as follows* :—

"A sufficient quantity of silver coin (about $\frac{1}{4}$ oz. per sq. ft. of surface of the tables) is dissolved in dilute nitric acid in a porcelain basin with the aid of a gentle heat. The solution is

* "Handbook of Gold Milling," 1899, p. 313.

evaporated to dryness very gently, preferably over a water bath, and then heated till the saline mass commences to fuse, and till all its bluish tinge is turned to greyish-black, this change indicating that all the soluble cupric nitrate is decomposed, insoluble cupric oxide being left behind. The salt is then dissolved in a small quantity of water and filtered into a jar or beaker. Pure mercury, to the weight of about three times that of the silver used, is poured in, a few drops of nitric acid added, and a few pieces of bright iron floated on the surface of the mercury. The silver will at once commence to precipitate and be absorbed by the mercury forming silver amalgam, the process taking a few days to complete thoroughly. The silver amalgam so produced should be of a pasty consistency."

In carrying out the foregoing method, it will be found that small silver coins dissolve more readily than the larger ones, and that small wire nails provide the iron required in a convenient form. In the process an iron-mercury couple is formed, which serves to deposit the silver present in solution upon the surface of the mercury which immediately absorbs it, whilst a corresponding amount of iron is dissolved.

If pure silver can be obtained, the evaporation of its solution in nitric acid, with subsequent heating to remove the copper, may be omitted and the process thus simplified, but if silver coin containing copper is used as a ready though somewhat expensive source of silver, and the copper not removed, the resulting silver-copper amalgam will show a greenish surface film soon after application to the plates, owing to oxidation of the copper present. The molecular state of fineness of the silver in the amalgam permits a much more perfect coalescence between the surface of the copper plate and the amalgam than when coarse particles of metallic silver are present in the latter. To ensure, however, a silver amalgam coating which will not tarnish on exposure to the air, time for absorption must be allowed, and for this reason the preparation of the plates some time before use is recommended. After milling has started the coating of silver amalgam is gradually removed from the plates as gold amalgam is scraped off, but by the time the former is all gone its purpose will have been served and its place taken by an equally efficient and permanent coating of gold amalgam.

The cost of silver coating copper plates in the manner described is relatively small as compared with the value of the amalgamable gold which otherwise passes away with the tailings in the early stages of crushing, and the allowance of $\frac{1}{4}$ oz. of silver per square foot of plate area is certainly liberal, as in my own experience a considerably less proportion than this has sufficed.

The following micro-photographs of ordinary copper plates, coated with silver amalgam, were kindly prepared for me by Mr. McA. Johnston.

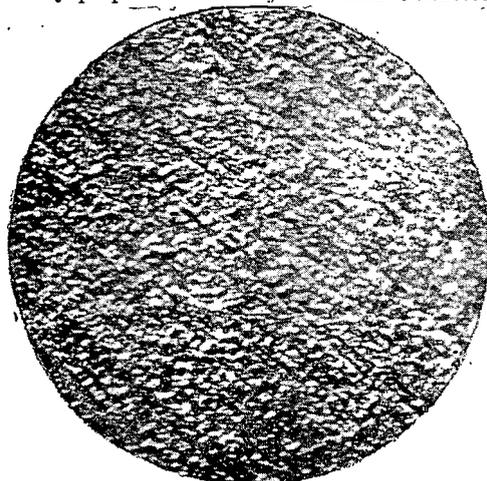


FIG. A.

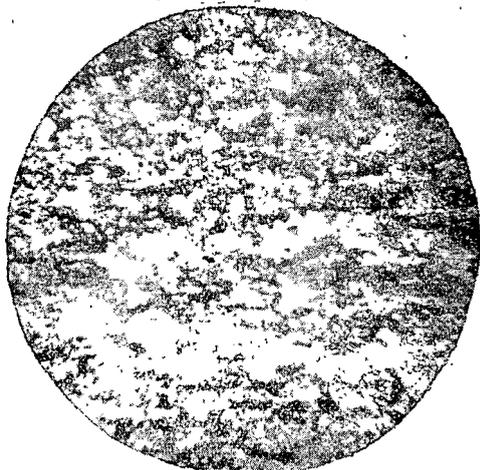


FIG. B.

Fig. (A) is a magnification of 36 diameters of the surface of the copper plate after having had silver amalgam applied to it once. The white portions are the globules of mercury standing out in relief, whilst between many of them the copper surface of the plate is visible quite untouched.

Fig. (B) is a magnification of 36 diameters of the surface of the copper plate after having had silver amalgam applied to it three times, the whole treatment consisting of four burnishings. Note here the more extended surface of the amalgam whilst in no part is the copper visible.

[Samples of the copper plates described in this note together with micro-photographs were passed round the room.]

The President: In amplification of the author's remarks I may say that one reason for

preparing a plate with silver amalgam is, amongst other things, a matter of cheapness, as its cost is comparatively small. A copper plate takes silver amalgam very readily, and once it is coated it does not tarnish, the result being that as soon as the battery is started the plates are in condition to catch gold at once. Of course, in scraping, a certain amount of silver will come off with the first gold amalgam; but that does not matter.

THE GASES RESULTING FROM THE USE OF HIGH EXPLOSIVES.

By WM. CULLEN (Past-President).

Some few years ago I ventured to read a paper on Miners' Phthisis—a subject of which I knew very little and on which I had practically no first hand information. The adage "that fools rush in where angels fear to tread" was at that time particularly applicable to me, but I am far from regretting the writing of that paper, as it was the means of stimulating enquiry in this particular subject, and I think I may safely assert that the direct result of the enquiry has been an all round improvement in the working conditions of those who labour in the bowels of the earth. Drs. Moir, Macaulay, and Irvine and Mr. Heymann, deserve our unstinted thanks for all they have done in our immediate sphere, and outside of it, the work of Dr. Haldane and Mr. Mann, of West Australia, has not been without influence. No one of these gentlemen would venture to assert that anything like finality has been reached, and I propose to-night to add another chapter to our knowledge. I shall have to go back on opinions which I have expressed on more than one occasion, here and elsewhere, and I am prepared to be told that both my facts and my deductions are wrong, but I shall deal principally with the former. I cannot say that I found these facts particularly palatable, and I do not expect that my mine manager friends as a body will like them either, but it is because they are facts that I think they should be made known as widely as possible. In the light of the figures which I am going to place before you presently I am more convinced than ever that the "ventilation of our mines" is the most important problem which we have to face at the moment, and this not only from a hygienic but also from the economic point of view, because the one is inextricably bound up with the other. A friend of mine—a mine manager of no mean repute—told me the other day that he came out to this country 10 years ago with 18 miners, to work on a certain mine. To-day only he is alive. All the others are dead.

This paper is one of a series of three, and it gets first place simply because I considered it the most urgent. It will be followed at a convenient interval by another by my colleague, Dr. Weiskopf, on the methods of analysis which have been employed in arriving at the figures which I give in this paper, and the third paper, a joint one, will treat of a new method of using explosives. I may, however, have to supplement this paper by another if I have an opportunity of carrying the investigations further.

Before proceeding I should like to refer very briefly to the results obtained by investigators in the same field of enquiry. Dr. Moir and Mr. Heymann working for the Phthisis Commission found that certain samples of mine gas contained an amount of carbon monoxide which was not only dangerous, but deadly. I was inclined to doubt their figures at the time, and received a certain support for my contention from the investigations of Mr. Mann, which followed shortly afterwards. Mr. Mann found no trace of carbon monoxide in the many experiments which he carried out, but found indications of nitric peroxide—by the starch iodine test—in several instances. This, to say the least of it, was peculiar. The work of Dr. Haldane has no particular bearing on our own conditions, but his results seemed to confirm those obtained by Mr. Mann. Reverting, however, to the early work of our local investigators, it is only right to say that at that particular time the quality of explosives used on these fields was, comparatively speaking, bad. The local product which was mostly used had been lying in magazines during the war period and the imported article was in the same plight. Old explosives do not give good results whichever way one looks at it. Since that time, quality has improved all round, and I question if any mining field in the world is better supplied than the Rand is at the present moment.

The explosive mostly used on these fields is blasting gelatine; practically no other is employed in driving, developing and shaft sinking; my work has been mainly confined to the gases resulting from the use of this explosive, occasionally slightly modified on lines which will be indicated. On being completely detonated, only carbonic acid, vapour of water, and nitrogen are formed—all comparatively innocuous gases. The particular equation which shows this result has been before you frequently, and I may say here that it is confirmed by explosion inside a steel bomb. Incidentally, I may mention that the cartridge wrapper, which Dr. Moir assumed took part in the explosion and led to the production of carbon monoxide, does not do so, at least not to any extent, as I have demonstrated more than once. This, however, is a side issue. My con-

tion has always been that if the conditions of the steel bomb were imitated as nearly as possible in practice, we should get results which at least approximate to the theoretical, *i.e.*, if the explosive is carefully tamped and certain other precautions taken. In no case was I able to get results which have the slightest resemblance to the theoretical, and with these introductory remarks I shall now proceed to describe the conditions under which the shots were fired, and the samples of gas taken. The experiments were carried out on more than one mine, and to certain unnamed mine managers I would express my best thanks for all the facilities which they gave me.

All the experiments were conducted in drives, *i.e.*, on development work, and a sample of the mine gas was generally taken before blasting—at a certain point. Immediately after the blast and before any air was turned on, the sampler proceeded to the same spot, generally 30 ft. to 40 ft. from the face, wearing a safety helmet, of course, and again sampled the gases from the blast. The actual sampling was done as follows. At the point fixed on, the sample bottle was opened, air was introduced into it by means of the bellows, which were emptied and filled 50 times. The same operation was gone through after the explosion. Scores of samples of mine air have been analysed, and although in most cases I have been given permission to make use of the figures, I do not propose to avail myself of this privilege to any extent. I may say, however, in passing that most of the samples were very bad indeed, though it is only fair to add, that in many cases those samples were taken from particular parts of the mine which were known to be bad. The astonishing thing about all these analyses is the almost universal presence of carbon monoxide, and that, more often than not, in dangerous quantity. The few examples which I give will however speak for themselves.

Before dealing with the experiments, I think it advisable to explain the underlying ideas. In the first place, there is a general impression that old explosives give much worse results than new; and that is so, as most people know, when we speak of the age of explosives in terms of years. Then again, there is a difference of opinion with regard to the best size of detonators—some contending that better results are obtained with No. 8 than with No. 6 and *vice versa*. With regard to tamping I hold rather extreme views. I have always had the idea that the current practice of employing sand cartridges was wrong in principle on account of the friable nature of the sand. Clay, to my mind, was infinitely preferable, so a comparison was tried. Incidentally, I may mention that Mr. Leslie, of the Simmer and Jack, who holds similar views to my own, employs a variety of

clay, and asserts that he gets much better work in practice. Then there was the vexed question of the benefit, or otherwise, of the so-called neutralisers, or antifume mixtures, of which there are sundry varieties on the market. There were various minor points, but the most important of all was to ascertain whether better all round results could not be obtained from a blasting gelatine which was so modified as to contain within itself more than the normal amount of oxygen. These experiments are unfortunately not yet concluded, but they have already given some most valuable data.

Now regarding all these points, I look upon the analyses of the resultant gases as the most accurate means of measuring the work done. In other words, the more completely the explosive is resolved into the simple gaseous mixture of vapour of water, carbonic acid and nitrogen, the better should be the footage, to use a technical term. Certain experiments seem to incline this way, but the results are not conclusive, and the experiments must be continued. What is conclusive is, that no matter how perfect the conditions were, very large amounts of carbon monoxide were always formed, and incidentally, I may say, that the conditions under which the experiments were carried out were above the average, in so far as exercise of care is concerned. When good results were sought, the best explosive obtainable was employed, the holes were all carefully charged and rammed home, tamping was done as well as it could be, detonators were known to be good, and all round the conditions were, as I have already said, above the average. Now for a few of the results.

In every case when not otherwise specified the tamping used was the ordinary sand cartridge.

TABLE I.—RESULTS OBTAINED FROM BLASTING ROUNDS.

No. of Experiment.	Explosive.	Detonator.	Distance from face.	GAS COMPOSITION.		Ratio of CO to CO ₂ .
				% CO.	% CO ₂ .	
1a	{Ord. blast. gel.	6	35'	·012*	·134*	1—7·8
1b	{3 months old			·88	6·92	
3a	{Newly made	6	27'	·026*	·328*	1—6
3b	{blasting gelatine			1·09	6·56	
6b	{Ord. blast. gel.	8	44'	1·28	8·07	1—6·3
4a	{3 months old			·021*	·299*	
4b	{Blast. gel. 3 mon.	6	35'	·90	7·44	1—8·2
2a	{old v. neutraliser			·013*	·50*	
2b	{Special composition, blast. gel.	6	40'	·56	3·7	1—6·6
5a	{Special composition, blast. gel.			·011*	·216*	
5b	{Special composition, blast. gel.	6	40'	·73	5·81	1—7·9
12a	{Modification of 2			·007*	·123*	
12b	{Modification of 2	8	16'	·718	6·05	1—8·4
13a	{Modification of 5			·023*	·058*	
13b	{Modification of 5	8	20'	·66	6·20	1—9·3
14a	{Different make			·035*	·25*	
14b	{of blast. gelatine	6	36'	·467	4·00	1—8·5

a and asterisks indicates before blasting. b after blasting.

Table I. deals with a series of experiments which are almost self explanatory, but in calculating the ratio of CO to CO₂, the figures of which appear in the last column, I have made no correction for the amounts present in the gases before the blast, though in some of the cases it is evident that they would materially affect the ratio. My reason for not doing so is that I do not think there is sufficient data to go upon, and one can play with figures too much. The one point which is established beyond the shadow of a doubt is, that carbon monoxide is always formed in large, even dangerous quantity. The presence of the large percentage of carbonic acid calls for no remark, and where larger quantities than the usual are indicated, the natural conclusion is that the sample has been obtained in a concentrated form. Strange to say, no trace of nitric peroxide was found in any of the samples, though we would not be justified in asserting that it was not formed. I think myself that it must have been formed in every case.

To come now to the actual results: 1, 3 and 6 are rather an interesting trio to compare. I fully expected that both 3 and 6 would show up better than 1, but they do not. Freshly made explosive is certainly more sensitive than explosive which has been stored for some time, and the Trautzl tests also indicate greater strength, but the results do not bring this out. The natural explanation with regard to sensitiveness is that the No. 5 detonators employed here are sufficiently strong to counteract any tendency towards insensitiveness, within a storage age of three months. Again, the prevalent idea is that No. 8 detonators give better results than No. 6, and although the experiments do not show this (Experiment 6), I think the practice of using No. 8 is sound, as it increases the safety factor for practical work. No. 4 is an experiment which stands by itself. The neutraliser was employed according to the inventor's instructions, but it cannot be said that the results are any improvement over ordinary practice. Incidentally, I may say that I never could see on scientific grounds why any of these so-called antifume mixtures or neutralisers could have any effect one way or another beyond perhaps diluting the gases with a certain small quantity of oxygen.

Experiments 2, 5, 12 and 13 were carried out with an explosive which is the subject of certain patents, and for which certain advantages are claimed. The results, with the exception of 13, do not indicate any improvement, but they are not complete. The percentage of the special oxygen yielding ingredients in Nos. 2, 5 and 12 are very small. In 13 they are increased, and by still further increasing them I am hopeful that it will be possible to produce an explosive which

will be more suitable for the work on these fields. I may say that all of them, but especially 13, have given excellent results in practice, *i.e.*, as measured by footage, and that on many mines. No. 14 is a make of blasting gelatine produced outside the Transvaal, but the results given call for no special remark.

Taken as a whole, therefore, the main conclusion which one is justified in making from these figures is:—(a) That carbon monoxide is always produced. (b) That the blasting gelatine employed in 13 indicates a slight improvement over the others.

Appendix I. gives the actual working conditions in greater detail than is shown in the table (No. I.).

On the question of tamping most divergent opinions are held. Several authors have referred to this question, among them being Mr. Weston at our last meeting. I do not propose to review these opinions here, and content myself by again expressing the personal view that sand is not a suitable tamping material. I believe that good clay is infinitely better, and if trouble were taken to procure it and to make it up into proper cartridges much better results would be obtained in whatever way one looks at it. The experiments given in Table II. are only of a preliminary

TABLE II.—INFLUENCE OF TAMPING.

No. of Experiment.	Tamping.	Distance from face.	GAS COMPOSITION.		Ratio of CO to CO ₂ .
			% CO ₂ .	% CO.	
15a	Dagga cartridges	47'	·049*	·356*	1-7.93
15b			·58	4.60	
16a	Ordinary sand cartridges	47'	·050*	·258*	1-6.5
16b			·28	1.84	

a indicates before blasting.

b indicates after blasting.

Ordinary blasting gelatine used with No. 8 detonators.

nature, but they are interesting as indicating a decided improvement in favour of dagga—which as you know is at least an approximation to clay. The tamping cartridges were made up in the usual way with the dagga dry, but they were used damp, which makes it somewhat sticky or adhesive. The details are given in Appendix II.

In a later paper I propose to deal with the subject of blasting by electricity, and the figures given in the next Table III. have only reference to a phase of that question. We all know that comparatively large amounts of carbon monoxide are produced from the burning of fuse, and if the details of Appendices I. and II. are examined it will be seen that the amount used for the blasting of a round is from 72 ft. to 96 ft. I shall deal with this presently. The cut is generally drilled so as to make the holes converge at the bottom, and this in practice means that the cut comes

TABLE III.—RESULTS OBTAINED FROM BLASTING CUTS.

No. of Experiment.	Explosive.	Detonator.	Distance from face.	GAS COMPOSITION.		Ratio of CO to CO ₂ .
				% CO.	% CO ₂ .	
8a	Blasting gelatine 3 weeks old	8	12'	.014*	.35*	
8b	Blasting gelatine 3 weeks old			.195	1.63	1-8
9b	Blasting gelatine 3 weeks old	elec dets	27'	.208	2.03	1-9.7
10a	Blasting gelatine 3 months old	elec dets	27'	.017*	.23*	
10b	Blasting gelatine 3 months old			.205	1.72	1-8.4
11b	Blasting gelatine 3 months old	8	27'	.22	1.71	1-7.7

a and asterisks indicates before blasting.
b indicates after blasting.

away at one blast, or at least so nearly so that one cannot detect any interval between the shots. I had an idea that firing by electricity, which ensured the simultaneous detonation of all the shots, would give better results, and the figures of this table, notwithstanding the fact that the electric detonators were only No. 6 as against No. 8 for the ordinary, certainly indicate a little improvement. Against this is, of course, the production of carbon monoxide from the burning of the fuse, but from other experiments it does not appear that the amount from this source could seriously vitiate the results given in the table. You will observe that in No. 10 only three holes were fired as against four in all the others. The details of these experiments are given in Appendix III.

The results of the foregoing experiments were so surprising and unlooked for, that I next decided to try gelignite, which is used for development on some mines. Gelignite may be described as a low grade blasting gelatine to which is added organic matter in the form of wood pulp, etc., and nitrate, so proportioned that the oxygen from the nitrate is capable of oxidising all the organic matter. Whether this oxidation is complete or incomplete has never been determined in practice, but judging from the analogy of ordinary black powder one would be safe in assuming that the combustion would not be complete. I shall not give the details in this instance, and shall content myself by stating that everything was done on exactly the same lines as in the former experiment, No. 6 detonators being used and the ordinary length of fuse.

For the results obtained, see Table IIIa. in next column.

So far I have dealt only with percentages of CO and CO₂ in the resultant gases, though most of the samples were completely analysed. These analyses enable us to calculate the composition of the gases produced in the borehole. A few of these results are given in the following Table No. IV.

TABLE IIIa.

GELIGNITE.		Before blast.	After blast.
Oxygen	...	20.310	17.96
Nitrogen	...	79.300	74.52
Hydrogen	...	nil.	.11
Carbon Monoxide028	1.23
Carbon dioxide362	6.18
Olefines	...	nil.	nil.
Oxide of nitrogen	...	nil.	nil.
		100.000	100.00

Proportion of CO to CO₂

1 to 4.9.

This is an astounding result.

TABLE IV.—CALCULATED COMPOSITION OF GASES PRODUCED IN THE BORE HOLE.

Gas.	14b.		Gelignite.		Ordinary Tamping.		Draught Tamping.	
	5.81	7.49	10.39	10.99	8.55	7.2	64.5	7.2
Carbon monoxide	5.81	7.49	10.39	10.99	8.55	7.2	64.5	7.2
Carbon dioxide	50.42	65.03	50.30	55.81	63.94	64.5	64.5	64.5
Nitrogen	43.49	33.16	37.98	35.58	26.61	26.4	26.4	26.4
Oxygen	nil	nil	nil	nil	nil	nil	nil	nil
Hydrogen	nil	nil	.95	1.01	1.40	2.0	2.0	2.0
Total	99.72	105.68	99.62	103.46	100.50	100.1	100.1	100.1

It will be observed that under the heading of "14b" and "gelignite" two columns of figures are given which represent two methods of calculating the result. The first column represents what I might call the "oxygen" basis and the second "carbon oxide" basis. Those acquainted with gas analysis calculations will understand what I mean, so I need not enlarge on the point.

It is evident, however, that the first, or "oxygen" basis method, appears to be the more accurate, but the difference between them would not affect the general conclusions in any way. I should also explain that the headings of the different columns in this table represent the same explosives as those referred to in Tables I. and II. The figures given are also the same, but differently stated.

According to theory, 12*b* and 13*b* should have furnished free oxygen—particularly 13*b*, but they do not. This is hardly surprising in view of the fact that the results obtained all through deviate so much from the theoretical. I shall return to this point later, as it is of great importance, but meantime I would again direct your attention to the high CO/CO₂ ratio of 13*b*—the highest so far found. It is stated in another way in Table I, but does not show up quite so high there, though the difference is not great. Unfortunately, the gases from experiment 4*b*, which, as you will recollect, was that one with which the neutraliser was used, were not completely analysed, but we have sufficient data to enable us to calculate the approximate composition of the borehole gases—which show no free oxygen. At this stage I may say that the theoretical gases given off by a scientifically constructed blasting gelatine are as follows:—

	With water as vapour 25° 635 mill. % vol.	No water condensed. % vol.	All water condensed. % vol.
CO ₂ ...	69·84	61·7	72·52
N ...	26·46	20·8	27·48
H ₂ O ...	3·70	18·0	nil.
	100·00	100·0	100·00

I now pass to another aspect of the question, viz., the effect of burning fuse on the composition of the mine gases. This ground has already been pretty well covered by Dr. Moir, so I shall confine myself to a record of one or two rather interesting experiments.

