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OF SOUTH AFRICA.

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Proceedings
AT
Ordinary General Meeting,
October 19, 1907.

The Ordinary General Meeting of the Society was held in the Chamber of Mines, on Saturday, October 19th, Prof. J. Yates (President), in the chair. There were also present:—

45 Members: Dr. J. Moir, Messrs. J. Littlejohn, F. F. Alexander, R. G. Bevington, W. R. Dowling, A. Heymann, A. McA. Johnston, A. Richardson, G. O. Smart, Prof. G. H. Stanley, A. Whitby, H. A. White, Prof. J. A. Wilkinson, J. R. Williams, S. H. Pearce, S. Beaton, E. H. Croghan, M. Ford, J. C. Greer, Jr., H. R. Grix, W. H. Jollyman, J. A. Jones, M. Knight, G. A. Lawson, R. Lindsay, W. P. O. Macqueen, D. McKerrell, P. T. Morrisby, T. T. Nichol, C. F. Parry, J. F. Pyles, E. T. Rand, C. E. Rusden, S. Shlom, T. W. Simmons, W. H. Smith, J. J. R. Smythie, A. L. Spoor, H. Taylor, W. Taylor, J. P. Ward, H. Warren, F. W. Watson and J. O. Welch.

10 Associates: Messrs. A. Avent, H. G. Brickwell, J. W. Carter, J. Chilton, R. W. Leng, R. K. Maxwell, R. W. Maxwell, C. G. J. Moore, E. M. Weston and L. J. Wilmoth.

10 Visitors and Fred. Rowland, Secretary.

The minutes of the previous monthly meeting, as published in the *Journal*, were confirmed.

NEW MEMBERS.

Messrs. Croghan and Jollyman were elected scrutineers, and after their scrutiny of the ballot papers, the President announced that all the candidates for membership had been duly elected, as follows:—

ANGUS, WILLIAM HAY, Knights Deep, Ltd., P. O. Box 143, Germiston. Foreman Amalgamator.

MANCE, JOHN COURTENAY, P. O. Box 97, Johannesburg. Mining Engineer.

NEWSON, JAMES ALDRÖVANDI, P. O. Box 113, Klerksdorp. Assayer.

VISIT TO THE JUMPERS G. M. CO.

The President: Since our last meeting we have had a very pleasant visit to the Jumpers mine. I should have liked more of the members to have availed themselves of the opportunity, for certainly those who did go had a very enjoyable and instructive time. I move, gentlemen, that a letter of thanks be sent to the manager for his great kindness.

VISIT TO GOLDFIELDS LABORATORY.

You will have noticed that the metallurgical staff of the Consolidated Goldfields of South Africa have been kind enough to invite the Society to inspect their laboratory. The visit takes place on Saturday afternoon next, and I should like such of you as intend going to be good enough to let Mr. Rowland have your names to-night. I have already had the pleasure of going over the laboratory several times, and I strongly advise any of you who have not seen it to avail yourselves of this opportunity. I am glad to say that Mr. McArthur Johnston is to favour us with a paper to-night on laboratory work.

**THE LABORATORY: ITS ECONOMIC
VALUE.**

By A. McARTHUR JOHNSTON, M.A., F.C.S.
(Member).

This Society has in the past been the medium of upholding the position of the assayer in gold mining work. Many of us can remember the time when the assayer was looked upon by the management as a necessary evil on whom it was very convenient to rely, if his results tallied with the actual gold won. Happily this has changed, and I think I am in no way overstepping the mark when I say that there is no more valued man on the mine staff at the present time than the assayer, of whom great accuracy and reliability

is demanded. Towards this change this Society has done much, and I would therefore crave the indulgence of members in presenting to their notice the work, and the value thereof to the industry, of a well equipped laboratory.