In most of the experiments in Table I. about 96 ft., or four coils, were employed for blasting the round. This amount of fuse was burned in a drive, and the gases were sampled before and after, just as in the other experiments. Unfortunately the "before" sample met with a misadventure, so I can only give the results of the "after" which were

CO ...	·050%
CO ₂ ...	·336%

As they stand these figures are not worth very much, excepting in so far as they show that

the CO produced by the fuse is more than insignificant in amount,—indeed, this amount is highly dangerous. Of course, all the other results already given, must be looked upon as the combined effect of a fairly constant quantity of fuse with a fairly constant quantity of explosive. Even in the case of the "cut" blasts, Table III., the fuse was reduced pretty much in the same ratio as the explosive, taking the figures given in Appendix I. as the basis of comparison.

The records of some interesting experiments are given in Appendix IV. The figures will, of course, vary with the quality of the fuse, and other factors will have an influence, but assuming that the toxic effect of a certain percentage of carbon monoxide on human beings is known, it is easy to reckon by the rule of three how many persons one could kill by a certain number of coils.

The examination of every-day mine gases with regard to their vitiation is outside the scope of this paper, and no useful purpose will be served by directing attention to particularly bad samples. Although, as already said, I have been given full permission to make whatever use I like of certain figures, I do not propose to avail myself of this privilege, for the simple reason that as far as possible I desire to avoid controversy. This much, however, I can say in general terms, and in saying it I am also expressing the opinion of the managers concerned, that some of the places were unfit for human beings to work in, and that it was wonderful how they managed to do so. On the other hand, some of the analyses indicated a purity almost equal to the atmosphere at Sea Point, or the Durban Bluff, but while gladly admitting this, I wish to make it very clear indeed that I think the subject of ventilation is not receiving that amount of consideration which it ought to do in the light of the advances which are being made in other directions. For instance, the Sea Point atmosphere was generally found where a less pretentious one would have served the purpose equally well. I believe that few of those responsible for the practical operation of our great mining industry realise the great importance of the subject, and what economic advantages improved ventilation would bring in its train. But how could they realise, one might well ask? The subject has been surrounded by controversy, results have been questioned, the samples were unfairly taken, the author showed bias, and so on, but the most important of all is that the facts which I have placed before you to-night were not known—or put it in another way if you please—were not realised. They introduce new factors which are unpalatable to me, and equally so to many of my friends and colleagues on these

fields. I therefore appeal to all those who are interested, to start the consideration of this subject afresh, to look at it from the broad as well as from the individualistic standpoint, and to show a genuine anxiety to get at facts. I know an appeal such as this is unusual in a scientific paper, but the subject is so very important, that I have ventured to do an unusual thing.

I now turn to one or two considerations which arise out of the figures which I have placed before you. You will recollect my reference to Mr. Mann's experiments—mostly with gelignite by the way—which showed an entire absence of CO. I believe, in the light of my results, that his methods of estimating it were at fault, and I am afraid that the same may be said of some of the work which has been done here. Whether or not, I have had doubts raised in my mind on many important points, which have hitherto been looked upon as settled. For instance, are our compressor gases as free of CO as we suppose, and have not mine gases frequently been reported as free of CO when they actually contained it? Remember that we are dealing with infinitesimal quantities of CO, that few people in this country know much about gas analysis, and that fewer still have the necessary apparatus for analysis. It is for this reason that you will have the full details regarding my figures placed before you at the earliest possible date. In no case was nitric peroxide observed, even in traces, though I believe, as I have said already, that very small quantities may have been produced. It is certainly produced when burning takes place, but all the experiments were carried out with great care in order to eliminate this complicating element. As a matter of fact, there was no reason for supposing that burning took place in any one of the experiments.

Then again, I think the whole question of ventilation of our mines has been complicated by the figures which I have placed before you to-night. Undoubtedly, it makes the problem more difficult of solution, but the first stage is to get at the facts and to ascertain what is the true average state of the air at every part of the mine. This will not be done in a day or in a month, and it will cost a lot of money, but ultimately, I am sure those responsible will feel as interested in the analysis of their mine gases as they are in the "grade," or the monthly tonnage, for improved ventilation must ultimately mean reduced costs.

It has been represented to me that all the gases were sampled under very abnormal conditions, and that in order to make my case complete I should have sampled the drive again at

certain time intervals in order to show the progressive decrease of CO. It must, however, be apparent that this would have been equally abnormal, so I think the only fair thing to do is to sample under working conditions before the blast and then after. They give one the two extremes, but the really important one after all is the one "before."

The conclusions at which I have arrived have been referred to during the course of this paper, but they may again be summarised as follows:—

- (1) Carbon monoxide is produced in large quantities on the detonation of blasting gelatine, under all the ordinary conditions of local practice.
- (2) The so-called neutralisers or antifume mixtures do not appear to have any influence one way or another.
- (3) Clay tamping seems to give better results than ordinary sand tamping.
- (4) Freshly made blasting gelatine gives no better results than that which is three months old.
- (5) Within an age limit of three months a No. 6 detonator seems to answer as well as a No. 8.
- (6) Blasting gelatine of the nature of 13b seems to give better results than the ordinary blasting gelatine.
- (7) Firing by electricity seems to give slightly better results than ordinary time fuse.
- (8) The ordinary quantity of fuse used vitiates the mine atmosphere to a very large extent.

No one is more conscious than I, that only the fringe of this subject has been touched, and further and equally interesting experiments are now in progress. If they are not completed when my reply is due, they will be made the subject of another communication.

With regard to No. 6, I have accumulated a large amount of data, all going to show that a greater footage is obtained with an explosive of this class, but I am diffident about placing it before you, as I know how liable we all are to be favourably predisposed, and if a miner thinks he is going to get better results than usual he can generally get them. The consensus of opinion with regard to the gases, however, is that they are very much better than my figures would indicate, and I am very hopeful that as a result of these experiments, a class of explosives will shortly be put on the market which will give at least as good footage as ordinary blasting gelatine, but very much better gases.

With regard to tamping (No. 3), it is difficult to say what amount of improvement the figures which I give will mean in actual practice, but it is sufficient for my purpose to demonstrate an improvement in the gases.

Regarding the results as a whole, it is very pertinent to enquire why they should deviate so much from the theoretical. That is a difficult

question to answer, but the following considerations are worth bearing in mind. When nitro-glycerine, say, is ignited in a steel bomb of convenient size, the resultant gases are in accordance with theory. If my recollection serves me aright—it is many years since I made the experiments—blasting gelatine behaves similarly. For my purpose the first case however is sufficient. Would nitro-glycerine in the form of dynamite give the theoretical results in our ordinary Rand practice? I do not think so (but intend to try), because the conditions are so entirely different. Then again, I have always had an idea that the varying burdens in the different shots of a round might account for the production of varying amounts of so-called noxious gases, but from the results of Table III. it is evident that this cannot be so. In the case of the electric detonators all shots were set off at the same time, and yet the amount of CO produced is relatively the same as in a round. No, it appears that we must look elsewhere for the possible causes, and these are now being investigated.

In conclusion, I should like to express my personal thanks to Mr. Waters, technical representative of my company, who has done practically all the underground work, and to my colleagues, Dr. Weiskopf and Mr. Greig, who have carried out all the analyses and done nearly all the calculations.

APPENDIX I.

Experiment No. 1.—Total depth of holes, 83 ft.; total No. $1\frac{1}{4}$ in. plugs, 79 = 43.8 lb.; temperature of level, 24.5° C.

Experiment No. 2.—Total depth of holes, 89½ ft.; total No. $1\frac{1}{4}$ in. plugs, 78 = 43.3 lb.; temperature of level, 23° C.

Experiment No. 3.—Total depth of holes, 89½ ft.; total No. $1\frac{1}{4}$ in. plugs, 82 = 45.5 lb.; temperature of level, 22.5° C.

Experiment No. 4.—Total depth of holes, 88½ ft.; total No. $1\frac{1}{4}$ in. plugs, 76 = 42.2 lb.; temperature of level, 22.5° C.

Experiment No. 5.—Total depth of holes, 90½ ft.; total No. $1\frac{1}{4}$ in. plugs, 78 = 43.3 lb.; temperature of level, 22.5° C.

Experiment No. 6.—Total depth of holes, 92 ft.; total No. $1\frac{1}{4}$ in. plugs, 84 = 46.6 lb.; temperature of level, 22° C.

Fuse used for all the foregoing, 96 ft.

Experiment No. 12.—Total depth of holes, 76 ft.; total No., $1\frac{1}{4}$ in. plugs, 81 = 45 lb.

Experiment No. 13.—Total depth of holes, 72 ft.; total No. $1\frac{1}{4}$ in. plugs, 77 = 42.8 lb.

Experiment No. 14.—Total depth of holes, 76 ft.; total No. $1\frac{1}{4}$ in. plugs, 78 = 43.3 lb.

Fuse used in each of the foregoing three cases, 72 ft.

The vertical depth in each case was between 1,400 and 1,700 ft., and the levels were all about 8 ft. x 5 ft. The tamping in all cases consisted of three 14 in. sand cartridges.

APPENDIX II.

Vertical depth to level	1,900 ft.
Distance from shaft to face	600 ft.
Size of level	7 ft. x 6 ft.

Dagga Tamping.—Three cut holes were charged with 24 plugs blasting gelatine and fired. The air was then turned on, and after about half an hour a return was made to the face and the air was sampled previous to blasting the round—which consisted of ten holes, averaging 5½ ft. in depth. The holes were charged with 66 plugs, $1\frac{1}{4}$ in blasting gelatine = 36.6 lb.

Temperature of level	25° C.
Fuse used for the round	60 ft.

Ordinary Tamping.—The same procedure was adopted as in the above, but as the cut had not come away clean, it was again charged up, four extra plugs being used for re-blasting the cut. The re-charged "cut" and round were then fired together; 70 = 38.2 lb. plugs being used in all.

Temperature of level	25° C.
Fuse used for the round	66 ft.

APPENDIX III.

Experiment No. 8.—Four cut holes; 26 plugs $1\frac{1}{4}$ in. blasting gelatine = 14.4 lb.; 24 ft. fuse.

Experiment No. 9.—Four cut holes; 26 plugs $1\frac{1}{4}$ in. blasting gelatine = 14.4 lb. electric fuses.

Experiment No. 10.—Three cut holes; 24 plugs $1\frac{1}{4}$ in. blasting gelatine = 13.3 lb.; electric fuses.

Experiment No. 11.—Four cut holes; 26 plugs $1\frac{1}{4}$ in. blasting gelatine = 14.4 lb.; 24 ft. fuse.

APPENDIX IV.—(Fuse Experiments).

Experiment No. 1.—13 in. of fuse, weighing 6.887 gm. (dry) were burned in a closed bottle of 12,130 c.c. capacity, and analysed the following day.

Analysis, Calculated on Dry Gases.—

	Vol. %.
Oxygen	17.64
Nitrogen (by diff.)	74.31
Hydrogen	1.57
Carbon monoxide	2.56
Carbon dioxide	3.51
Olefines41
Oxides of nitrogen	nil

From measured increase in pressure, as indicated by a manometer, the amount of gas formed

was found to be 874 c.c. (N.D.P.). The analysis of the air contained in the bottle before the experiment showed (calculated on dry):—

	Vol. %
Oxygen	20·876
Nitrogen	79·084
Carbon dioxide... ..	·04

The saturation with moisture was 60%.

From the above data the composition and amounts of gases given off by the burning fuse were found to be.

Composition, Calculation on Dry.—

	Vol. %
Nitrogen	22·41
Hydrogen	15·23
Carbon monoxide	24·73
Carbon dioxide	33·65
Olefines	3·98
	100·00

Composition in Volume per Cent. calculated to actual underground conditions, *i.e.*, saturated with water vapour at 635 mill-pressure and 25° C.:

	%
Nitrogen	21·58
Hydrogen	14·67
Carbon monoxide	23·81
Carbon dioxide	32·41
Olefines	3·83
Water vapour	3·70

Actual amounts of gases in cubic centimetre given off by 100 ft. of fuse calculated to underground conditions, as above:—

	Litres.	Cub. ft.
Nitrogen	23·62 =	·834
Hydrogen	16·05 =	·567
Carbon monoxide	26·06 =	·920
Carbon dioxide	35·47 =	1·253
Olefines	4·19 =	·148
Water vapour	4·05 =	·143

From the analysis it is also evident, that the fuse in burning consumes a considerable quantity of oxygen, which calculated to underground conditions as above per 100 ft. of fuse, amount to 14·5 litres of oxygen consumed, equal to 412 cub. ft. Several other experiments on the same lines were made, but the results obtained agree with the above.

The President: I wish to propose a very hearty vote of thanks to the author for his most interesting and instructive paper. I am very glad to learn that this is only an instalment of several papers to come. To mining men and indeed to many others connected with a technical society like ours, the subject must be of the greatest interest, and anything that can be done to improve the state of the atmosphere in mines, confers a benefit not only on miners and the

mining community, but upon the inhabitants of the locality generally. The author was good enough to give us an insight into what was coming, in the excellent address which he gave to those members of the Society who went to the Dynamite Factory. I took the opportunity, when there, of thanking the author and the directors of the B.S.A. Explosives Company, for their kindness in having us there and showing us round, and I wish again to extend our thanks to the author and the directors of the company. Many of us had had the pleasure of going there once some years before, and it was very apparent on our second visit that the company had not stood still in its work, but had been and was using every means to improve its processes and manufactures, so that its products might arrive at the highest stage of perfection possible, and I think that is a very gratifying thing to see in a local industry.

Mr. A. McA. Johnston (Vice-President): In seconding this vote of thanks to Mr. Cullen for the excellent paper he has given us to-night, I feel confident that I express the appreciation of all our members. The great amount of trouble he and Dr. Weiskopf have gone to in carrying out these researches is apparent on looking at the figures, and our Society is indeed indebted to them for the pleasure we have had in listening to the interesting conclusions drawn. Whether these conclusions will have the support of all our members is another matter. For example, the figures he has placed before us seem to have on their face some condemnation of the explosives we use here, but as he has most definitely assured us that these are of the best possible quality and surpassed by none, that the most up-to-date methods were adopted in charging and firing, and that every precaution was taken in sampling and analysing the resultant gases, we can only come to the conclusions that a reformation in the constituents of our explosives is essential, that present day methods of tamping need revising and, it would seem, that oil of the best quality for lubricating compressors is a *sine qua non* for good air. That Mr. Cullen and his able staff are on the track of the former two is a guarantee that all will be done that is possible, whilst the injurious effects of using inferior qualities of oil for compressors is gradually being appreciated by our managers and engineers, as well, let us hope, as by our buyers.

Re ventilation, the author is particularly strong, but this question has so often and so recently been thrashed out by this Society that I feel very diffident about again bringing it to your notice, the more especially as, though the author has given us very alarming figures, he has failed

entirely to elucidate their presence, and his data of the actual working conditions are meagre.

A glance at these figures certainly justifies the author's statement that the samples were taken from what are considered the worst parts of the mine, though few of us realised the presence of such quantities of carbon monoxide. The author does well to label many of these dangerous, but I think he ought to have given us a little more light on the probable sources of such large quantities. Given two not improbable premises, that blasting had taken place, say, eight hours previously, and that compressor air was used in driving, we can only conclude that the latter or the gas given off by the broken rock during lashing are the sources of contamination. As, however, lashing should have been finished an hour or two before charging, we are thrown back on the compressor air as our source, or is it possible, the want of it. I think some more detailed information is due to us, and I feel certain the author will be only too pleased to help us out in judging his results.

I am in agreement with him when he says that nitric peroxide must have been formed in every case. Speaking generally, in about a dozen samples of air taken by me some years ago, after explosion, I found this gas in practically all, even though the air samples were taken at a greater distance from the face than in the samples quoted. Until, however, the author and Dr. Weiskopf describe the methods they have adopted in their analyses, I feel we must refrain from undue criticism. In again, therefore, asking you to pass a vote of thanks, I think I can assure the author that his figures will be keenly appreciated throughout mining circles, and we hope that his efforts, and especially his candid avowals, will have the effect of clearing up many points and tend to better ventilation, or it may be to the introduction of more suitable explosives, in these drives whence his samples were obtained.

Mr. C. B. Kingston (*Member of Council*): The author's paper is one of exceptional interest, and I hope it may lead to a full discussion, and bring out some information regarding a number of points connected with the ventilation of our mines, which is not perfect. One point I would like information about, is the amount of gas that may be retained by the broken rock in the face of a drive after blasting. It would be interesting if some of our members could get definite data on this point. Samples of air might be taken a short time after blasting, and again at intervals during the course of lashing, to determine the extent to which the air is affected by the gases retained in the broken rock; and if there is a real advantage to be gained by drenching the

rock with water, when practicable, before lashing. The good practice of cleaning out the faces as quickly as possible after blasting is not always followed, and it appears to me that in addition to impeding the flow of air, and obstructing the drive, the broken rock constitutes a reservoir of poisonous gas that is slowly fed into the air the men have to breathe.

Mr. H. A. White (*Member of Council*): I should just like to say that it is the theoretical portion of this most valuable paper of the author which has the greatest interest for those of us who are chiefly occupied upon the surface of our mines. The figures given seem to imply a complete disappearance of about 7 or 8 of oxygen in the gases resulting from the explosion. It is perhaps possible that the solution of this discrepancy would throw some light upon the differences observed in the explosion of gelatine in a steel bomb as compared with the results calculated for the usual bore hole explosion. I do not know if the walls of the bore hole have any special physical effect or if the pyrites present have time to exert any significant chemical action, but the disappearance of so much oxygen seems to require some elucidation.

Mr. C. B. Kingston next read the following paper, first saying, "those of the members who are familiar with the history of gold milling will note that the process described by Mr. Cooke is a return to one of the very early methods of gold extraction."

DESCRIPTION OF ORE TREATMENT AT THE GIANT MINE, HARTLEY DISTRICT, RHODESIA.

By REGINALD C. H. COOKE (*Member*).

In general outline the process consists of crushing with stamps through coarse screens, partial amalgamation, fine grinding in tube mills, close concentration on blanding and canvas tables, followed by amalgamation of concentrates after treatment with nitric acid.

The ore may be described as a heavily mineralised chlorite schist, intermixed with a good deal of iron stone and quartz. The following is an analysis taken from the original report:—

	%
Iron as Fe ₂ O ₃ ...	18.0
Magnetite ...	14.0
Pyrites ...	14.6
Insoluble silica and silicates ...	46.0
Al ₂ O ₃ ...	6.0
Specific gravity =	3.1.

The gold is of two kinds, (a) that which is easily amalgamated, of a bright yellow colour, 30%, and (b) that which is refractory, of rusty appearance, requiring a special treatment before amalgamation. The ore, after passing two Blake rock breakers and falling into the bins with fines that pass the grizzlies, is fed into the batteries by Nelson feeders.

The following table demonstrates the work accomplished by the stamps:—

TABLE "A."

No. of Stamps.	Average monthly tonnage.	Duty.	Weight of Stamps, lbs.	Drop.		Screening.		Discharge.
				No. per minute.	Height, ins.	No. 1 & 2 Batteries.	No. 3.	
15	6,300	15 (of late)	1,250	103 (96 of late)	7½	¾"	1"	3"

It may be of interest to mention that 1/10 in. width slotted screens were originally used with a duty of 10.5 tons per stamp per 24 hours. A 1/8 in. mesh wire screen was then tried, but unsuccessfully, owing to rapid clogging; 1/4 in. slotted screens were then ordered, but their arrival being long delayed, at the suggestion of Mr. Galbraith, foreman amalgamator, a 1/4 in. wire screen was tried with success. Experiments were then made with a 3/8 in. and 1/2 in. mesh with the following results:—

TABLE "B."

Screening used.	Approximate duty per stamp.
1/10 in. Slotted screens	10.5 tons.
1/8 in. Mesh wire screening	11.65 "
1/4 in. " "	13/14 "
3/8 in. & 1/2 in. " "	15 "

It has not been found advisable to use 1/2 in. screening on more than one battery at a time, except when the ore coming into the mill contains less quartz than usual. If used on all three batteries, the ore crushed by the stamps exceeds the capacity of a single tube mill.

Table "C" shows the result of crushing through a 3/8 in. versus 1/2 in. screen

It will be seen from this table that the 1/2 in. screen gives an increase of 12.5% + 30 mesh. As the battery is simply a primary crusher, this is an important factor. The pulp from the batteries passes over copper amalgamating plates 4 ft. long, where 30% of the gold is retained.

N.B.—(The big tonnage and the coarseness of sand passing over these plates does not in any way injuriously affect their surface.)

TABLE "C."

Battery No. 1. 3/8 in. mesh.	Battery No. 2. 3/8 in. mesh.	Battery No. 3. 1/2 in. mesh.
+ 3/8		= 3.12
+ 1/4 = 2.50%	= .62	= 7.22
+ 3/16 = 7.38%	= 5.62	= 10.22
+ 1/8 = 6.15%	= 5.00	= 8.00
+ 30 = 25.00%	= 22.50	= 25.00
+ 60 = 12.50%	= 13.78	= 12.45
+ 90 = 8.19%	= 10.00	= 7.75
+ 120 = 5.00%	= 5.62	= 5.21
- 120 = 30.78	= 36.00	= 20.00
<u>97.50</u>	<u>99.14</u>	<u>98.97</u>

N.B.—The difference between No. 1 and 2 batteries sizing tests is due to the position of the grizzlies, which deliver most of the fines to No. 2 battery.

The pulp then flows down launders over very simple and effective amalgam traps, which prevent any amalgam escaping into the tube mill. These traps consist of a few 3/4 in. sized pebbles, scattered down the length of the launders, with a false bottom consisting of strips of 3/8 in. screening cut to fit the width of the launder laid over and nailed down. A piece of wood 3/8 in. square nailed across the width of the launder completes the trap. This allows the amalgam to sink through to the bottom of the launder, where the pebbles, etc., effectively retain it. On an average 400 oz. of amalgam are monthly cleaned up from these traps, and usually no trace of mercury can be seen in the blanket concentrates. The pulp now flows into the tube mill dewatering spitz 4 ft. x 4 ft. A 1 in. nozzle allows coarse sand to sink through the spitz into the tube mill with a ratio of 0.8 to 1 of water to 1 of dry sand.

Table "D," which illustrates the crushing powers of the tube mill, shows a reduction of 41.47% + 30 to 1.56% + 30, and a total of 68.54% + 120 to 22.68% + 120. 180 - 200 tons of dry ore per 24 hours was passing through the tube mill when test was made.

N.B.—The tube mills are 25 ft. long and 5 ft. in diameter, lined with silex blocks; life of liner, 31 weeks; consumption of French imported pebbles, 300 lb. daily; charge, 12 tons; speed, 28 rev. per min.

The overflow of the tube mill dewatering spitz consisting of surplus water with slime containing only about 2% of oversize + 120, is conveyed by a 3 in. pipe to the outlet of the tube, where it joins the pulp discharged from the tube in the usual way. This alone is not sufficient to dilute the pulp to the consistency necessary for obtaining the maximum extraction on blanket tables, and water is there-

TABLE "D."—SIZING TEST.

Sand entering Tube Mill.	Discharged from Tube Mill.
+ $\frac{3}{8}$ in. = 1.02%	
+ $\frac{1}{4}$ in. = 3.65%	
+ $\frac{1}{8}$ in. = 6.97%	
+ $\frac{1}{16}$ in. = 6.00%	= 0.00%
+ 30 in. = 23.83%	= 1.56%
+ 60 in. = 13.54%	= 2.00%
+ 90 in. = 8.33%	= 8.00%
+ 120 in. = 5.20%	= 11.12%
- 120 in. = 30.79%	= 76.00%
<hr/> 99.33%	<hr/> 98.68%

fore added by a $2\frac{1}{2}$ in. pipe to render the pulp 7 water to 1 dry sand. This pulp then flows over blanket tables upon which it is concentrated.

Description.—The blanket tables consist of 12 strakes (2 of which are spares) 12 ft. long by 2.5 ft. wide, with a 12.5% fall, these conditions being found to best suit the ore. The blanket-ing is made to special order in England. In texture it is similar to the ordinary military rug. It is made in long rolls, 3 ft. wide, and is torn up into $4\frac{1}{2}$ ft. lengths, this being the most convenient size for washing. These short lengths of blanket are laid on the strakes like shingles with an overlap of 6 in. The blankets from the strakes are washed at short intervals. When they are to be changed, the pulp is first switched on to the spare strakes. The blankets with concentrates attached are then folded, with the edges always towards the centre, and are carried to the washing vats, washed blankets being kept ready to replace them at once. The pulp is then deflected from the next strakes in series, and so on in regular rotation. In this way each blanket is changed and washed every half hour.

Washing.—The washing vats are 5.5 ft. in diameter by 2.5 ft. deep, filled with water. A boy catches hold of a blanket by the ends and draws it rapidly backwards and forwards through the water; about two dozen motions are sufficient to free the blanket of all concentrates and gold. I am convinced that the high extraction of 55% of total gold recovered on these blankets is due to the above method of using convenient short lengths of blanketing, and the removing and hand-washing of same at short intervals. No amount of sluicing or use of revolving blanket tables would free the blankets of their heavy concentrates or approach the results now obtained on this ore. By the above continued process of frequent changing and washing, a fairly large amount of concentrates—about four to five tons per day—is collected, and as this is too great a

bulk to be conveniently dealt with, an elevated blanket table is used for their re-concentration. The concentrates are gradually fed into a perforated box, where a $1\frac{1}{4}$ in. stream of water is allowed to slowly wash them down the re-concentrating table. The overflow from this table falls into the tube mill discharge launder, and again travels over the ordinary strakes. By this method a very rich concentrate of about half a ton is obtained. This is the concentrate from the total day's tonnage, and contains 55% of the assay value of the gold.

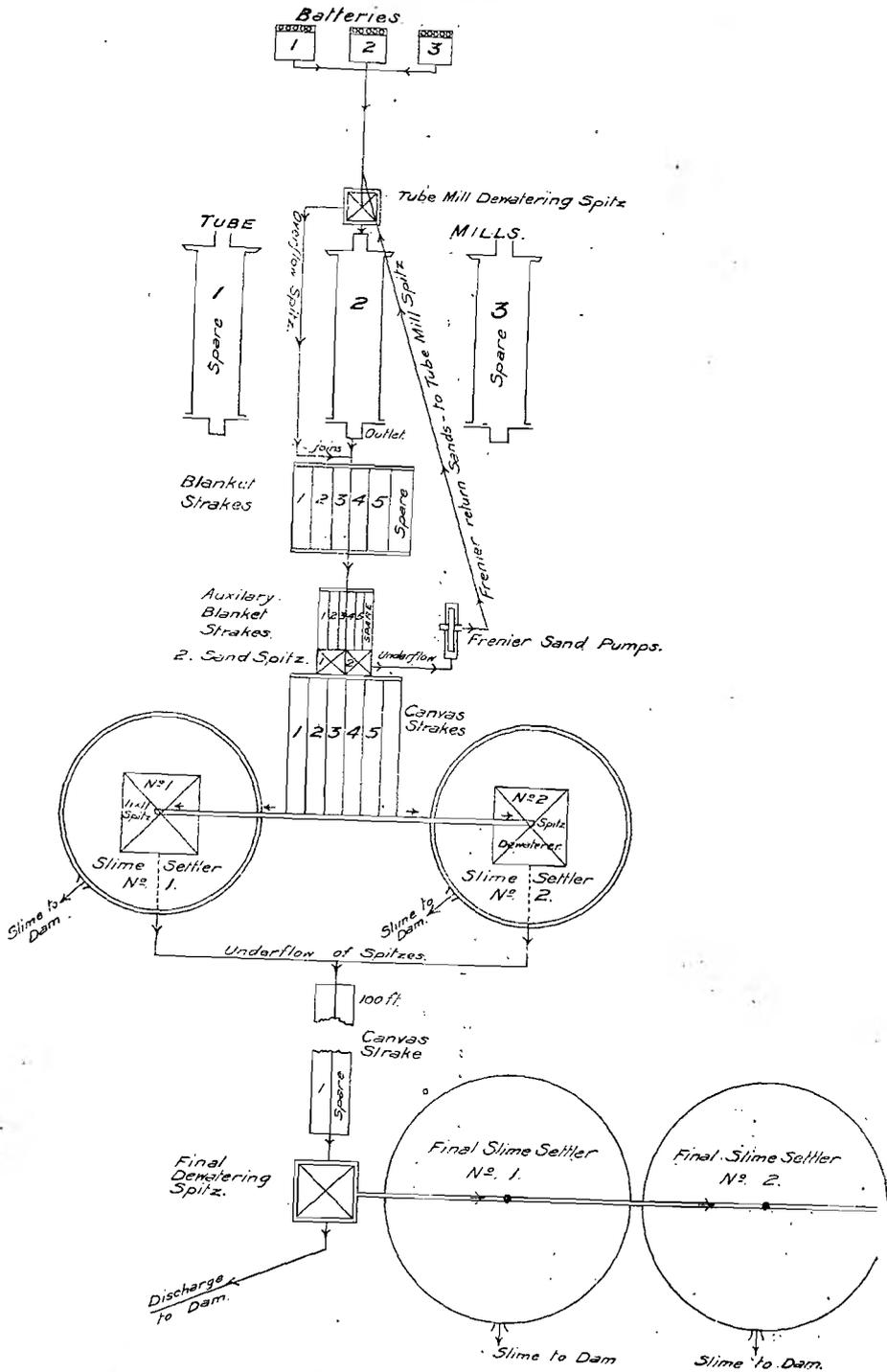
Amalgam Barrel Treatment.—This half-ton of re-concentrates containing the refractory rusty gold is now introduced into the amalgam barrels, and about 300 c.c. of concentrated HNO_3 is added to the charge, which is given a run of four hours as a preliminary treatment. Mercury is then added, the rusty gold being by this time sufficiently cleaned by the acid (which also renders mercury more active), and another four hours' run completes the process. Without the acid treatment only a small percentage of the rusty gold will amalgamate, the mercury becoming foul and sickened and much of it being lost by flouring. If the barrels are discharged without the acid treatment, and the amalgam collected in an earthenware mortar, this unamalgamated rusty gold can be seen. We are indebted to Mr. W. A. Caldecott for the suggestion of using nitric acid for cleaning this foul amalgam and rusty gold by pestle and mortar. This led to my idea of using the acid directly with the charge, with success. Many substitutes for the acid have been tried, but without success. The contents of the barrels are panned before discharging, and in the event of gold showing, the barrel is re-started with a little more acid. The contents are finally discharged on to the ordinary rotary Batea pan, and the amalgam collected and dressed in the usual way.

To continue treatment—The pulp after flowing over the ordinary blanket tables runs over five-narrow auxiliary blanket strakes; it then flows into a two compartment sand separating spitz, each compartment $3\frac{1}{2}$ ft. \times $3\frac{1}{2}$ ft., where a $1\frac{1}{4}$ in. nozzle on each allows the oversize +120 to sink into the lower of the two Frenier return sand pumps; this elevates it to the top Frenier, which in turn sends it back to the tube mill dewatering spitz. Again sinking it passes into the tube mill, a second time for further reduction to -120.

Table "E" illustrates duty of pump.

TABLE "E."—PULP TO FRENIER.

+ 30 = 1.1%
+ 60 = 4.8%
+ 90 = 23.0%
+ 120 = 27.0%
- 120 = 40.0%



The overflow from sand separating spitz, consisting of the slimed ore, containing only 1 - 2% of + 120, flows over five canvas tables at ratio of 10 water to 1 dry slime. These canvas strakes are 20 ft. long by 2½ ft. wide, and are covered on the same principle as the blanket strakes, but with the best fine white duck canvas. (Coarse canvas is unsatisfactory both as regards extraction and wear.)

On this canvas is retained the fine rusty gold that has escaped the blankets. The saving on the canvas is 0.25 dwt. gold per ton crushed. The short lengths of canvas are taken up and washed in the same way as the blankets, but as only about 5 tons of concentrates are collected in 15 days (because the canvas retains far less bulk than the blankets) re-concentration to render them rich enough for direct barrel treatment only takes place twice a month. This re-concentration takes place on an auxiliary table covered with canvas.

Battery Tails.—A sample is taken at the bottom of the above canvas strakes before the pulp enters a dewatering spitz in which it is split up into a light portion and a heavy portion.

Heavy Portion.—The pulp from the canvas strakes is divided, each part flowing into a large 11 ft. × 11 ft. dewatering spitz, placed in the centre of a 30 ft. diameter slime collecting vat with peripheral overflow. A 1½ in. nozzle on each spitz allows the heavy portion, about 180 tons dry ore, to pass out and flow over a canvas strake 100 ft. long, where 2 dwt. per ton crushed is retained. This pulp retains only 2½ water to 1 dry sand. At the end of this canvas table is a final dewatering spitz 8 ft. × 8 ft. A ¾ in. nozzle allows pulp to flow away to dam only retaining 1 water to 1 dry sand.

Light Portion.—Overflowing the 11 ft. × 11 ft. spitz, having a value of only 1 dwt. or under, this is retained in collectors, from whence it is periodically discharged to waste. The peripheral overflow of collectors gives nearly a clear overflow to return water pump. The overflow of final spitz, also containing light slime, is collected in a 40 ft. collector, the clear water being decanted off to filters connected to it through return water pump. The slime in this vat is also periodically discharged to waste.

Extraction.—Our total extraction averages 87.7%, having been gradually brought up to this point. The tails have been lowered to 1 dwt. and under, by untiring application to every favourable condition discovered by investigation. Cost per ton is high, 5s., but I have every expectation that this will now be reduced by from 3d. to 6d. per ton, due largely to the fact that the tube mills, which have proved troublesome and expensive in maintenance in the past, are now in a

satisfactory condition for running; also allocation of spares, which has been somewhat expensive of late, has now dropped 50%.

Before concluding this paper, I wish to acknowledge the courtesy of the general manager and consulting engineer for their kind permission to respond to a special request that I should write a paper for the Society on this subject.

The President: I am sure our best thanks are due to the author for writing this paper. It is very gratifying to have descriptions of processes, plants, and mining work in Rhodesia coming forward so quickly as they do. I only hope that our members up there will continue to keep us well informed as to what is going on. This paper is very interesting, as all the treatment seems to take place through concentration and amalgamation. There is no cyanide treatment at all, and the wonderful recovery of 30% of the gold on the plates following crushing through a ¾ mesh screen is very extraordinary.

Mr. W. A. Caldecott (Past-President): In seconding the vote of thanks, I should also like to express our indebtedness to Mr. Kingston for having obtained this paper for us when in Rhodesia.

In regard to the paper itself, I may say that the stamp duty of 15 tons at the Giant mine, which is one of the mines controlled by the Rhodesian branch of the Consolidated Goldfields of S.A., is probably the highest duty of any gravitation stamps in the world. This is mainly due to the soft nature of the ore, the liners in the tube mills lasting some seven months, and only 300 lb. of imported pebbles being required per day. As our President has remarked, experience has developed a mode of treatment which is in many respects a reversion to early practice, for it is hardly usual nowadays to recover the bulk of the gold in an ore by use of blanketings. As the author points out, satisfactory amalgamation recovery seems practically impossible, for not only is much of the gold rusty or coated with iron oxide, but the pyrites in the ore tend to foul the plates, so that in a short time after dressing the plates become inefficient. When the rusty gold in the ore is rubbed up in a mortar with cyanide solution and mercury, it amalgamates very imperfectly. Nitric acid is used for enabling the amalgamation to proceed in the barrel, as it is a solvent for oxide of iron, whereas potassium cyanide is not.

Prof. G. H. Stanley (Member of Council): There is just one point that occurs to me: we, most of us, know that nitric acid is not so good a solvent for iron oxide as hydrochloric acid. Why not use the latter therefore?

Mr. W. A. Caldecott: Nitric acid, sulphuric acid, and bisulphate of soda were experimented on, but nitric acid appears the best in practice.

A FEW NOTES ON CUPELLING GOLD-LEAD BULLION.

By GEO. MELVILL (Member).

As most of the mines now use the pan furnace for smelting, a few notes on the cupelling of gold-lead bullion are submitted in the hope that they will be of some interest to members.

The usual inside dimensions of the oval test frames used on the Rand are:—

3 ft. 5½ in. long by 2 ft. 3 in. wide by 7 in. deep.

A few years ago the tests were made of bone ash. On the Crown Reef the tests are made of mabor, cement and crushed fire brick, in the following proportions:—

Breast.	Body.
60 lb. new mabor	150 lb. used mabor assay cupels crushed
30 lb. White's English Portland cement	75 lb. cement
30 lb. best crushed fire bricks	75 lb. best crushed fire bricks
about 12% water	about 12% water

It is very important that each ingredient used, be passed through a very fine sieve (say, 1,600 mesh), as the finer the material the longer will the test last. The ingredients are thoroughly mixed and sufficient water added to make it fairly damp. If the mixture is twice passed through a 200 mesh sieve an even moisture throughout is ensured. The body of the test is prepared in exactly the same way as the breast, except that used assay mabor cupels crushed are used instead of new mabor. The breast and body are then put separately into the test frame, on top of which is placed a special frame 7 in. deep, so as to allow the whole mixture to be filled in one operation. It is first trodden down with the feet, then tamped with iron rods about 1 in. thick, and finally with the usual tamper about 2½ in. in diameter. The tamping, with two natives, should not take more than ¾ hour. It is a mistake to tamp tests, made with cement, too much, as this causes cracking. When the tamping is finished the top ring is removed, the surface levelled off, and the test carved out in the usual way, leaving the breast 12 in. and the sides 2 in. from the inside of the ring. The depth may be 4 in. with the sides nearly perpendicular. Four litharge holes (1½ in. diameter) are bored, and four channels cut, which are slightly sloped towards the holes. Three of the channels are 1½ in. wide by ¼ in. deep and one 1½ in. wide and

2 in. deep. The latter is used after all the bullion has been fed, in and saves cutting down, or too much tilting, when finishing. The channels wear down very slightly.

It is a great advantage to have six test frames, so that a test is never less than four or five months old when put into use. The test is allowed to set for at least seven days before turning it on its side, and must then be dried very gently. The cuttings out of the test are put through a sieve and used in the body of the next test. The test is supported on the usual carriage (see "The Metallurgy of Lead," by Hofman), which is made to tilt backwards or forwards by means of a regulating screw. When the test is in position the blower pipe is fixed. This consists of a straight piece of 3 in. pipe very slightly flattened at the end, and put in at an angle of about 30°. There must be no bend or hood at the end.

The dimensions of the cupel furnace can be seen in "The Metallurgy of Lead," by Hofman. The roof should be built with a 4½ in. baffle above the test side of the fire bridge. It helps to keep the flame down instead of travelling along the top of the roof. This is a great improvement, as a more even temperature can be maintained, with a small quantity of coal. It is a great advantage to have an independent flue and stack, so that the two furnaces, cupel and pan, can be worked at the same time. The feed hole of the furnace must be built large enough to allow two bars, one on top of the other, to be fed in at a time.

When a new test is being used the heat must be raised very gently, and it is advisable to put a couple of bars in the test when the fire is started. When the furnace is hot enough the lead-gold bullion is fed in until the bath nearly reaches the litharge channel. The heat is then raised a little higher than the usual working temperature, the air blast turned on, and when there is sufficient litharge the feed is pushed in gently until the litharge overflows. The feed is regulated by the amount of litharge in the bath. The working temperature should not be too great, just enough to allow the operator to see what amount of litharge is on the bath. Three and a half bars, about 1,000 oz. each, can be cupelled per hour, and with good clean bullion four bars.

After all the lead bullion has been fed in the temperature is gradually raised, and the test is tipped forward by degrees until all the lead is oxidised and litharge blown off. The gold is then subjected to a high temperature with a reduced blast to eliminate as much of the remaining base metals as possible. The test is now lowered and drawn away from the furnace and the gold is taken off before it sets too hard.

The life of tests on the Rand varies considerably. As many as 520 bars, 1,000 oz. each, have been cupelled in one of these tests in fifteen cupellations. This is probably a record. There is very little trouble with the litharge holes wearing back, and very often six cupellations are done through one hole. The sides of the test wear the most but these are patched with fresh test mixture after each cupellation, and then they are as good as new. With the blower pipe, fixed as before mentioned, the pressure of the blast should be great enough to make about $\frac{1}{2}$ in. deep ripples on the surface of the molten metal. The assay value of the litharge is $1\frac{1}{2}$ oz. As a rule, the gold recovered from the cupel agrees within 6 oz. of the gold called for in the lead bullion, the value of the latter being arrived at from a sample taken continuously during tapping of the pan furnace. The fineness of the bars from cupelled gold averages 842.

These few notes are offered in the hope that they will bring forth some discussion on the subject.

The President: I should like to move a vote of thanks to the author for his notes, which form a very welcome addition to Mr. Thomas' paper. Certainly the life of the "test" which the author mentions is very extraordinary. As to the tilting carriage, which he mentions, I believe the introduction of the device into practical use on these fields was due to one of our members—Mr. Q. J. Leitch.

Mr. W. R. Dowling (Member of Council): I would like to second the vote of thanks. Mr. Gibson, the smelter at the Knights Deep, after he has blown off the last litharge adds some flux to gather up the little remaining portions of litharge, and blows that off before attempting to take up the plate of gold. This helps to reduce the amount of base metal in the bullion.

Mr. H. Rusden (Associate): I should like to ask the author what type of blower he uses, whether it is of the Root's pattern or an ordinary fan? What material is used to fill the 2 in. deep channel when it is desired to run the litharge over the shallow channel? It appears to me to be rather a dangerous practice to put a couple of pigs of lead in the test when the fire is started, as moisture is frequently retained in the bottom for some time after the heat is sufficient to melt lead in the bath. Lead bullion is usually sampled by taking a dip after the slag has all been removed in the pan furnace, with quite satisfactory results; is there any advantage in sampling continuously during tapping? Our thanks are due to the author for bringing to our notice this mixture for test making which has such excellent properties; in

the past I have felt satisfied if a test has stood four to six cupellations, now I feel most anxious to give this mixture a trial, and hope the result will be as satisfactory as the author's.

Mr. G. Melvill: It is an ordinary Sirocco fan.

Mr. H. A. White: I see the author quotes 840 as the fineness of his cupelled bullion. I would like him to let us know the amount of silver and base metal present?

Mr. G. Melvill: The silver runs up to 10 or 11%.

Mr. H. A. White: On the May Consolidated G. M. Co., Ltd., we adopt a method for further refining the gold bullion. We add one or two bars of clean lead after the first cupellation is finished and then run this off. This has the effect of bringing up the average fineness to 860 gold and 110 silver; leaving about 30 of base metals in the bullion.

The Secretary next read Mr. Adair's reply to discussion on his paper, as follows:—

THE ADAIR-USHER PROCESS.

(Read at May Meeting, 1908.)

By ALFRED ADAIR (Member.)

REPLY TO DISCUSSION.

Mr. A. Adair (Member): In reply to the discussion on my paper, I wish to thank Prof. Yates for his appreciative remarks, and all those who have contributed figures or statistics.

Operations are carried on on such a large scale at the Rand, that the otherwise insignificant loss of 3s. per ton in residues, assumes almost tragic importance, because the loss extends to the large sum of between £2,500,000 to £3,000,000 stg. per annum, or about £7,000 every day. In my paper I have shown how some saving of gold is to be effected, but my critics are unable to rise to the occasion.