In introducing my subject let me stipulate the scope and duties devolving on this branch. First and foremost, it must be conceived as a sound financial concern. It must be a commercial success, and the ultimate aim should be the testing of all supplies in use on our mines. We all know the state of affairs in Britain, where unfortunately the works chemist is little more than tolerated, but if we go further afield, we find other countries farther advanced in this respect. Germany pays great heed to the work of the chemist, with the result that that nation has, so far outstripped her insular rivals in certain of the arts, whilst the strides made in the United States in all branches of metallurgy is undoubtedly due to the research work instituted by its chemists and metallurgists. The latter country also leads, in that many of the largest companies and railroads buy their supplies on a guaranteed basis, and I learn that a considerable amount of success has followed the adoption of this principle. "Some of the large coal companies," writes *Mines and Minerals*, "such, for instance, as the H. C. Frick Coke Co. already have well equipped laboratories in which not only the coal is analysed, but all supplies such as paints, oils, etc., just as is done now by practically all of the railroads of the country."

Let us consider our case here. We know perfectly well that these gold mines are financial concerns. They buy and they sell. Mark, however, the difference. They buy what is given to them—with restrictions and reservations certainly, but they sell on a definite basis. Their marketable product—the gold—is sold on a very definite footing. Now, on the face of it this seems incongruous. Our near neighbours, the coal mines, are able to supply their wares on the basis of a trial consignment, or on the analysis of a sample of their coal taken across the reef, or it may be on the plausibility of their agents or business managers. The result is that several months' supply of coal is obtained and used before the engineer realises that the anticipated standard is not being obtained, and during this interval considerable friction has been caused with, mayhap, a general clearing out of the firemen. Now, I do not take up the standpoint that the laboratory is and must be the primary factor in determining the class of material to be used. The engineer should come first in this respect, but I most distinctly affirm that the laboratory is and can be of most material advantage if properly used, and it is to point out the various methods which

might be adopted to this end that this paper is chiefly concerned.

Unfortunately the laboratory has in the past in this country been looked upon as an expensive luxury. Our financiers naturally expect some tangible return, from an outside source, of the value expended in the initial cost and running expenses, and as it very often happens that such is not forthcoming, its importance is not appreciated. They forget that it is there that most of the initial work in experimenting and in building up data on which to base changes in running operations, is carried out. Many of the results obtained undoubtedly are negative, but if occasionally one or other idea turns up trumps then the industry is the gainer. Looked at in this way, I may safely affirm that these initial experiments cost but a mere trifle as compared with what they would cost were they attempted in actual practice and, as often happens, found wanting.

I may state that the one and chief end of our work in the laboratory is to reach the managers and engineers in such language and facts as they can quickly grasp and utilise. We appreciate that they are busy men and have little time or even inclination to go into all the complexities and details of an ultimate analysis. For example, in dealing with coal samples sent in, we try to show by the proximate analysis, the exact value of the coal in conjunction with the calorific value attainable with complete combustion. These figures give definite standards—comparable with other coals—and we would only complicate matters, not to talk of the extra amount of work and hence, expense, involved, were we to conduct and report an ultimate analysis on every sample of coal sent in.

Having now outlined the basis on which I consider the work of the laboratory should be conducted, I will proceed to give an outline of the various methods in use in the Consolidated Gold Fields laboratory.

As being of more particular interest to the majority of members, I will begin by showing wherein the laboratory may be of advantage to the reduction works. The mill manager, I find, is not above the aid of the metallurgical chemist, and any tests and work done connected with his department are always welcomed by him. Let me instance one case. It is well known that in starting up with new plates there is a considerable deficit in the gold anticipated, the loss being due presumably to absorption by the plates. Now some tests we conducted showed that by rubbing in silver amalgam, the plates can be brought into good condition, and there seems no reason why this should not be adopted in future practice on these fields. A month's preparation of the plates

in this manner prior to starting would be well repaid if even only 50 per cent. of the gold usually absorbed were recovered. Practical results may of course not bear out this laboratory test, but the fact remains that we did get good results on a small scale, and with the use of nothing which could not be adopted on a large scale.