Supposing we could save, say, a million or so, or 1s. 6d. per ton. In the first place we should have so much more cash put into circulation, enough money to finance a couple of deep levels annually. But the more important consideration is that of making payable mines out of rock, which does not yield a profit now. There is sufficient of such rock in the Kimberley and other series, to add 50% to our stamping or crushing power, and it is only such an expansion as this, which will ever help Johannesburg as a city. It is considerations such as these affecting the

welfare of half the white population of South Africa which induces one to work at gold saving problems, and which helps one to overcome the difficulties and the more deadly inertia that confronts inventors. I believe in my work, and because of that belief am prepared to fight for it against all comers. These difficulties, or some of them, are evident in the discussion.

In the discussion on umber it is most exhilarating to have every one against me. Surely there must be something in such opposition; but when one comes to the examination one finds mere vague opinions, feeble ones, too, and not one with a firm basis. To one and all I reply, that I gave experimental proofs of each of my contentions, and not one of my opponents has been able to disprove or weaken one of them, viz., that umber, properly used of course, dissolves more gold, especially with weak cyanide solution, and dissolves it in less time. Further, that it conserves the cyanide and hinders the formation of prussiate, therefore it also adds to the certainty of good results. UMBER will be of greater service when more rapid processes are introduced, but under the present slow processes I have never claimed a difference of more than a few pence per ton, no plant is required at all, and it adds nothing, or next to nothing, to working expenses.

I now reply individually: Mr. J. R. Williams, beyond casting doubts about, has nothing to say. Both he and Mr. White rake up that old bogey, the Omnibus Patent. I confess my surprise at Mr. Williams. As a member of that remarkable body of discriminators, the Patents Committee, he ought to know that a man can no more obtain a valid patent for a list of oxidising agents applied to cyaniding than he could for a list of mechanical devices, which might be applied to, say, aeroplaneing.

I have replied to Mr. Melvill on the treatment of slimes, and I am also quite willing to demonstrate to him where he made his mistakes, and why he is getting poor residues now.

Mr. White is generally in a pessimistic mood, and therefore amusing. I think, however, he seldom allows his prejudices to run away with him so completely as in his discussion on my paper. With respect to umber, he advances several *reasons*, as he calls them, against umber. Thus: UMBER is not interesting, because, as he fondly imagines, it cannot be patented. His dinner cannot be patented. Therefore he ought not to be interested in dinner. Here is another of Mr. White's reasons: "There are only two oxidising agents in use." If there are but two billiard balls on the table, might there not be room for just *one* more? Another of these reasons of his, is, that because you cannot use the full theoretical amount of umber, you need

not use any, and so on, and so on. Mr. White is uncommonly careful about trifles of wording, but shuts his eyes to facts of importance. He has something to say under the next heading which substantiates this.

Mr. Croghan is rather late in the field, but is a "foeman worthy of one's steel;" he comes with armour in the shape of figures. I regret very much that he confined himself to the pessimistic side of the question. I should like to have seen those back figures he refers to, as his present figures are somewhat beside the mark. I do not take his figures as any real guide to actual practice, because he uses quantities out of all reasonable proportion, and entirely omits slimes from his tests. In testing any process, some attempt should be made to get within a reasonable distance of the practical conditions. Mr. Croghan gets a green solution, showing the presence of an alkaline manganate, an agent never occurring in actual practice but which may account for the loss in alkalinity. But under these conditions where Mr. Croghan in his 4% experiment used five times as much cyanide and almost ten times as much alkali as is common in slimes treatment; he also uses about five times as much umber, and also a manganate. Even under these conditions Mr. Croghan cannot point to a loss of alkalinity which may not be due to experimental error, .197 instead of .215. I should like to know how the manganate was produced. I have tried umber with all strengths of cyanide, and have not once got it. I easily got it by adding permanganate or sodium peroxide—I doubt that umber of Mr. Croghan's very much.

Some few years ago, one of the contributors hinted in the *Journal* at my venerable years and perhaps also ignorance of advances in science. I did not attempt to reply, because I had really forgotten a great deal. Now, Mr. Croghan deliberately suggests inabilities in theory—well, I love my chemicals, and the more I know about them the less inclined I am to lay down laws for them to follow. But to use Mr. Croghan's own phrase, it requires no "genius of theory" to explain these little matters. Mr. Melvill and Mr. Alexander looked in the wrong place for their acidity. Acidity generally comes from the ore or water, and before blaming umber they should have made quite sure that the acidity did not arise there, or from the effect in Mr. Melvill's case of carbonic acid in an unduly strong alkaline solution. In Mr. Alexander's case the presence of HSCN in his solution proved that sulphuric acid, or perhaps hydrochloric acid from chlorine works residues, had got into his slimes, as only a strong acid could replace the HSCN of the sodium salt. With respect to Mr. Croghan's figures, the manganate

may have had something to do with the destruction of cyanide. It is known to all water analysts that all nitrogenous organic compounds are decomposed by it, refer to Wanklyn, Chapman, and Smith (*Journal Chem. Soc.*, vol. v. [ser. 2], 1867, p. 445, *et sequitur*). Others merely express opinions, and these I have dealt with.

I now come to the point of discovering the why and wherefore of all this pessimism and objection to umber.

Fact No. I. So far no tests of the process have been made under the strict conditions usually applied to new processes, viz., tests on the same ore, faithful sampling and assaying conducted throughout with strict impartiality by impartial men. It was stated some time ago that "fair and extensive" trials had been made with umber, and at the same time no mention was made of the fact that I, as the patentee, repudiated these trials, in fact, I knew very little about them. In two of them I knew that the men were utterly incapable of undertaking such responsible work, and in another, the man who was in charge did not know for what purpose he was adding the reagent. No proper tests were made, and to an unprejudiced mind the use of the word "fair" in connection with such tests must bear a totally different meaning to the one usually associated with it. I have not the least doubt that this incorrect statement has misled many people.

Fact No. 2. The balances in general use on the field are incapable of ascertaining the gain or loss of 6d. to 1s. per ton as worked under the usual conditions. The ordinary balance can register only to $\frac{1}{15}$ oz. of gold on the assay ton *i.e.*, 6s. 3d. Notwithstanding the fact that $2\frac{1}{2}$ assay tons are quite sufficient to give good figures with a good balance, 5 assay tons or more have to be used with the common balance to get down to 1s. 3d. per ton, and on many mines the results are only taken at that figure. Hence the impossibility of hoping to show a gain of 6d. per ton, unless long times of testing and better balances are used.

The poorest kind of economy on mines is to supply assayers with inefficient tools. They lose more in a month from this cause than would pay for a new balance every month, because preventable losses of gold occur every day and on every mine. The moral of this is that when people do not know what they lose, they lose nothing, and therefore cannot gain anything.

The Usher plant has now been so generally accepted as *un fait accompli*, that no defence on my part is called for, but perfection is not reached in a day or a year, and there are doubtless defects in the plant, some of which I have already stated. The stand pipe system has been

generally adopted, because of its cheapness; in construction some £25 per 30 ft. vat is, I believe, about the figure, and the royalty is small. With the stand pipe, an extra 6d. to 10d. per ton is being gained on the Rand, and the question is whether it is best to secure the 6d. or 10d. per ton now, at an expense which is covered by a month's profit (not to speak of the gain in tank space) or to wait indefinitely until something better turns up. Such informations are coming, but it will take time and perhaps a different form of plant, so I am strongly of the opinion that the man who does not get the nimble 6d. or 1s. now, is behind the times.

My personal preference would be for the moving pipes, even at the extra cost, because it will give an extra 6d. per ton, and the liability to choking disappears; and no attempt has yet been made to improve on the original design. Another advantage is the saving in circulation. The experimental work in my paper points the way to success, and no cyanide man should be satisfied to-day, or for a year or two, until he has reduced his slimes residue to .15 or under.

Mr. White is again in a pessimistic humour with the Usher process. Here he requires perfection right off and, I am told, refuses to try to get his 6d. with the imperfect plant, and would rather wait for perfection. Mr. White's dislike to anything in the shape of novelty is so marked that one feels inclined to work off a few on him for entertainment. In this place he naïvely expresses his preference for uniform residues; varying results, even if they show a gain on the whole, are horribly disconcerting to this precision. He sees defects. Why? We all see them. Why does he not try to improve on them, rather than to shut his eyes to his own obvious disadvantage?

Mr. Graham follows, but he is really too reckless in his language, as I shall presently again point out. It was not from "conflicting statements" that I did not give results, but literally and truly as stated by me.

Mr. Pearce refers to G. S. Duncan's patent. I can assure Mr. Pearce that neither Major Usher nor myself were acquainted with this patent, but having seen it recently, I can see the difference which made the difference between failure and success. Mr. Duncan's plan is unworkable on Rand ores.

All Sliming.—I used the term to avoid coining such a term as "The New Metallurgy," but it is apt to mislead, and therefore alter it in my case to "a continuous single treatment process," as, at any rate, unobjectionable on the score of grandiosity.

My description was intended to show the principle only, since a full description would have

made the paper wholly unwieldy. And as yet I have not completed drawings.

Mr. Pearce's criticism is the best and calls for a full reply. He is perhaps right in thinking me optimistic about sands being ground for 1s. a ton milled. I admit that, as I was also optimistic in '87, but it is not founded on fancy, as I shall show. I also admit Mr. Pearce's right to speak on tube mills, he has many under his control; why did he not give us some figures? They would be really useful. All my critics fight shy of figures. What is there to be afraid of?

Mr. Pearce seems to be overweighted with Rittinger's theories. I greatly admire the simplicity of his conceptions and the clearness of his expositions myself, but they are purely mathematical, and on a subject which is hardly possible to reduce to mathematical terms. Unless supported by practical results they are pure theories and next to useless for practical purposes. Apart from the irregularities in sizes, shapes and hardness of any given rock, which defy accurate calculation, there are the different machines which may be used—percussive, crushing or grinding. There are also the difficulties that percussion may be employed for one grade, shearing or crushing for another grade, and grinding for a third. The problem will never be solved by mathematics only, perhaps not even be guided thereby. It is therefore useless to wait on that account. Mr. Pearce thinks fine grinding will reach an impossible figure, but he is afraid to come to figures. I point to cement works where the requisite fine grinding of a hard clinker is successfully performed at reasonable cost. But to come nearer. In a week's run on the Rand, in the Adair-Cruickshank mill, ordinary battery sands were ground to 80% through 150 mesh, and all through 90. The I.H.P. accurately determined was 15 for each ton of sand ground, including friction (unknown). Since then, 1893, the cost of everything has been reduced, and the total cost would now be for metal ground, oil, attendance and power, less than 1s. per ton of sand ground. So I was not depending merely on tube mill results.

The tube mill commends itself to me on account of its capacity and simplicity and the certainty of greater efficiency in time. Its performance is not bad now. Mr. Graham gave us the last figures for the Rand, omitting decimals 68 of +60 and 20 of +90 mesh were reduced in one operation to 11 of +60 and 27 of +90 mesh, 50 of the 88 were therefore put through the 90 mesh; evidently it would cost much less to similarly treat the remaining 38 than the original 88. If Mr. Pearce will compare this 50% and allow 25% for slimes washed out he will see that I did not overstate the grinding effect. The figures for this first grinding have been stated to

be as low as 4d. on the ton milled, but even if we allow 8d. the total cost should not exceed the 1s. per ton milled quoted by me. If the tube mill will not do it, then we must revert to other mills that will.

No one seems to have noticed that beside the extra recovery in gold the cyanide consumption would be reduced to one-half. The water consumption lowered by one-third to one-half, zinc saved and much labour; against these items of gain, gold, cyanide, water, zinc and labour, comes the sole item of grinding, and the nett profit therefore works out at 1s. 9d. to 2s. or more per ton over partial tube milling, or 3s. or more without the tube mills. It is on this account that I am utterly opposed to Mr. Pearce when he talks about "making haste slowly." So long as we are losing £7,000 daily we can afford to lose a good deal in hastening, as the £7,000 is irrecoverable once away from the plant. We are sure to make mistakes and will never succeed without making them, whether we start now or wait until another million has gone. I would therefore urge him to move now and leave the waiting and the "pros and cons" to those who have no need to hurry.

On this subject Mr. White opines that I have overlooked something, and although it is buried in words, I infer that it is in reference to the time of treatment of fine sands. It is strange that Mr. White makes no suggestion of his own, and his quotation from Mr. Lea's paper conveys no information on this point. It would be indeed strange if I overlooked one of the main advantages of fine grinding and of my process. Most of us are aware that 70% of the gold in coarse tailings is set free by fine grinding. If Mr. White will take the trouble to study the plant, he will see that there are three new conditions introduced which differ from the sands process, two of which at least favour quicker solution than in the sands plant. I hope he will find them. It is a pity that Mr. White is so very conservative.

As to Mr. Graham, I quite envy his gusty language, it is, however, too reckless, as already pointed out.

Mr. Pearce thinks my statements optimistic, but Mr. Graham says they are misleading, and is promptly moved to refute them. Alas, for brave words, for he does not refute them, not even by deputy.

In the next sentence but one, he asserts that Mr. Pearce has pointed out errors in my reasoning based on false premises. Alas, for Mr. Graham. Mr. Pearce does nothing of the kind. Again, I never said that reduction to -90 mesh was all sliming. In another sentence Mr. Graham refers to my "visionary" description of the plant, I admit its vagueness, but then he

proceeds to adjudicate upon something which is merely visionary.

I am glad that one remark of his affords me the opportunity of giving precedence and the honour of introducing a continuous process to Mr. E. T. Rand, and also of expressing my sympathy for his undeserved bad luck. I also sympathise with Mr. Graham in his disappointment with my paper, I was disappointed myself. But when I come to reply to Mr. Graham's criticisms and look for some point to reply to, I can only find visionary phantoms of unsubstantiated illogicalities. Nevertheless, his youthful indiscretions have been enjoyable and made one half wish, well, I do not know what.

Mr. Jamieson's Results.—I was surprised that no one came forward with figures to dispute the statement that we are losing £400,000 per annum over and above what is known, by an assay error, not previously recognised. The best method does not show the whole loss, but is much superior to the methods practised hitherto. It is, however, too tedious for ordinary work, but the loss is too important to neglect, and I would suggest that it be reserved for weekly samples.

Mr. Pearce, Mr. White, and Mr. Graham refer to dissolved gold; why? I do not know, because it had nothing to do with the results obtained by Mr. Jamieson, as the sample was distinctly referred to as a sample of slimes, not of residues, hence never had contained any dissolved gold. There is not an assayer in practice who does not know about dissolved gold and does his best with it. But the assay of a sample, is a thing apart from the preparation and is not often attended to by the same man. The gold is got in the assay, whether it had been there originally in solution or not.

Personally I had nothing new to offer on the subject of preparing the sample, and therefore held my peace.

The President: We must thank Mr. Adair for his spirited reply to the criticisms on his paper. I will now ask Prof. Stanley to reply to the discussion on his paper.

A LABORATORY COMPARISON OF TUBE MILL PEBBLES.

(Read at June Meeting, 1908.)

By Prof. G. H. STANLEY, A.R.S.M., M.I.M.E., M.I.M.M. (Member of Council).

With an Appendix on Liners, by M. Weber.

DISCUSSION.

Prof. G. H. Stanley (Member of Council): There has been no discussion on this paper, and therefore a reply is scarcely called for. I would,

however, take the opportunity of thanking you, Mr. President, personally, for your appreciative remarks. I regret that none of our members who have been experimenting with different types of liners have come forward with any figures bearing upon their comparative life, but perhaps these will come forward in some other connection. I hoped to have been able to give you this evening the actual length of the life of the liner mentioned in the paper; I have written for the particulars, but they have not yet come to hand. Still I hope it will appear in the next *Journal*.*

BATTERY AND CYANIDE GOLD SMELTING.

(Read at July Meeting, 1908.)

By A. THOMAS (Member).

DISCUSSION.

Mr. A. H. Hartley (Member): The author is fortunate in having only a difference of 0.14% on weight of amalgam between that and the weight of bullion and mercury after retorting. I find there is a constant difference of from 0.5% to 0.75%, or an apparent loss of from 25 oz. to 40 oz. on 5,000 oz. of hard, machine squeezed amalgam, as against the author's 7 oz. I made a series of tests for moisture and found that by drying slowly for 8 hours at a temperature of 120° F., or until the weights remained constant, the percentage difference was almost identical with the per cent. deficiency after retorting in bulk. When drying the samples I used small trays of pure copper, and loose lids of the same metal in order to avoid any risk of loss in transfer and to detect any possible volatilisation of mercury. In comparing this 0.75% to 1% of moisture with that contained in any other hard pressed material, it is not abnormal.

The author rightly emphasises the necessity for thorough and careful calcination in connection with the cyanide clean-up. He might also have profitably dilated on the advantages of a thoroughly equipped acid plant. The present method, in many cases, of continually adding H₂SO₄ to a liquid saturated with zinc sulphate is not the most economical one. Nor is the practice of pumping the same liquid, partially diluted, through the filter press, there to be only partially washed, to be recommended. These are only some of the bad effects of an inadequate plant. It is a matter of time, or plant. The time is never available, and the necessary plant not always.

* This liner lasted 111 days.

The President: When reading a book called "Australian Mining and Metallurgy," by Donald Clark, B.C.E., some time back, I was struck by the stress laid therein upon the application of refining flux to "unmelted" retort metal; and the necessity for allowing the flux to act upon the metal in that state "since fluxes have very little refining effect upon melted bullion." Clark also recommends in the case of retorting amalgam, which may be somewhat impure, that nitre be placed in the retort with the amalgam, and as the matter may be of interest in connection with the subject of the paper under discussion I will quote the words of the author of the book mentioned. On p. 491, when dealing with the retorting of amalgam, Clark, having regard at the moment to an impure amalgam, says:—

"A small quantity of some oxidising material, such as nitre, may be placed in the retort with the amalgam; in this case, however, paper, rags, or other organic matter should be excluded. A little nitre, bichromate of potash or other oxidising agent may be added to a slight depression made on the surface of each ball of amalgam."

The method recommended appears to me a very extraordinary one. I have had no necessity for trying it, but I should like to know if it has been tried by the author or by any of our members, and whether the result has proved beneficial or otherwise.

Further on pp. 492-3, when dealing with the melting of gold, Clark says:—"When impure gold is melted it is practically impossible to purify it by mere melting, even with the addition of fluxes. Nitre, carbonate of soda, salt and such materials only attack the impurities in the gold to the slightest extent, while the crucible is attacked violently. When retorted gold is dirty looking or discoloured, it should be melted in a clay pot. The spongy retorted gold, if not very dirty, may be fed into the pot with some nitre. A layer of finely ground salt should also be added; the salt itself has practically no action on the gold or impurities, but as it becomes very fluid when molten it prevents the slags from frothing. Nitre should be added from time to time and the temperature kept low. The base metals will be turned into oxides, and when the gold melts it will separate from them. By prolonging the heating before melting, a greater amount is separated than if suddenly melted, for once gold has melted fluxes have but little action upon it. As soon as the nitre ceases to attack the base metals the temperature is raised, the whole lot melted and poured. The slag in this case consists of salt, which is fluid, and stops on the top of the mould, and oxide of potash containing oxides of the base metals. The slag will easily wash off with water leaving a very clean gold. When the

retorted gold is very dirty it should be melted down in a clay pot as before, but when it is necessary to raise the temperature, borax instead of salt should be added, the borax dissolves the oxides of the metals which have separated out and makes a fluid slag, allowing the gold shots to sink into the main mass. Sometimes, if borax is not added, lumps of oxide of copper or other oxides will form a crust on top and prevent all the gold being poured out. By taking care, the most impure bullion may be purified to such an extent that it contains no base metals; that is only gold and silver. It is quite a mistake to think that base bullion can be purified to any extent by fluxing alone after it has once been melted."

In these days here on the Rand our amalgam is so well cleaned and the retorted metal consequently so pure that refining is seldom necessary, but in other districts, and especially where small mines are being worked it is, I believe, the case that a considerable amount of impure material has to be dealt with, and as the handling of such has not been touched upon by the author the probability of the information being of use to some will form my apology to Mr. Clark for quoting from his work, and to the Society for placing the above extracts before it.

Personally, I have almost always been able to get amalgam clean enough not to require any subsequent treatment beyond a little borax soda flux, which has been sufficient to take up any slight dross or earthy matter which may have been present; but should there have been a dirty lump among the bulk of retort metal, a few pinches of nitre placed on the centre of the bare surface of the molten metal, followed by borax soda flux to take up the nitre slag, has been all that was required, and in the event of sulphur being present from any pyrites left with the amalgam, a good stirring with an iron rod before pouring. Of course, the point which strikes one in Clark's book is, as I mentioned at first, the necessity for keeping the unmelted metal under the influence of the flux, and I should like to know if the author or other of our members who have to deal with bullion melting consider that this is correct procedure. Naturally, dealing with cyanide slimes in a finely divided state is quite another matter.

Prof. G. H. Stanley (Member of Council): It is generally assumed that calcination is the operation which is most liable to result in loss of gold, partly by true volatilisation and partly by mechanical carriage in the fume and gas evolved. It would seem, however, that the loss in the former manner is very small, in view of the negligible vapour tension of gold at the tempera-

tures employed, and the very small effect on this constant, of the presence of zinc and other volatile impurities, such as mercury and tellurium, notwithstanding the very common belief to the contrary.

Bearing on this point, I can cite the following sets of experiments:—

K. Friedrichs¹ (J.S.C.I., Apr. 30th, 1903) made many experiments on the volatilisation of gold-zinc alloys, in which the proportion of zinc varied between 8 and 21,000 times the amount of gold present, and temperatures from 1,050° C. to 3,000° C. The most important results, from the point of the present discussion, are, that at 1,070° with 15 times as much zinc as gold, heated for twenty minutes, the loss was only .28 per 1,000, and with 23 times as much, heated for thirty minutes, 1.6 per 1,000. Losses were only very large at the highest temperatures with abnormal quantities of zinc. With the lower temperature and much less proportion of zinc present in calcination, the loss by true volatilisation can be but small.

Holloway and Pearse² (*Mining World*, Feb. 8th, 1908) found by careful experiments on rich telluride gold ores containing mercury, and with others to which cinnabar had been added; that no loss of gold occurred by pure volatilisation, pointing to the fact that the losses which did occur in practice were purely mechanical.

Rose³ also finds that on cupellation at 1,000° C. in the presence of various volatile impurities, no volatilisation loss can be detected.

The possibility of mechanical loss, however, is much more serious, and a strong draught could undoubtedly cause a comparatively heavy loss. Attention to this point is therefore essential, but convincing experimental evidence, as regards the value recoverable, is still wanting to justify the erection of dust catching chambers.

With regard to volatilisation losses during bullion melting: Rose⁴, (*Engineering and Mining Journal*, August 17, 1907) found that pure gold begins to volatilise just below 1,100° C., and that the rate of loss at 1,250° is four times as great as at 1,100° (he says, though, that volatile impurities increase the loss, but of course, these temperatures are considerably higher than in calcination). The volatilised gold condenses almost at once, and is carried forward by the furnace gases, some of it at any rate, being deposited in the flues. Dust from flues at the London Mint carried 1.39% gold, and at the Sydney Mint, dust even from the coping stone of the stack carried 1.46%.

In view of this volatilisation loss a dust collecting chamber was built at the Philadelphia

Mint, which cost £180, and which in six months collected gold worth £900, or 0.1 oz. per 1,000 oz. melted, which Rose states is about the maximum loss. Obviously, on a mine, this proportion would seldom be worth recovery, though in mints and similar situations it is quite worth while on account of the much larger quantity melted. I remember a case at Birmingham, too, where it was stated that the dust from some old furnaces and flues, which were demolished, was of sufficient value to pay for a new set.

Obviously, loss by volatilisation is increased by exposing the gold to a current of gases, in the same way as water evaporates more quickly in a draught, hence the importance of keeping crucibles covered.

Richards¹ gives a very interesting study of the volatilisation of gold and silver in the *Electrochemical and Metallurgical Industry*, March, 1908, in which he states that a vapour tension of .0002 mm. Hg is sufficient to make a metal show signs of volatilisation; this should be reached with gold at 942° C., *i.e.*, while the metal is still solid—we have a parallel in the evaporation of ice as such—and it has actually been observed (*vide* Rose and others) at below 1,100° C. when its vapour tension is about .003 mm. At about 1,300° it increases to .093 mm., quite sufficient to cause slow volatilisation.

Now to apply this to our conditions: Supposing the furnace gases passing over the gold are at 1,300° C., if saturated with gold vapour it is easily calculated that each cubic yard will carry .34 gr. of gold value .68d. Of course, in practice, the air does not all pass over the gold, and as a result is far from being saturated, so that the loss is fortunately not as great as thus indicated; nevertheless, it is a point worthy of consideration when thinking of melting gold in a reverberatory type of furnace, where it is exposed to the furnace gases (as for instance in cupellation), rather than in crucibles, which can be closed by a cover.

I should like next to refer to some experiments of my own in smelting zinc-gold slimes direct, without acid treatment: they are about two years old now, and I had hoped to find time for more work on the point, but this, unfortunately, has been impossible.

On seeing the slimes treated with nitre cake solution instead of sulphuric acid, it occurred to me that the same result might be obtained by fusing direct with the nitre cake; afterwards raising the temperature to decompose the base-metal sulphates formed, and fluxing the oxides thus produced by an acid flux such as borax or silica. I find since, that the first part of the above scheme had occurred to others also, but

1. See this Society's Proceedings, vol. iii., p. 438.

2. See *May Journal*, 1908, p. 357.

3. See *Jan. Journal*, 1905, p. 167.

4. See *October Journal*, 1907, p. 131.

1. See *July Journal*, 1908, p. 33.

not, so far as I am aware, the completion of the conversion to melted gold, by this method, in one operation.

The first sample of slime I experimented with carried about 20% of gold, and with the small quantity available I was able to make seven tests on quantities of 10 gm. at a time, with borax varying from 50 to 100% of the weight of slime and nitre cake from 100 to 400%, the fineness of the resulting beads varying from 756 to 947. The highest value was obtained with a charge of 4 parts nitre cake, 1 part borax, and 1 part slime, and the button was very malleable.

From the point of view of cost, the best result was obtained with 2 parts nitre cake, $\frac{1}{2}$ part borax, and 1 part slime, the button assaying 866.5, and again being malleable. N.P. A second series of experiments was made with a much poorer slime, running only about 1.8% gold: on this five tests were made with 20 gm. lots of slime, the borax in each case being 50% by weight of slime and nitre cake ranging from 100 to 250%. The fineness of the resulting buttons varied from 832 to 987, the highest, as before, being obtained with the highest proportion of nitre cake, but with 150% nitre cake the button still assayed over 900.

On the large scale such a process could be carried out in clay lined crucibles or a small reverberatory furnace, the cost per ounce of gold would probably be less for chemicals and fluxes, though probably more for crucibles and fuel, at any rate in the former case, on account of the larger bulk to be smelted; though as some set-off we have calcination omitted. It would, however, apparently offer considerable advantage in these directions, namely, lessening risk of loss and saving of time.

ZINC DUST PRECIPITATION AT CERRO PRIETO.

By ROBERT LINTON (Associate).

(Read at September Meeting, 1908.)

DISCUSSION.

Mr. S. H. Pearce (*Past-President*): The author's paper on zinc dust precipitation is extremely interesting from the point of view of its re-awakening our interest in the subject. It is of special interest to me, because I had the opportunity some four years ago of seeing the process in operation at the Homestake, and was certainly envious of the facility of cleaning up, as compared to the operation as practised on these fields. I made up my mind there and then that it was exactly what we wanted here,

so as to save expense in the material used, and labour in handling the gold slimes. Added to that, there was the advantage that a total clean up was made at any period desired, and there was a minimum of loss by theft. To this end, I persuaded our people to put up the money for the experiment, and Mr. Graham of the Goldenhuis Deep to undertake a series of tests to see if it was applicable here. The final results are expressed in the fact that we are still using zinc shavings. It should not be inferred from this that it is finally settled that the process is not applicable here, for, personally, I am inclined to think that the failure of the experiment was perhaps due more to want of knowledge of all the necessary conditions, rather than to the defects of the process itself.

It is natural to assume that zinc should precipitate gold from cyanide solutions as effectually in the form of dust as in the form of shavings, and I am still somewhat dissatisfied at the failure experienced.

I have read the author's paper very carefully, because I hoped to find a difference of condition that we had failed to fulfil, and that there would be some encouragement to try it again. I find there certainly are some essential points of difference, the chief one of which is the strength in cyanide of the solution treated. The author mentions solutions "as low as 0.7% KCy" being amenable and yielding the gold without difficulty. This was also our experience, inverted, as we found that solutions "as high as .05% KCy" would give satisfactory results, whereas the ordinary slimes solution which runs often below .01% were very unsatisfactory. In fact, it was just that failure to obtain precipitation from the slimes solution that caused us to abandon our experiment. The reasons for this are, of course, obvious to those acquainted with our methods here, but I have frequently found some difficulty in explaining them to would-be introducers of new methods of precipitation. For the author's benefit, therefore, I may state that inasmuch as we have found it economical and expedient to use fresh "lead coated" zinc shavings for slimes solutions, and when these are no longer "active," by being fouled by any deposit from this weak solution, they are transferred to other boxes used for the stronger solutions from the sand plant, and become practically as effective as new or fresh zinc for that purpose. It naturally follows then, that this is the weak link in the chain, and any system should be tried primarily upon these solutions, otherwise there would be no means of disposing of the poorly coated zinc, except by means of the acid tub, and considerable expense incurred in the use of both acid and zinc.

I would therefore, ask the author to consider if the conditions we have, could be met, so that the adoption of "dust" here would prove an economic success. I do not think the cost of refining would be a drawback to the process here, seeing that it would be readily run down in a lead furnace, and probably quite as easily as our present zinc precipitate.

As far as our trials of this method at the Geldenhuis Deep are concerned, it is hardly necessary to describe them in detail. The plant consisted of two 10-ton tanks worked alternately. The zinc dust was added as an emulsion to the solution therein and agitated with compressed air for variable periods, as well as during its being pumped through the press. The filter press used was a Johnson 24 x 24 in. gold clean-up press. The trials consisted of varying the amounts of zinc dust, with and without lead acetate and also added cyanide, and passing the solution through the press at varying rates. Defects in the method of filling the press were detected and remedied and, in short, the method was tried under every condition which we could think of, in order to obtain commercially successful results. In looking over the matter, there is one thing that was certainly neglected, viz., the testing of the zinc itself. This may perhaps be of more importance than was considered at the time, but I take it that, however impure it may have been, the use of a larger quantity should have rectified matters, which was not so in result.

The chief reason for the abandoning of the trial was because we found that unless cyanide was added at a ruinous rate to this dilute solution, the precipitation of the gold contents was only effected at such a slow rate as to put the process out of all practical adoption, viz., at 0.5 ton per sq. ft. of filter per 24 hours at most.

The conditions under which satisfactory precipitation is obtained by our existing method are as follows:—

Tons precipitated per 24 hours, 800.

Cyanide strength leaving filters, about .01%.

Lbs. of cyanide added per 24 hours, 80 lb.

Cyanide strength of solution leaving boxes, about .008%.

Gold contents of solution entering boxes, 0.66 dwt. per ton.

Gold contents of solution leaving boxes, 0.02 dwt. per ton

The consumption of zinc in precipitating slime solution is not estimated for reasons already given. Taking it over the whole plant, it works out on an average to a little over .25 lb. zinc per ton treated, irrespective of the grade of the material treated. If expressed in relation to the gold recovered, it would vary from 1 to 3 lb. of

zinc per oz. recovered. These figures are considerably in excess of those quoted by the author, and it would be interesting to know how much of this difference is due to the value of the material treated, and how much to our system of precipitation? Perhaps he will supply us also with these figures, so that we can make a comparison on parallel lines.

The author certainly deserves our thanks for his paper, and it is to be hoped that others will follow his example in that part of the world, for, notwithstanding anything that may be understood to the contrary, we are only too pleased to take tips from our fellow workers abroad and to reciprocate in kind.

SMALL MINES OF RHODESIA.

(Read at September Meeting, 1908.)

By B. I. COLLINGS (Member).

DISCUSSION.

Mr. C. B. Kingston (Member of Council): The author is to be congratulated upon having contributed a paper of great interest and value. The small mines of Rhodesia are becoming important factors in South African mining to-day.

Rhodesia has afforded a welcome outlet to many of the energetic and enterprising men of our community, who have been forced out of their positions by the recent depression. To these have been added other men, of whom there are many in every mining camp, who are attracted by the chances of sudden fortune, and with whom the security of a fixed salary weighs as nothing against the charm of independent action. These men have found an attractive field for their energy in the Chartered Territory, and Rhodesia has been given an opportunity to rehabilitate a mining industry possessed of great intrinsic possibilities, but which was languishing in neglect as a result of the mistaken methods of the pioneer mining companies of the fields, which committed the perhaps natural blunder of attempting to deal with its widely differing ore deposits, on the conventional lines of the Rand. The difficulties into which many of the early Rhodesian companies were plunged may be traced directly to this cause.

When working capitals had been spent in exploring ore deposits that were not quite clearly understood, and as the good chutes had not been developed sufficiently to expose satisfactory reserves of ore for milling purposes, shareholders became discouraged, and directors, in the absence of further capital, were driven to letting the mines on tribute in order to keep the enterprises alive. The early tributors secured very favourable terms and made handsome profits. In

addition to having main shafts sunk and equipped, and hundreds of feet of driving done, they, in some instances, had complete milling plants placed at their disposal, and even found ore ready for stopping. They did more or less development work according to the length of their lease. They preferred to do rather less than more, so long as there was ore in sight. Under these circumstances their success was immediate, and quickly attracted the attention of others on the look out for openings. When the available tributes had been absorbed, attention was given to new properties. This led to the development of the numerous individual enterprises included by the author under the general heading, "The Small Mines of Rhodesia."

The extent to which general mining development has grown in Rhodesia will be understood, if it is realised that there are now about 200 producing properties contributing to the published returns of the Government Mining Department. Seventy-five per cent. of these may be included under the heading of "small mines." The balance being operated by companies.

Of the "small mines," some are rightly so called, because they contain a very limited tonnage of pay ore, others are included because of the limited scale upon which they are being worked. Among the latter are some that may be expected to expand into very important enterprises in course of time.

The total output of the Rhodesian mines has already reached a large figure, and would have attracted even more attention than it has, if it were not overshadowed by the phenomenal returns of the Rand. An output of more than £200,000 per month is by no means insignificant, although not more than $\frac{1}{10}$ of the Rand production.

Having spent many years in active practice in Western Australia, the writer has been struck by the many points of similarity between the two fields. Nearly every deposit seen by him in Rhodesia has its counter part in Western Australia.

Rhodesia, it is true, still lacks the Golden Mile of Kalgoorlie, but there is still room in the country for the discovery of a group of mines worthy of comparison with that magnificent area. The pioneer in Western Australia was the prospector and small miner. The "small workers" are doing over again in Rhodesia what was done by their fellows in Australia. These men repay study. They are filled with an abounding energy, and are possessed of an inextinguishable optimism. A little success makes them self opinionated to an almost incredible degree, and their fads are frequently elevated to the dignity of profound theories. Their disregard for accepted theory is not altogether without advantages, because it sometimes leads to the discovery of ore deposits that might be overlooked by a too rigid scientist.

The reverse is perhaps equally true. They are for the most part content to deal with facts as they find them during the day and to leave the consideration of theory for the camp at night.

There is probably more technical knowledge among the Rhodesian prospectors and miners than has been the case in other countries, because so many come from the great training ground of the Transvaal. The knowledge gained here, however, must be used with care in connection with the Rhodesian ore deposits, and there are many failures awaiting the man who is careless in transplanting his Rand experience to the Rhodesian field. They soon learn wisdom, however, and men remember the lesson taught by mistakes for which they have to pay out of their own pockets.

The "Small Mines of Rhodesia" represent one of the most interesting of recent South African developments. The men engaged in the work are doing good service for the State. If the motive power of the individual is self interest, the class as a whole, is proud of the work they have done in advancing the development of the country and in disclosing some of its great possibilities.

The author has contributed the first of what may be hoped to prove a series of papers on a subject of great interest. He has broken new ground by his general paper. There remains much to be done in collecting detailed descriptions of the different types of deposit that are being exploited by the "small worker."

The biggest worker of Rhodesia was he whose name the country bears, and his prophecies as to its mining possibilities are being rapidly fulfilled by the "small man," who in the end may prove to be bigger than he looks.

I hope the members of our Society, resident in Rhodesia, may bestir themselves to supply our records with papers and notes covering this fruitful field that lies ready to their hands.

The meeting then closed.

Obituary.

The following deaths are recorded with much regret :—

Mr. J. W. KILBY, assayer of the South Randfontein G. M. Co., Ltd., who died suddenly on the 21st October, 1908. Mr. Kilby was admitted an Associate in February, 1903, and was transferred to membership on the 20th August, 1904.

Mr. JOSEPH HENRY COMBELLACK, mine captain, who died at the Roodepoort Central Deep Mine, Roodepoort, on the 4th November, 1908. Mr. Combellack was elected a Member of the Society in March, 1903.

Visit to Dynamite Factory.

A party of about 70 members visited the Dynamite Factory of the British South African Explosives Company, Ltd., at Modderfontein, on Saturday afternoon, the 24th October, 1908.

On the arrival of the special train about two o'clock, the party were received by Mr. W. Cullen, the general works manager, and other leading officials, and shown over the extensive works and its departments, general laboratory, explosives factory No. 1, box factory, dope factory, tradesmen's workshops, flour mill, sulphuric acid (contact and chamber process), nitric acid, concentration of nitric and sulphuric acids, denitration, lead pipe making and collodion cotton blending, under the guidance of Messrs. W. Cullen, Bachmann, Colquhoun, McIntosh, Lyall, W. Colbert, Donaldson, Campbell, Dr. Weiskopf, W. Waters and G. F. Ayers.

The following is a short description of the factory and its manufactures:

The factory is at present manufacturing about 220,000 cases of explosives per annum, but it is capable of producing 400,000 cases without any addition to the existing plant. The number of people now employed is 240 whites and 700 natives. All the officials and employees live on the property, which is 5,193 acres in extent.

The explosives now being manufactured are designated as follows:—Blasting gelatine, gelatine dynamite, gelignite, dynamite No. 2 (various grades) and coal dynamite. The first three belong to the so-called gelatinous class, and the latter two, as their names indicate, are modifications of the old kieselguhr dynamite (generally called dynamite No. 1), which is not made in South Africa. The basis of all five is of course nitro-glycerine, of which coal dynamite contains only 20% and blasting gelatine 93%. The manufacture of further varieties is contemplated.

Sulphuric acid, nitric acid, and hydrochloric acid are also manufactured, but while the first two are incidental to the making of nitro-glycerine, the last is not. All can be supplied chemically pure.

The company also manufactures lead pipes up to 3 in. diameter, and will shortly be in a position to make sheet lead of any thickness. The manufacture of various other chemical articles for which there is a demand in South Africa will be undertaken at an early date.

The manufacturing operations are highly technical, and the company employs 12 chemists, several of whom are constantly engaged in research work. An immense quantity of water is used in the various processes, and this is pumped from either of the three dams, the total capacity

of which is 250,000,000 gallons. The main supply is now being obtained from dam No. 3 abutting on the farm Frankenwald.

At the conclusion of the inspection the party was entertained at a conversazione at the casino. During the course of this function the President (Mr. R. G. Bevington) proposed and Mr. A. McA. Johnston (Vice-President) seconded, a hearty vote of thanks to the company, the general works manager and the staff for their courtesy, kindness and hospitality, and alluded to the great improvements and the extensions to the works which had been carried since the last visit.

Mr. Cullen in responding gave an interesting résumé of the paper subsequently read at the November meeting, which appears in full on pp. 144—151 of this *Journal*.

An interesting demonstration of a new system of shot firing by electricity was also given by Mr. Cullen, Dr. Weiskopf, and other members of the staff.

Notices and Abstracts of Articles and Papers.

CHEMISTRY.

THE EXISTENCE OF PLATINUM METALS IN CHEMICALS USED FOR ASSAYING. — "During the past twelve months upwards of fifty samples of rock have been assayed in the Government Analytical Laboratory, Cape Town, for platinum, in all but two cases with negative results. These two exceptions having yielded such minute traces it was a matter for considerable surprise when a sample of acid dolerite from Stutterheim in the Eastern Province, and a sample of white quartz from Swellendam, in the Western Province, assayed at the same time, *i.e.*, some four weeks ago, were each found to contain 4.8 gr. of platinum per ton of 2,000 lb.

The possibility of the platinum having come from the rocks appeared to be so remote that it was deemed advisable to test the assay materials in order to ascertain whether they were platiniferous. The hydric sodic carbonate having been proved free from platinum on previous occasions, only the litharge and the borax were examined. In place of possibly platiniferous rocks four assay ton charges of sea-sand were used, and it was then found that when 160 gm. of the litharge previously used were made into a charge with platinum-free borax, a nil result as regards platinum followed, whereas, when 40 gm. of the borax employed in the above assays was coupled with platinum-free litharge, 0.04 mgm. of platinum was invariably obtained as the result of the assay.

In such cases the silver bead from the assay showed the peculiar dull spongy appearance which is characteristic of beads containing over 5% platinum. On dissolving this particular bead in hot concentrated sulphuric acid a few specks of a black powder were left which weighed 0.04 mgm. This powder, when dissolved in aqua regia, the solution being evaporated to dryness and the residue taken up with water, gave the platinum reaction with potassium iodide. Test

it should be thought that the platinum could have been derived from the sea-sand an assay was made containing only borax, platinum-free litharge, and sufficient carbon to produce a lead button. In this case, too, the bead produced contained the quantity of platinum already stated. In order to confirm this result further assays were undertaken, one on 200 gm. of borax and another on 1,000 gm. of borax, with the following results:—

200 gm. of borax yielded 0.26 mgm. of black powder, 1,000 gm. of borax yielded 1.42 mgm. of black powder.

The 0.26 mgm., when converted into chloride and treated with ammonia chloride and alcohol, yielded the characteristic yellow precipitate, which, upon ignition, was converted into spongy platinum, and from this was once more obtained yellow ammoniac chloro-platinate.

The 1.42 mgm., when treated with aqua regia, did not completely dissolve, but left a residue weighing 0.12 mgm. The residue did not appreciably lose weight after eight hours' treatment with boiling aqua regia, and appeared under the lens as greyish metallic spangles, which were unaltered by ignition. Unfortunately there was not sufficient borax available from which to obtain enough of this substance to determine its nature. Having regard, however, to the foregoing tests and the manner in which the residue was obtained, it seems highly probable that it consists of iridium osmide.

The chloride soluble in water was converted into ammoniac chloro-platinate.

The results available, therefore, are as follows:—

40 gm. borax used for assaying dolerite gave 0.04 mgm. platinum.

40 gm. borax used for assaying quartz gave 0.04 mgm. platinum.

40 gm. borax used with sea-sand in place of rock gave 0.04 mgm. platinum.

200 gm. borax used with sea-sand in place of rock gave 0.26 mgm. platinum.

1,000 gm. borax used with sea-sand in place of rock gave 1.30 mgm. platinum and 0.12 mgm. osmiridium.

It therefore appears certain that the borax contains platinum to the amount of 1.3 parts per million, i.e., 0.00013%.

In casting about for the source of platinum it seemed possible that this impurity might have been introduced by fusing the borax in platinum vessels, assuming such to have been used in its manufacture. In order to test this theory 250 gm. of platinum-free borax was fused in a platinum basin at a red heat for four hours. The melt was allowed to cool, and when removed from the dish 200 gm. of it yielded 0.6 mgm. platinum. A second 250 gm. was treated in a similar manner, but in this case so as to obviate the possibility of any platinum having been mechanically removed by the hard fused borax, the latter was poured while still liquid into clean sand. 200 gm. of this yielded 0.46 mgm. platinum. A third quantity confirmed the result obtained in the second experiment.

It is thus shown that fused borax has a very distinct action on platinum basins.

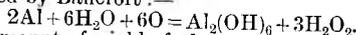
It may be interesting to note that up to the time that platinum was discovered in the borax ordinary assay borax had been employed. The comparatively large quantities of platinum were found in Merck's 'm.g.r. puriss. borax.' Even ordinary assay borax is not always entirely free from platinum, traces of the noble metal having recently been found to exist in a sample at present in use in the Government Laboratory.

Under ordinary circumstances the occurrence of such minute traces of platinum in an assay chemical would not be of more than academic interest, for it would probably not be seriously asserted that rocks containing the infinitesimal traces of the metal which, the use of this platiniferous borax would make them appear to contain could pay for working, and as a rule, therefore, time was not wasted in looking for such traces. In view, however, of recent allegations of the occurrence of platinum metals in South African rock formations the matter assumes a somewhat different complexion.

The assay balance used in the foregoing experiments turns easily with one-fiftieth mgm., and with care can be made to indicate one-hundredth mgm.—J. G. ROSE, Government Analyst, Capetown, Cape Chemical Society, Cape Town, July 17, 1908.—*Chemical News*, Aug. 28, 1908, p. 104. (E. H. C.)

ACETYLENE IN THE LABORATORY.—“For three years Messrs. Erdmann and Makowka have employed liquid acetylene, or the gas, for quantitative analysis of copper, silver, mercury, and precious metals, at the laboratory of the Higher Technical School of Berlin. The copper is easily and completely precipitated by the acetylene in cuprous salts solutions, either ammoniacal or acidified with acetic or tartaric acid, in the form of a granular precipitate, easily washed, of cuprous acetylide C_2H_2CuO .”—*Echo des Mines*, Oct. 8.—*London Mining Journal*, Oct. 17, 1908, p. 488. (A. R.)

A HYDROGEN PEROXIDE CELL.—“Two papers have been presented before the American Electro-Chemical Society by H. T. Barnes and G. W. Shearer in conjunction with H. M. Tory and G. H. Cole, in which the results of experiments on the effect of dissolved gases in water on metal surfaces have been given. In the second paper a cell was described which consisted of electrodes of aluminium and magnesium in a solution of aluminium sulphate, to which some hydrogen peroxide was added. Aluminium behaves in a peculiar manner in contact with dissolved air, or oxygen, and becomes electro-negative to a similar aluminium electrode in water free from dissolved air. Magnesium does not show this effect, hence a cell with the two metals for electrodes has a comparatively large E.M.F. developed when dissolved air or oxygen is introduced. The effect is considerably increased by adding hydrogen peroxide. Acting on the suggestion of Prof. Bancroft, tests were applied to the water in which clean aluminium had stood for a few hours, and a measurable quantity of hydrogen peroxide was detected. This was developed from the action of the dissolved oxygen on the aluminium, probably according to the following reaction, also suggested by Bancroft:—



The amount of yield of the peroxide was considerably increased in several ways. Without dissolved air no trace of peroxide was observed. The generation of the hydrogen peroxide explains the abnormal E.M.F. developed between two metals so close together in the electro-chemical series.”—*Engineering Review*, Sept., 1908. (J. A. W.)

ANALYSIS OF LEAD AND SILVER IN ORES.—“By a series of experiments, Dr. J. Lövy has demonstrated that the Belgian process for dry analysis of lead and silver in ores (melting in iron crucible) is much superior to the English or German

methods (melting in clay crucible with addition of metallic iron). The Belgian process gave, with 60 to 65 % sulphuretted lead ores, 98 % to 100 % of the real percentage, determined by the wet method, whereas the yield by the English method was only 92 to 95 %. The percentage of silver determined by direct cupellation of the regulus obtained in the iron crucible corresponded exactly, in the majority of cases, with that obtained by scorification, whereas that of the lead furnished by melting in a clay crucible was always too little. It is to be noted that the deficit in percentage of silver is almost exactly proportional with the loss of lead. It results from this observation, that this deficit is not due to volatilisation of silver, but to the deficiency in yield of lead. The Belgian process, giving 98 % to 100 % can then be employed for estimation of the silver by direct cupellation of the regulus. These data only refer to relatively poor ores (galena, etc.) With those containing a high percentage of zinc the Belgian process also gives a deficiency in results, the greater according to the higher percentage of zinc.—*Revue des Mines.—London Mining Journal*, Aug. 1, 1908, p. 150. (A. R.)

SILVER IN GOLD BULLION.—"The usual method adopted for the determination of Ag in gold bullion is that wherein the difference between 1,000 parts and the sum of the gold and 'base' contents is assumed to represent the Ag present in the alloy. The gold is directly estimated, and the 'base' determined by loss on cupellation with sufficient lead foil and using a check piece of approximately similar composition. The loss in cupellation will vary according to the temperature and other conditions during the operation, and, necessarily, considerable variations in the results, by different operators, may be looked for. Portions of a well-mixed sample of bullion sawings were submitted for Ag valuation to six independent assayers of repute, and the results advised were:—(1) 105.0 mm. Ag, (2) 99.0 mm. Ag, (3) 104.9 mm. Ag, (4), 97.7 mm. Ag, (5) 102.0 mm. Ag, (6) 104.9 mm. Ag. Two of the assayers, realising the crudity of the method of determination, returned other sets of valuations showing the Ag contents as varying with the temperature employed during the removal by cupellation of the 'base,' an experience similar to our own. In their judgment their particular value in the above table was correct as having been obtained by cupellation at the correct temperature. The other assayers, whilst not submitting the opinion, yet doubtless considered their results as correct by virtue of having performed them at a temperature which experience showed them to yield accurate results, or perhaps results agreeing closely with the results advised upon realisation of current bullion in London or elsewhere. The great variation between the highest and lowest results impresses the difficulty of maintaining a correct temperature for cupellation, and this is to be obtained only by the utilisation of gas-fired muffles with pyrometer installations. Even these are of no utility except there has been predetermined, by actual tests with prepared alloys of known composition, the particular temperatures and conditions for cupellation of bullions of particular compositions. As most mine assay offices are equipped only with coal-fired muffles some difficulty would be experienced in the realisation of that control of temperature necessary to ensure accurate results. Confronted with these drawbacks, Mr. H. B. Wright, chemist of the Great Boulder Perseverance Co., has devoted some time to the problem of working out a wet

method of assay likely to yield more accurate results than does the fire method. Finally, the method involving the fusion of the bullion with metallic cadmium, solution of the fused alloy in HNO_3 , and titration of the resultant solution with AmCyS has yielded satisfactory results. The triplicate assays of a sample agree in all cases very closely, and as a result of assaying check pieces, alloys of known composition, similar to the material being operated on, the retention of Ag in the undissolved gold is found to amount to about 1 to 2 parts per 1,000. Such amounts of copper as are present in a bullion of composition—Au = 800 mm., Ag = 100 mm, base = 100 mm.—do not interfere with the delicacy of the end-reaction in titration, nor is the method subject to any disadvantages of manipulation, no losses in this respect being possible by virtue of the fact that solution and titration can be performed in the same flask. The method of analysis is as follows:—Standard solution AmCyS.—1.6 gm. of the pure salt dissolved in 1,000 c.c. distilled water. This is standardised against pure silver foil and 1 c.c. equals approximately 4.483 parts of Ag per 1,000. The check piece, containing an amount of silver approximately equal to the contents of the bullion being assayed, is fused with cadmium, and thereafter treated similarly to the actual assays. The approximate amount of silver in the bullion is determined by the usual fire method; 0.5 gm. of the bullion is fused with 1.5 gm. of cadmium under a cover of potassium cyanide in a porcelain crucible in the flame of a (benzoline) blow lamp. Enough cyanide must be present to cover the cadmium, otherwise the metal will oxidise. A period of five minutes suffices for complete fusion. Allow to cool, place in a stream of running water, when the cyanide will rapidly dissolve and leave the alloy ready for solution in HNO_3 . Transfer the bullion to a flask with 20 c.c. H_2O and add 10 c.c. HNO_3 , at a time up to 40 c.c., whilst boiling for one hour, dilute to 150 c.c., add 10 c.c. ferric alum indicator and titrate with the standard solution of AmCyS. An alternative plan, used when time permits, is to allow the buttons to digest in the cold overnight in the flasks with 10 c.c. HNO_3 , and then the after-boiling need not be so protracted. Having noted the results produced at different temperatures as judged by the eye, in the cupellation method, and compared the results with those obtained by this wet method, a series of Ag determinations on 14 samples were undertaken (1) by the ordinary cupellation method; (2) by the wet method. Following are the results in millimes:—

	Fire.	Wet.
1	127.6	129.8
2	139.3	142.2
3	131.7	134.3
4	146.7	149.0
5	129.4	132.7
6	69.4	70.4
7	58.1	57.9
8	134.3	135.6
9	130.9	132.2
10	138.9	141.8
11	156.9	157.5
12	132.4	135.2
13	141.8	144.0
14	154.1	155.4

As an item of interest it may be mentioned that metallic cadmium can be used as a substitute for silver in estimating the Au in the bullion.—E. H. TAYLOR, *Chamber of Mines Journal*, W.A.—*Australian Mining Standard*, Aug. 26, 1908, p. 235. (A. R.)

ESTIMATION OF GOLD AND PALLADIUM IN SOLUTION.—"A new method to analyse gold and palladium by electric conductivity, invented by Mr. J. Dovan, is briefly described as follows by the *Revue d'Electrochimie et d'Electrometallurgie*. The inventor ascertained, by comparative study of solutions made colloidal by CO, in the case of Au and Pd, that with gold the increase of conductivity is not proportional the percentage of gold in the solution, but that a characteristic formula can be obtained. In the case of palladium, on the contrary, there is a proportion between certain limits. The formula, giving in milligrammes the amount of gold in 100 c.c. of liquid, is $2 = 1.76x - 0.0237xy + 0.0138x^2 - 0.00111x^2y + 0.000739xy^2$, in which x is the increase of conductivity and y the initial conductivity of the solution. In the case of palladium, it suffices to multiply the increase of conductivity by the factor of proportion— 121×10^6 —to obtain the amount of Pd in 100 c.c. of solution."—*London Mining Journal*, Sept. 19, 1908, p. 361. (A. R.)

METALLURGY.

EXTRACTION AND USES OF MOLYBDENUM.—There are two methods in general use for the reduction of the ore:—The aluminothermic process yields a product free from carbon, but containing small quantities of silicon and from 1 to 2% of iron. Alloys with chromium and nickel are also made by this process. The electrical process is worked by heating the ore in a carbon tube, employing a current of 350 amperes at 60 volts, when a portion of the sulphur is evolved as sulphur dioxide. On increasing the current to 900 amperes at 50 volts, complete fusion is obtained, and the rest of the sulphur is expelled. The metal produced in this way contains about 7% of carbon, of which about 1% is graphitic. The whole of this carbon can be removed by heating the crude metal with molybdenic oxide.

Properties.—Metallic molybdenum is of a silver-white colour, and has a specific gravity of about 9. It does not scratch glass, can be readily filed, and is malleable. Moist air does not affect it, but when heated to from 500° to 600° C. in air it burns brilliantly.

The most important outlet for molybdenum, at the present time, is in the manufacture of molybdenum steel. The general effect of the addition of molybdenum to steel, up to 4%, may be said to be to increase the hardness, toughness, and elongation without the production of any deteriorating effect when the steel is heated or welded.

The use of this element in steel manufacture is largely in its experimental stage, and opinions differ as to the value of the alloy in comparison with tungsten steel. Molybdenum is stated to be about three times as powerful in its action as tungsten. Tool steels may contain from 2 to 4% molybdenum, and an alloy containing 3% is stated to be particularly suitable for the manufacture of armour plates. Molybdenum steel at high temperature becomes very hard, but when annealed is softer than tungsten steel. It can be tempered in water without showing fissures, and it is said not to break 'cold short' so easily as does tungsten steel. The molybdenum employed in steel works is usually in one of three forms; (a) a dark-blue metallic powder containing 95 to 99% of molybdenum; (b) ferro-molybdenum, of which typical specimens have the composition (1) molybdenum 87.5, iron 6.4, carbon 6.3%; and (2) molybdenum 75.8 with less than 2% of carbon, and the rest iron; (c) molybdenum-nickel, containing 75% molybdenum and 25% nickel. An alloy

with chromium is also made containing 50% of molybdenum and chromium.

The ammonium salt of molybdic acid is employed in chemical analysis as a reagent for the estimation of phosphoric acid. It is also stated to find a use as a fire-proofing material and as a disinfectant for the upholstering cloth used in railway coaches. Molybdenum salts give a fine blue colour to pottery glazes, and at one time were employed, to a small extent, in the preparation of pigments for textile fabrics. Experiments have also shown that it is possible to employ certain salts of the metal in conjunction with logwood to impart a deep yellow colour to leather.

Commercial Value of Molybdenum and its Ores.—To meet the present requirements of the market molybdenum ores should contain not less than 42% of the metal, and should be free from other metallic minerals. Copper, in more than minute traces, renders the ore practically valueless. The price obtainable, as is the case with most of the rarer minerals, is a matter of special negotiation, and information on the subject is, therefore, not readily secured, but it is stated that ores showing 90 to 95% of molybdenum sulphide (60 to 63.3% of molybdenum) fetch at present from 16s. to 19s. per unit, per cent. of molybdenum, per ton. The price of metallic molybdenum (98 to 99%) is said to range from 5s. to 6s. per lb. Ferro-molybdenum containing 50% of molybdenum is offered at about 3s. 9d. per lb."—*Bulletin of the Imperial Institute*.—*Iron and Coal Trades Review*, Aug. 28, 1908, p. 873; *Page's Weekly*, Sept. 18, 1908, p. 596. (A. R.)

STEEL RAILS.—"The steel rail question has of late attracted much attention in America, and in connection with it many ideas were ventilated at the recent meeting of the American Society for testing materials. Dr. Henry Fay reports the results of microscopical examination of broken rails and classifies the causes of fracture under four heads: (1) unsuitable chemical composition; (2) segregation; (3) unsuitable heat treatment; (4) rolling flaws. Under (1) we have carbon, which may be too high or too low; phosphorus, which will produce brittleness when in excess; sulphur, which, if present as iron sulphide, will make rolling difficult; slag, an indefinite term, embracing oxides or silicates or both. Sulphur, present as manganese sulphide, has generally been considered harmless, but the investigations show it to be extremely brittle and possibly a dangerous material when present in steel under certain conditions. A number of rails which had broken in the foot in crescent form, in nearly every case showed near the top surface of the fractured pieces a thin layer of apparently more brittle material extending in the direction of rolling. Each of these crescent breaks contained manganese sulphide in the form of long threads, and microscopic examination showed streaks of ferrite running parallel to the sulphide. Experiments in producing cracks showed that these usually tend to follow the manganese sulphide lines. In examining rolling flaws, only two fractures have been examined, and it would obviously be unfair to draw definite conclusions from them. However, it is significant that the crack developed in each case followed a streak in which manganese sulphide was embedded. The solidifying point of manganese sulphide is given as 1,162° C. As rail steel solidifies at about 1,450° C., and if rolling begins at any temperature about 1,162° C., the manganese sulphide will be liquid from the rolling temperature down to 1,162° C., and below this temperature the sulphide, being plastic, will be elongated in the direction of rolling. For this reason

it is stated that the steel high in sulphur should not be rolled at too high a temperature, as the sulphur is then drawn out into a form that will ultimately lead to trouble. Dr. Fay concludes with the following suggestions. Specifications should be so drawn as to limit the amount of sulphur in steel. At the present time some specifications do not even mention sulphur. The next step is to allow the metal to stand a longer time after the addition of ferro-manganese. The specific gravity of manganese sulphide is 3.966, and that of steel 6.82, therefore if given sufficient time the sulphide should rise to the surface and enter the slag. Usually the time interval between adding the ferro-manganese and pouring the ingots is very short. Dr. Fay concludes by stating that if it is not permissible to start with an initially low sulphur, or to allow a sufficient time interval for the removal of the manganese sulphide, resort must be had to electric refining of molten metal by means of basic slag."—*Engineering Review*, Sept., 1908. (J. A. W.)

THE FILTRATION OF SLIMES.—"A new filter, the invention of Mr. Burt, of the El Oro Company, is well worth the attention of mining men. Batteries of these filters are in operation in Mexico, and have proved very successful, the features of the machine being simplicity in construction and running, coupled with discharge of slime cakes without manual labour. Briefly, a unit of the plant consists of a steel cylinder, 20 ft. long by 8 ft. 6 in. diameter inside by $\frac{3}{4}$ in. plate, with a door at the bottom which is opened or closed by a toggle operated by the piston of a hydraulic cylinder; this door could be hand-worked if necessary. The filter leaves, of which there are eighteen in each cylinder, are pear-shaped, 40 in. long by 36 in. maximum width, the frames being made of two pieces of $\frac{1}{2}$ in. w.i. pipe, bent and connected at the lower end by a piece of iron rod, and at the upper end to a swivelling water-tight T joint. On the inside of the frame of each leaf $\frac{1}{4}$ in. holes are drilled, with 2 in. to 3 in. spacing, and a piece of cocoa matting is cut to fit the frame and covered by heavy canvas filter cloth sewn on to the $\frac{1}{2}$ in. piping.

The swivelling T joint, which allows the leaf to hang perpendicularly, is connected through the top of the shell to a 4 in. pipe, which serves to carry the solution to its storage tank. Each leaf has a pet cock and valve to allow of the quality of its filtrate being tested from time to time; and, if necessary, any leaf can be put out of action until repaired.

To operate the filter the bottom door is shut, slime under a head of 60 lb. per sq. in. admitted, and the filtrate forced through the leaves and 4 in. header into a storage tank, whence it goes to the zinc boxes. After nine minutes the slime is shut off and the excess slime forced, by means of compressed air admitted at the top of the cylinder, through a pipe at the bottom of the cylinder to the excess slimes tank, the air simultaneously assisting in drying the cakes of slime on the leaves and forcing the filtrate through to the solution tank.

This operation requires eight minutes, after which the door at the bottom of the cylinder is opened and the compressed air switched on to the inside of the filter leaves, which knocks off the slime cakes, so that they fall out of the cylinder into the launder fixed to catch them. A jet of wash water admitted at the upper end of the cylinder clears out the last of the slime cakes and completes the cycle of operations, which totals twenty minutes.

Before admitting the next charge the excess slimes tank is emptied, by an electrically driven centrifugal pump, into the filter.

At El Oro the slime cakes formed vary from 2½ in. to 3 in. thick, and contain about 30% of moisture; whilst the slime coming from the settlers carries 80% moisture.

Each filter has an effective area of 234 sq. ft., and will treat from 120 to 130 tons of dry slime (El Oro) per twenty-four hours. Owing to insufficient filter-capacity, the El Oro cakes are not washed with water; such washing would increase the cycle of operations to forty-five or fifty minutes, but the extra capital cost involved would, in most cases, be amply repaid by the additional extraction.

In conclusion, I would add that Mr. Burt has patented the idea, the merits of which should be thoroughly investigated by those about to instal plant; as the first cost is low, much of the tedious process of settling and decantation in tanks is obviated, and, given sufficient fall, but little power is required to operate the plant."—H. G. PAYNE.—*London Mining Journal*, Sept. 26, 1908, p. 390. (A.R.)

SAMPLING OF SILVER-COBALT ORES.—"There are few ores that present greater difficulty in sampling than the silver-cobalt ores of Cobalt. The ore consists generally of cobalt and nickel arsenides and sulphides, but the trouble is caused by the occurrence of large amounts of metallics composed of native silver, or an alloy of silver and arsenic, which acts in the mill the same as native silver.

The ore leaves the mine in heavy jute sacks containing about 100 lb. each, and is shipped to Copper Cliff, Ont., in railway box cars under seal. In the case of very low grade material no bags are used, and the ore is shipped in bulk.

From the car it is trucked to the weighing scale, where it is weighed in lots of 10 sacks, and the first gross weight obtained. The sacks are then opened and the ore passed through a large Buchanan jaw crusher. The empty sacks are tied up, weighed, and returned to the shipper. If the ore is dry it is shovelled directly into the ball mill. If it is wet it is spread on steam drying plates until it is dry, and then it, too, goes to the ball mill.

As the ore comes from the jaw crusher a small shovelful from each sackful is set aside for a preliminary moisture sample, representing moisture contained in the ore as shipped. This moisture sample is coned and quartered to about 100 lb., after which it is taken to the sampling room, where it is passed through a small Allis-Chalmers laboratory jaw crusher. Then it is cut down to four samples of 5 kg. (11 lb.) each, which are placed in pans in a steam oven, for about 20 hours, at a temperature of about 80° C. This material eventually returns to the crushing floor and goes through the ball mill.

The ball mill is of Allis-Chalmers make and requires 25 h.p. It consists of a large metallic cylinder which revolves horizontally, and is lined with three sets of screens, the finest which is 20 mesh, being farthest from the center. The grinding is done by a large number of hardened steel balls, of a total weight of 1½ tons, which are carried up the side of the cylinder as it revolves, and then drop back on the ore. As the ore is ground to 20 mesh, it is discharged below to an automatic sampler. Screen tests show that 50% of the milled ore will pass a 100 mesh sieve, and 80% 50 mesh. The capacity of the mill is about 1½ tons per hour.

The large metallics remain in the ball mill, and after the run is complete, they are removed, weighed, melted in a furnace and run into bars of bullion.

The speiss and the slag from this are combined and sampled together, while the bullion is sampled separately.

The automatic sampler, which is a 27-in. Snyder, cuts out one-tenth of the milled product. It consists merely of a circular casting shaped much like a miner's gold pan, having four openings in its sloping flange, and revolving on the end of a horizontal shaft. Two opposite openings are closed, thus leaving two cuts per revolution. The material to be sampled is directed by a spout so as to fall inside of the sloping flange of the sampler. The rejections slide off the flange and the sample drops through the openings as they pass under the spout. The sampler makes 25 revolutions per minute, and thus gives 3,000 cuts per hour for about $1\frac{1}{2}$ tons of ore, or one cut for every pound of ore, or 60,000 cuts per car of 30 tons. A chain drive prevents slipping so that the cuts are regular.

The main part of the milled product (about nine-tenths of the whole), is here weighed and then passes to the storage bins of the smelter.

The sample is now removed from the sample chamber and weighed, and this weight is added to that of the milled product above. Payment is made on these combined weights, less the moisture.

Two complete weighings of the shipment are thus made which should agree closely. This gives the shipper a check on his weights. Thus the gross weight of ore in sacks should be the same as the weight of (a) milled ore including sample; (b) sacks, and (c) water lost on drying plates.

A sample for the final determination of moisture is taken by tube sampler from each pailful as it is removed from the sample chamber. This moisture sample is cut down to three samples of 3 kg. (6.6 lb.) each. The result thus obtained is used in the calculation of dry weight. The weight of water lost on the drying plates can be calculated by taking the difference between this and the first moisture result.

The main sample is now thrown on the concrete floor of the sample room, and after being shovelled over twice, is coned and quartered into two halves called sample No. 1 and sample No. 2. These samples are treated alike so that a description of one will suffice for both.

Sample No. 1 is coned and quartered by shovelling on the concrete floor down to about 100 lb., which will be four or five cuts according to the size of the original sample. Cutting down is continued by halving in a Jones sampler till two samples of approximately 20 lb. each are obtained. One of these is placed in a box and sealed by the shipper's agent for future reference, in case any accident should happen to the other samples. The other sample is now dried thoroughly and ground in a Sturtevant laboratory disc grinder till the fines pass through a 100 mesh sieve leaving the metallic scales on the sieve bright and clean. Part of the final grinding is sometimes assisted by a laboratory pebble mill of the Abbe Engineering Co., and sometimes by a Hance drug mill manufactured by Hance Bros. & White.

The metallic scales and fines are weighed and sampled separately. The fines are placed in a pebble mill and mixed for an hour before sampling.

Sample No. 2 is handled as above excepting that no reference sample is retained.

The methods of sampling as described above are according to exceedingly good practice, and the final samples should be about as close to the truth as it is possible to get them.

It requires three days to complete the sampling of a 30-ton car."—ARTHUR A. COLE.—*Mining World*, Aug. 1, 1908, p. 164. (J. Y.)

THE PREVENTION OF RUST.—“A paper on the prevention of rust was recently read by Mr. Thornton-Murray before the Birmingham University Metallurgical Society. The author first dealt with the theories of rust. It was once considered to be due to chemical action, carbon dioxide and water being necessary for its occurrence, and the compound formed having the formula $2Fe_2O_3 \cdot 3H_2O$. The supposition has to some extent been modified by an electrolytic theory due to Cushman, who considers that the action is mainly promoted by irregularities in the metallic surface, though the presence of carbon dioxide and water greatly facilitates matters. Turning to the prevention of rust, Mr. Thornton-Murray briefly described the various methods employed for this purpose, including tinning and galvanising, and showed that of the different varieties of iron, some resist rusting action to a greater extent than others. In conclusion, he mentioned a method, due to Mr. T. Corlett, of Birmingham, which consists in immersing the article in a hot solution of some phosphate and an iron compound. This covers the metal surface with a mixture of ferrous and ferric phosphates, and presents a pleasing dull black appearance. The process, it is said, makes the iron highly resistant to corrosion, and is adapted for light engineering work, such as cycle frames, gun barrels, stampings, and press work.”—*Indian Engineering*, Aug. 15, 1908, p. 109. (A. R.)

NICKEL.—“With the exception of the nickel contained in the ores shipped from the Cobalt district, the production of nickel in Canada is derived entirely from the well-known nickel-copper deposits of the Sudbury district. The output has been increasing steadily for a number of years, although the actual amount of nickel contained in matte shipped in 1907 is somewhat less than in 1906. Two companies are carrying on active operations: The Mond Nickel Co., at Victoria Mines, and the Canadian Copper Co., at Copper Cliff. The ore is first roasted and then smelted to a Bessemer matte containing from 77 to 80% of the combined metals, copper and nickel, which is shipped to the United States and Great Britain for refining.”—JOHN MCLEISH.—*Sessional Paper, No. 26a, Department of Mines, Canada, Summary Report of the Mines Branch, 1907-8, 1908, p. 73.* (C. B. K.)

MINING.

PREVENTION OF COAL-DUST EXPLOSIONS.—“Experiments have been made by Mr. Dautriche, engineer of the Poudres and Salpêtres, on the means of suppressing flames at the mouths of cannon. This he succeeded in doing by producing a cloud composed of potassium or sodium salt. This cloud opposes combustion of gases and inflammable dust. This discovery has a very wide range. Its application in fiery mines and industries where the dust is liable to cause serious explosions will make it possible to greatly decrease the number of accidents. It appears that the action of sodium carbonate is the same as that of this potassium or sodium cloud. It must suppress or greatly abate the flame usually produced by explosions, and, consequently, permit of the use of much heavier charges of explosives without danger of conflagration.”—*Revue Industrielle*, Sept. 26.—*London Mining Journal*, Oct. 17, 1908, p. 488. (A. R.)

COAL DUST AS A FACTOR IN MINE EXPLOSIONS.—“At the June meeting of the Coal Mining Institute of America a valuable paper on the subject of mine dust as a factor in coal mine explosions was read by Dr. Henry H. Payne. Dr. Payne is in a position to speak authoritatively on the phenomena of coal mine explosions, and his paper is made doubly valuable by the fact that it presents a concise review of the more important publications on the subject and a summary of the conclusions reached in the extensive researches on the conditions determining the elements entering into and controlling the inflammability of various dusts, which have been made in England, France and Germany. In conclusion he sums up what his own experience and his wide reading lead him to believe are the established facts and the most creditable theories.

Granting that secondary explosions may be propagated indefinitely by the larger sizes, only coal dust which will pass through a No. 100 screen is capable of initial or primary explosion.

Such dust, after having been subjected to ordinary atmospheric air for only a few hours, becomes largely decomposed and exists as a bubble of constituent gases whose film is composed of undisintegrated carbon and impurities, which, after combustion, are known as ash.

Such dust, when suspended in a homogeneous cloud with a moderate air velocity, is susceptible of ignition either through shock, compression, or sufficient heat to inaugurate combustion.

Such results will be classified as combustion or explosion, depending upon the volume of dust ignited, its supply of oxygen and the space within which combustion takes place.

Where gas alone is ignited, and the mine is free from dust, a ‘high explosive’ is obtained, and the explosion may be strictly local, due to the cooling effects of the walls,

The chief product of a dust explosion is carbon monoxide, whose expansion under combustion is greater than that of methane, and which receives its continuous supply of oxygen by feeding toward the intake.

A dust explosion, while assisted by the intake air, must nevertheless, follow those entries or airways furnishing the most material upon which to feed; and when this course follows the return airways, the conversion of the carbon-monoxide to carbon-dioxide renders the air extinctive, and prohibits further propagation. Evidence of such a condition will be found in the coke splashing or crusts, formed by the deposit of red-hot cinders carried by the air waves, and testifying to incomplete combustion.

The initial explosion may, and generally does, distil so large an amount of gas that complete combustion is impossible at the site of distillation, and this mass of gas and dust, in varying stages of ignition and combustion at a temperature greatly in excess of the point of ignition, were sufficient oxygen present, will develop into local explosions at irregular intervals wherever adequate atmospheric oxygen is available, such as at junctions of airways, widened passages for side tracks, or cavities where falls of roof rock have occurred, and are frequently called ‘flame areas.’ When such an explosion, either primary or secondary, travels toward a dead-end of an entry passage, the compression generated by its expansion and momentum causes an almost incredible rise in temperature, sufficient to distil the various hydrocarbons from even the ribs of the coal itself, and supplementing it with a heat potential far in excess of its losses through radiation and expansion.

The liability of any coal dust to explosion increases almost directly with its percentage of volatile matter that is combustible, *i.e.*, the quotient of its percentage of volatile matter divided by the sum of the percentages of volatile matter and fixed carbon.

While coal dust alone, under the conditions enumerated, is distinctively explosive, the presence of even the smallest amount of methane augments materially the susceptibility to ignition.

On account of the great elasticity of air, it is highly probable that no proper conception has yet been attained of the almost incredible speed with which a dust explosion, through its gaseous products, may be extended to far distant portions of a mine, under the force of initial expansion, properly called the ‘percussive theory.’

Changes in barometric pressure only affect the liability to explosion in so far as they allow, when the barometer is falling, a settling of any possible accumulation of methane from a dome in the roof-rock into the ventilating current; but such small quantities of gas are infinitely safer when diffused in the current of air than when concentrated in one place. Moreover, even if we grant that a low barometer allows greater occlusion, it also allows easier ventilation for the diffusion of such occlusion.

A mine may be overventilated until the air-current has such a velocity that it stirs up dust and would feed any slight ignition which might take place and otherwise die out.

The difference in the amount of real dust made by either air-punchers or electric chain machines is so slight, and so variable, depending on the nature of the coal and the skill of the machine man, that it cannot be said that either machine, as a class, creates more dust, than the other.

The results of experiments with electric ignition of dust show that the danger from electric wiring is no greater than that of stirring up a cloud of dust from a broken air-pipe or a loose connection.

Coal dust cannot be made wet, in the usual sense. The method of fine spraying is indicative of the best results; but, even then, it is hypothetical if the most careful system of watering is not merely an infinitesimal portion of the ‘ounce of prevention,’ and it is an open question whether it is not positively detrimental.”—H. H. PAYNE.—*Engineering Magazine*, Aug., 1908, p. 773.

EXPERIMENT IN MINE VENTILATION.—“It is interesting to quote the following from the annual report of the Western Australian Mines Department:—‘Inspector Cullingworth has forwarded the following description of a rather interesting experiment recently tried in mine ventilation in the Boulder Deep Levels mine:—The 1,100 ft. level of this mine is badly ventilated, there being no winzes down below 500 ft. The conditions are further aggravated by the fact of the shaft not being centred for the last 30 or 40 ft., and being a wet shaft, and the water kept down by baling, there is a continuous shower of water which spreads all over the uncentred portion of the shaft, and further serves to deaden the air. The experiment consisted of affixing to the compressed air column on the surface a locomotive vacuum air brake, and connecting with a steam pipe from the boiler. The compressor was stopped, and the hose taken off the air pipe in the level below. Steam was then turned on, passed through the air brake, and exhausted into the air. We then descended to the level, taking a thermometer. On the plat when we landed the temperature was 82° F. At the face, some 600 ft. from the plat, there was a good current

of air rushing up the air pipe, sufficient to put out the light of a candle at 2 ft. In about 10 minutes' time the temperature all along the level to the plat had been reduced to 74° F. So far as I am aware, this is the first time a Westinghouse vacuum air brake has been tried for this purpose. The experiment was merely preliminary, and the question of costs cannot yet be arrived at. It seems as though it would be well worth continuing these experiments in a smoky face after firing, where at present, even with a jet of air on, it takes some hours to clear the smoke."—*Australian Mining Standard*, Sept. 16, 1908, p. 310. (A. R.)

FUSE TEST.—From time to time in connection with accidents on the goldfields, it has been alleged that samples of fuse have "run through," causing premature explosion. In the course of 13 years' inspection of the fuse of this State, involving the examination of many thousands of samples, I have never encountered one which "ran." It occurred to me, however, that it might be possible that "short circuiting" occurred, due to the method of handling the fuse in our mines.

When a miner fires a "round" of holes, it is customary for him to tightly double up the length of fuse protruding from the holes and force it, in a crushed condition, into the mouth of the bore hole, this being done to prevent the fuse being cut by flying stones, etc., from the earlier holes of the "round." In order to test this, galvanised iron tubes were employed of 1½ in. internal diameter (the size of the ordinary machine hole on our mines), and about 12 in. long, and lengths of fuse 6 ft. long (about the average length of the miners' "stick") were coiled or bent and tightly crushed into these tubes; they were then burned along side of lengths of the same fuse laid out on the ground in the ordinary way.

The results were very striking. Out of several hundred tests, no increase of speed was manifested by the samples in the tubes, and in fact, as a general rule, by this treatment the rate of burning was retarded, in some cases to a considerable extent: Of the total number of 287 fuses tested in tubes 107 showed a retardation as compared with fuses burnt in the open, while only 200 showed an acceleration of speed, and this very slight.

While this method of treatment, therefore, has no effect in accelerating the burning rate of the fuse, and the tests still further emphasise the improbability of any of our safety fuse "running through" during use, it is of some importance to note that a certain retardation of explosion may be caused by this doubling of the fuse, and give an additional reason for discouraging the tendency, which often exists, for men to return too quickly to holes which they suspect have missed fire.—E. A. MANN.—*Report of the Department of Mines of Western Australia for the year 1907*, p. 221. (A. R.)

MISCELLANEOUS.

A PAINT WITH A NEW USE.—"It is stated that a paint is about to be placed upon the market which will serve as an indicator of excessive heat in machine parts. When the parts painted are cool, the paint is red in colour; but when they become hot, the colour becomes black, and returns to red as the part cools again. This paint is said to consist of mercuric iodide and cupric oxide, mixed in varying quantities, according to the amount of heat considered permissible."—*Indian Engineering*, Aug. 22, 1908, p. 123. (A. R.)

ANKYLOSTOMIASIS IN SOUTH AFRICA.*—"The disease ankylostomiasis, commonly spoken of as miners' anaemia, is produced in South Africa by the presence in the small intestine of one, sometimes two, species of nematode worms, namely:—

- (1) *Ankylostomum duodenale*.
- (2) *Necator Americanus*.

It was Dr. Mathias, of Kimberley, who, in 1896, first drew attention to the fact that the peculiar symptoms of anaemia, etc., from which so many of the De Beers European miners were suffering, were due to the presence of the ankylostoma parasites. To Dr. Mathias all credit is due for first sounding a warning to South Africa of the presence of this dangerous disease among a section of its community.

In 1905, while I was examining gangs of native labourers, freshly recruited from the mines, my attention was attracted by the pigmented patches, such as I read Delamere considered pathognomonic of ankylostomiasis, on the tongues of some of them, though I know from subsequent observations that this pigmented condition is common amongst most Bantu races, irrespective of the presence of the ankylostoma parasites. It was this which first led me to make a systematic investigation in the matter.

A microscopic examination of faeces, collected in the native hospital, from patients of various tribes, at once proved that the disease was prevalent among them. I could give the results of some hundreds of examinations of faeces, but I think it unnecessary, as in the hospital I naturally selected the excreta of patients who were anaemic and showed other signs of the disease, consequently the number of positive results obtained from this source was large, and is liable to give an exaggerated idea of the proportion of natives infected.

Geographical Distribution.—If we consider first the natives of the East Coast, we see that the Nyassa, Mozambique and Quilimane boys, that is to say, those who occupy the territory between the southern boundary of German East Africa and the Zambesi, are grossly infected (61.9%).

A little further south, natives from Beira, coming from the old Gazaland, on the banks of the Pungwe River, are shown by the limited figures at hand to be infected to some extent.

Of the inhabitants living in the interior of the country, along this length of coast line, the table shows records of natives in British Central Africa, Nyassaland and on the eastern borders of Lake Nyassa, also natives which I have classed under the heading of Angonis, as most of them belong to a Zulu tribe of that name, and who come from the Zambesia district, about Lat. 16° S. to the S.W. of the southern portion of Lake Nyassa.

A percentage of the inhabitants of all these inland districts harbour the parasite.

South of Lat. 22° S. we find about 35% of Myambani, Mtyopi, Shangaan, and Tonga boys are infected, consequently the worm is found in the coast countries between Lat. 22° S. and Delagoa Bay.

Of the inhabitants of Zululand I cannot speak from personal experience, but Dr. Hill, Medical Officer of Health for Natal, in his report for 1906, states that Dr. L. G. Haydon demonstrated the ova of the worm in 3.9 per thousand of native prisoners removed to Durban after the last Zulu rebellion.

We must therefore assume that both Zululand and Natal are infected to some extent.

* See "Ankylostomiasis: A Warning," by W. C. C. Pakes, This Society's Proceedings, vol. iv., pp. 389-404 (Jan., 1904).

I hesitate to give an opinion as to the presence of the disease or otherwise in Damaraland. The table shows that two Maherero harboured the parasites, but possibly they were infected in the Transvaal, as they had been working in the mines for some time.

As regards the rest of the country, I believe that the high veld and more temperate parts are free from the disease; that is to say, the Cape Colony (excepting De Beers' mine at Kimberley), Orange River Colony, Basutoland, Bechuanaland, and most of the Transvaal. I make this statement because I have not come across the disease among many thousands of natives passing through my hands from these parts.

The next question to be considered is: 'What effect has the importation of this enormous body of infected labourers on the underground workers in the mines of the Transvaal?'

Knowing the manner in which the disease spread in Kimberley and some of the mines in England and Europe, one would be justified in anticipating disastrous results.

I think, however, that those responsible for the mines are to be congratulated on the fact that, excepting the underground hands on one (possibly two) properties, the European miners have not become infected, nor as a general rule have the native labourers been re-infected.

In view of the previous statements showing the amount of infected material which must for years have been poured into the mines, the assertion that the European miner does not contract the disease is a daring one to express, and requires substantiation before it can be accepted. I therefore submit the following facts for your consideration:—

(1) I have made careful inquiries from medical men practising on the mines as to the occurrence of the disease among Europeans employed underground. With the exception of men on one mine, I have only obtained accounts of two Europeans being infected. One of these was a recent arrival. I am not certain that the other had not been working in the infected mine already referred to.

The medical men along the reef are thoroughly alive to the possibility of the existence of the disease, and specimens of the faeces of miners suffering from symptoms in any way resembling those of ankylostomiasis are frequently submitted for microscopical examination, and negative results obtained.

If the disease were spreading to any extent among the Europeans, it would be recognised, as it was in Kimberley and on the one infected mine here.

(2) I think a more convincing argument against the infection occurring extensively is that whereas the percentage of infected natives who have never been below ground is 55.48, that of boys who had worked on the mines is 32.35, and most of these have certainly worked barefooted in the mines for months; because, if infection were occurring to any extent below ground, there would certainly be no reduction in the numbers found to be harbouring the parasite. On the contrary, after a period of exposure during their work in the mine, one would expect a marked increase.

(3) While making post-mortem examinations I have been astonished at the small number of parasites found in each intestine.

(4) In the course of my duties I see hundreds of sick natives of all tribes who have worked on the mines, yet I cannot call to mind cases of ankylostomiasis occurring among any but tropical boys. If the mines were seriously infected I should see cases among the M'Xosa and other Cape Colony natives, as well as among those from more northern parts.

These facts have satisfied me that at present the miners on the Rand, European and coloured, are not being infected with the disease, or at any rate, if they are, it is only to a very trivial extent. Whether this satisfactory state of affairs will continue in fresh mines remains to be seen.

We have now to consider, under the circumstances, why everyone employed underground is not infected.

The filthy habits of many of the natives exclude all idea that the sanitation is so perfect that it prevents the disease spreading. Other factors must therefore be looked for. I believe there are several in operation. To the best of my knowledge they are:

- (1) The acid reaction in many of the mine waters.
- (2) Iron salts contained in some of the mines.
- (3) Low temperature of some workings.
- (4) Dryness of some mines.
- (5) Nature of soil in the mines.

These factors must be considered separately, though more than one of them may exist in some mines or in one portion at the same time.

(1) *Acid Reaction of Mine Waters.*—Many of the mine waters are extremely acid. One sample of a series I had taken showed an acidity equal to 0.79% sulphuric acid. The degree of acidity varies in different mines and in different parts of the same mine, depending as it does on the oxidation of the pyrites and on the amount of water passing over them. Therefore, until the reef is exposed, very little acid is formed, consequently it takes a much longer time for the water of a deep level mine to acquire an acid reaction than for that of an outcrop. The following analyses demonstrate this point:—

<i>Mine A.</i>		Vertical Depth.	Degree
		Feet.	of Acidity
Samples taken on 6th level	...	786	1.55
" " 8th level	...	1,049	28.20
" " 10th level	...	1,348	26.76
<i>Mine B.</i>			
Samples taken on 6th level	...	820	2.70
" " 8th level	...	1,086	19.80
" " 11th level	...	1,618	43.30
<i>Mine C.</i>		Degree of Alkalinity	
Samples taken from vertical sump	...	0.5	
" " east incline	...	0.27	
" " east shaft tank	...	0.18	

Mines A and B were outcrop mines, with markedly acid water. C was the deep level mine to A and B. Its water was alkaline throughout, which may be accounted for by the fact that there was a large amount of water coming down the shaft through alkaline strata. The amount of reef exposed in the mine was small, consequently there was insufficient acid formed to neutralise, much less acidify, the bulk of the mine water. One may expect, however, that, as the mine is worked, and as more and more reef is exposed, the alkaline water will be neutralised and finally given an acid reaction.

This will, I trust, give some of the reasons accounting for the degree of acidity varying in different places.

To return to the effect of the acid reaction on the spread of ankylostomiasis. Dr. Boycott, when reporting on the Levant and other mines, states that the sodium chloride in them prevented the spread of the disease. He found that a 2.5% salt solution quickly kills ankylostoma larvae. If a saline solution is useful in this manner, one would expect an acid one to have an equally beneficial effect. Dr. May, the Government Bacteriologist for the Transvaal, undertook experiments in this matter, which

prove that a 0.75% solution of acid stops the formation of ankylostoma larvae, and that probably a much lower degree of acidity will prevent the larvae when formed from penetrating the skin.

It must therefore be admitted that as some mine waters have a larger amount of acid than 0.75%, this acid reaction does at times safeguard the underground worker from infection.

(2) *Iron Salts*.—Dr. McCrae, the Government Analyst for the Transvaal, has suggested that possibly the iron salts (sulphates) may act in the same manner as the sodium chloride has done in some Cornish mines in preventing the spread of the disease, but experiments in this direction have not been made.

(3) *Temperature*.—It is stated that larvae do not develop at a temperature below 77° F. Some of the workings of the mines in the Transvaal do not reach that temperature.

This factor must be looked upon as a further safeguard.

(4) *Dryness*.—The larvae cannot develop without a certain degree of moisture. Some mines in the Transvaal are excessively dry, and so are protected in this way.

(5) Dr. Mathias has suggested that possibly the nature of the soil below ground may have an influence on the spread of the disease. He suggests that a foundation of finely-ground quartz, such as might be found in parts of gold mines, would not be such a suitable medium for the larvae of the parasite to live in as, for example, the moist blue ground of a diamond mine.

We see, therefore, in discussing the presence or absence of this disease on the Rand, one must consider each mine and each portion of each mine on its merits.

Probably other causes will yet be discovered to account for the non-infection of the miner of the Transvaal.

I mentioned one mine as being an exception. On this mine, every European employee who went below ground, from the manager downwards, contracted a cutaneous eruption, recognised as 'bunches,' and was found after a time to harbour the parasite, and had to undergo treatment.

The mine in question is a deep one; the temperature below ground was high; it contained a large quantity of water, samples of which gave a uniformly alkaline reaction when tested. The underground worker was therefore not protected by any of the four factors described above.

From all accounts it appears that the timbering in the mine was the most fruitful source of infection—miners sitting on damp wood contracted sores on their buttocks.

I have noticed that many pneumonia patients from tropical parts develop an irregular temperature on or about the fourth day after their crisis.

This irregularity is, I believe, often produced by the parasites, because I frequently find their ova in the excreta from such cases, and a course of thymol usually brings the temperature to normal and permits convalescence to proceed without interruption.

Diagnosis.—A microscopical examination of the faeces is the only manner among the South African natives that the diagnosis can be made with certainty. The blood count is useless as a means of differential diagnosis, as so many other parasitic diseases are prevalent in this country, which cause an increase in the number of eosinophiles. This is well demonstrated by the following table, which shows the various species of worms found at post-mortem examinations:—

Tribes	No. examined.	Bilharzia worms in liver.	Ankylostome	Round worms.	Tape worm.
Mozambique ...	215	102	139	17	1
Qnelimane ...	129	60	81	32	1
Nyassa ...	52	16	27	10	2
Brit. Cent. Africa ...	10	4	2	1	1
Beira boys ...	3	1	2	0	0
Angoni ...	14	2	3	1	0
Shangaan ...	65	23	32	27	2
Tonga ...	6	0	1	1	2
Myambaan ...	106	28	30	36	7
Mtyopi ...	40	9	15	8	8
Transvaal Basuto ...	12	1	1	0	1
Damara ...	19	0	2	1	0
Swazi ...	1	0	0	0	1
Pondo ...	6	0	0	1	3
M'Xosa ...	5	0	0	2	1
Mixed Races ...	9	0	0	0	0
Total ...	692	246	335	137	30

—G. A. TURNER.—*Transvaal Medical Journal*, Sept., 1908. (J. A. W.)

MINE TAILINGS ON THE RAND (planting grass, etc.).—"I examined some samples of the tailings taken from such heaps some time ago. I therefore append an abstract of a report which I wrote in 1905 on behalf of the Municipality of Johannesburg. Four samples were received, which were described as follows:—1. From fresh tailings taken from the skip on the way to the dump. 2. From tailings about six months old, taken from just under the surface of the dump. 3. From tailings about twelve months old. 4. From tailings which had been exposed to the weather for at least six years. All samples were from the Ferreira dump. In No. 1 no appreciable amount of cyanide could be detected (the examination was not made until the sample was a few weeks old), but the presence of sulphides in considerable quantities was clearly evident. I am inclined to think, therefore, that the sterility of the dump is dependent more upon the presence of sulphides than of cyanides. The composition of the tailings is shown by the following figures:—Moisture, 5.47; loss on ignition, 1.10; silica, 90.76; iron oxide and alumina, 2.35; lime, 0.91; magnesia, 0.21; potash, 0.05; phosphorus pentoxide, 0.03; total, 100.87. The mineral ingredients necessary for plant growth—potash, lime, magnesia, and phosphoric acid—are thus present in sufficient quantities for the requirements of plants, the first named being perhaps most deficient. But the presence of oxidisable sulphide of iron renders the material sterile even if the absence of nitrogenous matter did not do so. The sulphide of iron undergoes slow oxidation, being slowly converted by absorption of oxygen into sulphate of iron and free sulphuric acid, which at first is neutralised by the lime present. Thus at first (No. 1) the tailings are distinctly alkaline in reaction, due to the presence of free lime, after some time (No. 3) they become neutral, while after some years (No. 4) they develop a distinctly acid reaction. The total sulphur present diminishes as time elapses, owing to the formation of soluble sulphates and their removal by rain. Determinations of the total sulphur and soluble (in water) sulphur gave the following:—Total sulphur: Fresh, 1.21; six months,

1·21; twelve months, 1·10; six years, 0·24%. Soluble sulphur: Fresh, 0·012; six months, 0·010; twelve months, 0·011; six years, 0·029%. It would thus appear that during the first year the oxidation and removal of the sulphur was slow, but that afterwards it proceeded more rapidly, but that even after six years there still remained some unoxidised sulphur. Of course, these deductions are based on the assumption that the material when deposited was of similar composition during the whole period.

In order to render the tailings more capable of supporting plant life, it seems to me to be necessary—
1. To destroy the oxidisable sulphides—pyrites, etc. 2. To increase their porosity and friability. 3. To add the necessary nitrogenous organic matter. 4. To neutralise the acidity which develops upon the oxidation of the pyrites.

No. 1 could only be done on the large scale by the air, and for this to occur No. 2 would have to be effected. No. 2 could probably be accomplished by mixing the surface of the dump with granular 'clinker' from engine furnaces, rejecting the fine ashes and the very coarse clinker. This admixture would render the surface porous, and would favour oxidation by facilitating the access of oxygen. The oxidation would be favoured by periodically watering the surface. No. 3 would be best done by adding stable manure or similar material after Nos. 1 and 2 had been in operation for some time. If added too early, the organic matter being itself oxidisable would tend to prevent the oxidation of the pyrites. No. 4 could be best effected by the addition of powdered lime added simultaneously with the stable manure. As to quantities, it is difficult to advise; probably 10 or 12 tons of clinker per acre would suffice. The granular clinker should be spread on the surface and harrowed or raked in, and an occasional harrowing, say, once a month, would probably be helpful, especially after rain or watering.

The stable manure should be applied at the rate of 20 or 25 tons per acre, and powdered lime to the amount of, say, 2 tons per acre, well harrowed in, followed after a month or six weeks by the grass or other seeds.

I fear the above method would be expensive, but I cannot suggest any cheaper plan likely to be successful. Of course, another method, but I fear almost too laborious and expensive, would be to cover the dump with a layer of soil to a depth of 6 or 8 in., and plant the seeds in that.

Our agrostologist, Mr. Burt-Davy, suggests Bermuda grass, 'Oprecht kweek gras' (*Cynodon Dactylon*) as a suitable grass for the purpose of trial.

Some relief from the annoyance of dust blowing from the dump heaps might possibly be derived from the use of shelter belts of trees planted round the heaps, but, of course, a considerable time would be required for the trees to grow sufficiently high to be effective. Our conservator of forests, Mr. Legat, advises *Eucalyptus viminalis* as the most suitable tree, to be planted in belts at least a chain wide around the dumps, the trees to be planted 6 ft. apart, and those nearest the dump to be allowed to be highest, those furthest away lowest, so that the wind would be turned upwards by the belt. I am afraid, however, that the problem is a difficult one, and in any case would entail considerable expense and labour. I regret that I cannot suggest any simpler and less troublesome plan.—HERBERT INGLE. — *Transvaal Agricultural Journal*, July, 1908, p. 636. (G. H. S.)

IRON-ORE CEMENT IN SEA WATER. — "A pamphlet has been published by a German cement company covering a series of tests of the resisting and enduring qualities of iron-ore cement and Portland cement, from which the following conclusion was reached by the company:—

Through our experiments we arrive at the conclusion that the resistance of the Portland cement which we used for comparative tests depended upon two points, namely, the method of manufacture and the percentage of alumina. Those made under the wet process invariably showed greater constancy of value than those made under the dry process, supposing that the contents of alumina did not vary too much. The amount of alumina, without doubt, is the more decisive factor of the two. All experiments were convincing evidence of the fact that a cement disintegrates the sooner or suffers so much more serious damage within a given time the higher its contents of alumina. Portland cements, being low in alumina, show greater resistance in the presence of gypsum or in sea water than those of a high percentage of alumina, but, in the long run, even the former go to destruction. Iron-ore cement, as previously stated, passed the severest tests without the slightest damage—its contents of alumina never exceeding 1·5%.

All the foregoing statements warrant the declaration that well-burned iron-ore cement is to be used for marine construction, for sea-water canals, and for all cases in which the concrete comes in contact with sulphuric acid salt solutions.—United States Consular Report.—*Mines and Minerals*, Aug., 1908. (A. R.)

REINFORCED CONCRETE.—Its efficiency has been thoroughly demonstrated, and it is now recognised as a permanent method of construction; it is proof against the deteriorations to which other materials are subject. It is fire-proof, resists earthquakes, is rust-proof, practically everlasting, and its strength is continually increasing with age.

The constituents from which it is made can easily be obtained in almost any locality, *i.e.*, sand and crushed stone or ballast; only the steel and cement being brought any distance. In the early stages crushed stone concrete is a little stronger than ballast concrete, but at the end of 30 days ballast concrete is just as strong as stone concrete, and from then onwards its strength begins to surpass it.

It has recently been proved that a moderately fine sand will give a stronger concrete than a sharp coarse sand; sand taken from beds in contact with impure water kills the action of the cement and should be condemned. Pit or bank sand is a very satisfactory kind to use. Cement should be of the very best grade and be subjected to many mechanical tests during the progress of the work.

Concrete mixing is an important feature, only clean water free from acids and strong alkali should be used in mixing. The resultant should be that known as a wet mixture; no concrete which has once begun to set should be deposited.

Concreting should never be done in freezing weather; concrete should be reinforced in a vertical as well as a horizontal plane.

Reinforced concrete is the best form of construction when properly handled, and the worst when improperly handled.—M. KAHN.—*I. of Engineers and Ship Builders of Scotland*. (C. B. S.)

Reviews and New Books.

(We shall be pleased to review any Scientific or Technical Work sent to us for that purpose.)

CYANIDE PROCESS. By E. B. WILSON, E.M. Fourth Edition, revised and enlarged. Price, 6s. 6d. (New York: John Wiley & Sons. London: Chapman & Hall, Limited.)

"Cyaniding is now perhaps the best known process in gold milling, yet since the first edition of this work was published in 1895, although many theories have been advanced and proved or found worthless, there remains still much to learn. Cyaniding has widened in scope to such an extent that ores are now successfully treated which were once considered beyond the capabilities of the process.

The present edition is designed especially to describe the improvements that have taken place in the treatment of slime since the last edition of the work published in 1902. No unnecessary details are incorporated in the work, and by thus dealing almost solely with practical facts about the process, rather than with mechanical details, the author has been able to publish this little work in a terse and lucid form.

The following apparatuses are described: Butter's distributor with Blaisdell improvements, Sweetland press, Moore's stationary and vacuum filter, Blaisdell pressure filter, Kelly pressure filter, the Adair-Usher slime process (introduced in South Africa in 1907), and the Nicol's slime settling process. All these processes are clearly illustrated by diagrams, and the rate of capacity of the various appliances connected with each process distinctly stated, together with their respective advantages.

Considerable space is devoted to the application of electricity in cyaniding, both time, cyanide, and zinc being saved by its adoption, as well as other advantages gained through its employment. This branch of the subject is thoroughly dealt with in the various chapters on electrodes, current, anodes, and cathodes.

A chapter is devoted to the Siemens-Halske and Pelatan-Clerici processes, another to general information on cyaniding, and an appendix at the end of the book gives instructions how to proceed with a treatment of a case of cyanide poisoning in the absence of medical assistance. The work is carefully and concisely written, and is thoroughly indexed."—*London Mining Journal*, Oct. 10, 1908. (W. A. C.)

HINTS ON AMALGAMATION AND THE GENERAL CARE OF GOLD MILLS. By W. J. ADAMS. San Francisco, Cal., 1908; *Mining and Scientific Press*. Pp. 120, illus. For sale by *The Mining World*. Price, \$2.

"There are a number of books on gold mill practice, and while some have merit by reason of the experience of their authors, others fail to satisfy the popular demand, because they are written in the technical language which few practical millmen can understand. Mr. Adams, in revising the second edition of his excellent treatise, has taken pains to employ the language that can be understood by all, and wisely condensed his explanations of the important features of gold milling, so that the busy man need not lose time in gaining the knowledge which he desires. The title of the book has been well chosen, for the reason that 'hints' on amalgamation are wanted alike by the experienced and inexperienced millman."—*The Mining World*, Sept. 19, 1908, p. 449. (W. A. C.)

GENESIS OF METALLIC ORES AND OF THE ROCKS WHICH ENCLOSE THEM. By BRENTON SYMONS, M. Inst. C.E., C.M.E.; 154 illust. Price, 7s. 6d. With map, 8s. 6d. (London: *The Mining Journal*.)

"Of the geological formation and mining possibilities of many lands, Mr. Brenton Symons has written much that is valuable, lucid, and to the point. In the latest published of his works, 'The Genesis of Metallic Ores, and of the Rocks which enclose them,' he has endeavoured, and that with marked success, to bring the complex scientific problem of the genesis of ore deposits out only within the comprehension of the average man in the street, but to make it intensely interesting reading, and that much on the lines that inspired Camille Flammarion when he brought astronomy within the understanding of the average lay intellect. In his work, which is well printed, well written, and admirably illustrated, Mr. Symons, avoiding all scientific phraseology, which, however ordinary it may be to the student, is caviare to the multitude, leads the reader on step by step from the hypothesis of creation to the certitude of the created as exemplified in that portion of the earth's crust which he so lucidly classifies. The publication of the work should be particularly welcome in that it fills the hiatus between the primer, which is of little use outside the sixth standard, and the average text-book which assumes the student's knowledge to be on a par with that of the author."—*The Mining Journal*, Sept. 26, 1908, p. 406. (W. A. C.)

GOLD AND SILVER. Comprising an Economic History of Mining in the United States, the geographical and geological occurrence of the precious metals, with their mineralogical associations, history and description of methods of mining and extraction of values, and a detailed discussion of the production of Gold and Silver in the world and the United States. By WALTER R. CRANE, Ph.D. First Edition. Pp. 727. Price, 21s. (New York: John Wiley & Sons. London: Chapman & Hall, Limited.)

"Although the title on the cover of this work ('A Treatise on Gold and Silver') would indicate that it covers a world-wide range, it is practically, as shown in the preface, confined to the occurrence of the precious metals in the United States, and will ultimately form a part of the Economic History of the United States, to be published by the Carnegie Institution of Washington.

This compilation should prove a valuable work of reference, especially as the original sources are very fully given. About half of the work deals with the occurrence and geological distribution of the metals—over 100 pages with the mining, and over 200 pages with the treatment of the ore. There is also, in addition to the numerous valuable tables which appear with the letterpress, an appendix of tables dealing with the discoveries of districts, the occurrence and mineralogical association of gold and silver, and the outputs and values for individual districts and mines. A useful bibliography is also appended.

The work will, under any circumstances, be found of extreme value as a book for reference, and the sections dealing with the extraction of values, although, as already stated, often unduly brief, are ably dealt with, present the processes clearly and correctly, and are well up to date, and may be read with advantage before consulting other treatises which have been written on special branches of treatment."—*London Mining Journal*, Sept. 26, 1908, p. 405. (A. R.)

Selected Transvaal Patent Applications.

RELATING TO CHEMISTRY, METALLURGY AND MINING.

Compiled by C. H. M. KISCH, F.M. Chart. Inst. P. A.
(London), Johannesburg (Member).

(N.B.—In this list (P) means provisional specification, and (C) complete specification. The number given is that of the specification, the name that of the applicant, and the date that of filing.)

(P.) 418/08. Louis Schedl. Improvements in jockey or rope grips for mechanical haulage systems and the like. 19.10.08.

(C.) 424/08. Heinrich Flottmann. Improvements in rock drilling machines. 23.10.08.

(C.) 425/08. Patrick Joseph Donovan. Improvements in filters. 22.10.08.

(C.) 426/08. Charles Budd Richards. Fluid operated tools. 23.10.08.

(C.) 427/08. Louis William Greve (1), Charles Budd Richards (2). Pneumatic hammer and drill bit. 23.10.08.

(P.) 428/08. Alexander Cullen Whitehead. Adjustable tube scraper. 26.8.08.

(P.) 429/08. Robert Clifford de Lorme (1), Wilfred George de Lormé. Improvements in tipping trucks. 26.10.08.

(C.) 430/08. William McVitty. Improvements in driving means for rotary ore feeders. 26.10.08.

(P.) 431/08. Henry James Shedlock Heather. Improvements in lightning arresters and other apparatus for protecting electric circuits against the effects of abnormal rises in voltage. 27.10.08.

(P.) 442/08. Wilhelm Mauss. Improvements in self-feeding percussive apparatus. 4.11.08.

(P.) 443/08. Hugh Henry Boyd Stewart. Improvements in the construction of dams, weirs, quay walls, retaining walls and the like. 5.11.08.

(P.) 445/08. James West. Improvements in grips for cable haulages. 5.11.08.

(C.) 447/08. Sir James Heath (1), William Hales Turner (2). Improvements in or relating to the drilling of rock or the like and apparatus for use in connection therewith. 6.11.08.

(C.) 449/08. Henry Rainhard. Improvements relating to the manufacture of metal tubes, rods, and the like. 6/11/08.

(P.) 450/08. Alexander John Arbuckle (1), Alfred Osborne (2). Improvements in means for separating pulverised ore or other comminuted solid matter from liquid. 7.11.08.

(C.) 452/08. Fritz Wagner. Improvements relating to a method of and means for obviating the vibration of main pressure valves. 13.11.08.

(C.) 453/08. Mortimer Frederick Meiville. Improvements in apparatus for the production of carburetted air. 13.11.08.

(P.) 454/08. George Hans Kirsch (1), Walford Robert Dowling (2). Improvements relating to tube mills and similar grinding apparatus. 13.11.08.

(C.) 455/08. Henry Wolfgang Foust. Ore concentration jig. 13.11.08.

(C.) 456/08. James Palemer Hearn. An improved pendulum pump worked with an adjustable weight. 18.11.08.

(P.) 457/08. Hermann Stadler. Ventilating structural sheet. 18.11.08.

(C.) 459/08. Sidney Smith. Improvements in carburetting apparatus. 20.11.08.

(P.) 460/08. Tullie Bubola. Improvements in or relating to tanks or vats for the treatment of crushed ore products. 21.11.08.

(C.) 461/08. Harry Murson Thomas (1), John Wylie Craig Niven (2), John Greig (3), Robert Niven (4). Improvements in furnace for heating hand rock drills, machine rock drill bits and similar edge-tools preparatory to the sharpening of same. 21.11.08.

(C.) 462/08. Robert Findlay Sturrock. Improvements in furnaces for boilers. 23.11.08.

(P.) 463/08. Richard Colson. An improvement in dropper and posts and similar articles used for fencing and similar purposes. 23.11.08.

(P.) 464/08. Thomas Passe. Improvements in means for imparting the intermittent rotary motion to the pistons of rock drilling machines. 23.11.08.

(P.) 465/08. Charlton Frederic Bayly. Device for regulating the underflow of cones, spitzkasten and the like. 24.11.08.

(C.) 468/08. Hubert Elwell Smith. Improvements in and relating to carburetters. 27.11.08.

(C.) 469/08. Richard Scherl (1), Emil Falcke (2), Paul Froehlich (3), Alfred Kuerth (4). Improvements in or relating to gyroscopic mechanism. 27.11.08.

(C.) 470/08. Heinrich Flottmann. Improvements in packing for rock drills. 27.11.08.

(C.) 471/08. Heinrich Flottmann. Improvements in rock drills and other pneumatically operated tools. 27.11.08.

Changes of Addresses.

Member and Associates are requested to notify the Secretary immediately of any change in address, otherwise it is impossible to guarantee the delivery of Journals or Notices. The Secretary should be at once notified of non-receipt of Journals and Notices.

BAWDEN, F. A., *l/o* Germiston; Ingleside, Redrnth, Cornwall.

BELL, H. C. F., *l/o* Cleveland; Cala; Tembuland, C.C.

BELL, S. W., *l/o* Rhodesia; Tankerville House, Newcastle-on-Tyne, England.

BRICKHILL, H. G., *l/o* Germiston; Nigel Deep, Ltd., P. O. Box 50, Nigel.

CARRUTHERS, J. T., *l/o* Fordsburg; 670, High St., Brixton.

CHAMBERS, A. L., *l/o* Johannesburg; Subeni Coppers (Natal), Ltd., Subeni P. O. via Vryheid.

DRAKE, FRANCIS, *l/o* New York; 310, Kohl Building, San Francisco, Cal., U.S.A.

HERBERT, S. A., *to* Simmer Deep, Ltd., P. O. Box 178, Germiston.

INGLIS, A. B., *l/o* Cleveland; Thornton Little G. M. Co., Barberton.

OGLE, F. B., *to* Simmer Deep, Ltd., P. O. Box 178, Germiston.

PRICE, W. S. V., *l/o* Johannesburg; Ginsberg's Cottages, Benoni.

SIMMONS, T. W., *l/o* Johannesburg; Consolidated Langlaagte Mines, P. O. Box 15, Langlaagte.

TURNER, H. F. S., *l/o* Johannesburg; New Primrose G. M. Co., Ltd., P. O. Box 193, Germiston.

URQUHART, W. G., *l/o* Krugersdorp; Simmer Deep, Ltd., P. O. Box 178, Germiston.

WESTON, E. M., *l/o* Brakpan; 19, Jumper St., Jeppes Extension, Johannesburg.

WOODBURN, J. A., *l/o* Johannesburg; Messina Development Co., Ltd., Grenfell Camp, via Pietersburg.