The cyanide manager requires his lime tested and his cyanide supply standardised. These two articles the Gold Fields group of mines have for some time now been buying on a guaranteed percentage basis, the former determined by the percentage of caustic lime it contains and the latter by the percentage of cyanide (estimated as KCN) and the absence of sulphides. The sulphides are not supposed to rise above 0.02 per cent. Na_2S , whilst the cyanide should have over 1.29 per cent. KCN. Little difficulty is now experienced in obtaining cyanide of the requisite strength and purity, but the basis of estimating the caustic lime present in our local supply has been rather a sore point with the seller, though now that the lone hand practised by the Consolidated Gold Fields has been strengthened by the paper read at a recent meeting by Mr. Croghan, we anticipate a more satisfactory basis—satisfactory, I mean, to both buyer and seller. Occasional testing of the working cyanide solution has been carried out, but further analyses in this direction would be advisable, since the time may come when inferior solubility of the gold or incomplete precipitation of it may require research work, and so far as I know the data on working cyanide solutions is meagre enough. Occasionally, too, some change in the ore or the crushing thereof may necessitate a few experiments to find out the best line of treatment, and those of you who know the paucity of time available for experimental work to either the cyanide manager or assayer can well appreciate the laboratory where such tests can be carried out.

A well-known metallurgist on these fields once told me that whenever he went to a new mine the first man he made friends with was the engineer. I heartily endorse this remark, for I can well appreciate that the man who wants, and who can appreciate, the uses of the laboratory is the engineer. His connection therewith should be practically a part of his daily work, and I am sure he will excuse me if I point out to him some phases of the work there which would advantage him and perhaps simplify his work with, I am sure, a considerable saving of worry. Most of these are already familiar to our own engineers, but a perusal may help to renew recollections of the work already done.

At the present time the supply of coal is exercising the attention of all engineers on these

fields. A short time ago, as you will remember, an amalgamation took place between many of the colliery owners. The idea was to raise the price of coal sufficiently to place the collieries on a paying basis, since competition had so lowered the price at the pitmouth as to allow a very small margin of profit. The importance of this subject to the industry can be well understood when it is considered that the mines foot an annual bill of £1,175,000 for coal alone. The question then naturally arises—how can the mines act so that they may ensure their getting their money's worth? The first response would be the establishment of definite standards of quality in the supply. Undoubtedly the ideal system would be a guaranteed minimum of steam generated in the boilers per unit of coal used. About a year ago Mr. H. C. Behr read a paper on "Power and Efficiency Tests on the Mine" before the Transvaal Institute of Mechanical Engineers. He outlined there how to test which coal gave the best efficiency for the class of furnace and boiler in use. This information, if rigidly obtained, would undoubtedly be the basis on which the future supply of coal would depend, and it would then be the duty of the laboratory to test this coal that the standard be maintained. Payment by results on steam generated is subject to too many variables and difficulties, the personal factor being very often largely in evidence. These difficulties could undoubtedly be overcome, but the simpler plan suggests itself, viz., a thorough sampling of the coal and the laboratory proximate analysis and calorific value. "Engineers, who have adopted fuel testing," says Kershaw, "as an aid in the work of boiler management rarely relinquish it; and as their knowledge of its usefulness extends, they place more and more reliance upon the laboratory test results, and less upon the steam raising trials, in forming their comparative judgments of different fuels."

Initially then we must get a proper sample. In this there is no particular difficulty, since sampling on many times larger tonnages is in constant use on every mine. One method should be adopted and strictly adhered to. I may say that the sample taken during the discharge into the bins is undoubtedly the most reliable. Engineers must beware of placing much value on colliery sampling, since perhaps even more so than in gold mining do seam samples give an unreliable estimate. Car sampling cannot be always reckoned as absolute, for layers may have formed from different parts of the mine during the filling operations; and again, the lump coal—invariably the best—is liable to remain on the top, the smaller particles sintering to the bottom.

The sample is finally crushed and triplicates taken, two being reserved for further reference if

desired. All samples should be placed in stoppered bottles.

In conducting the analysis the evaporative factor is undoubtedly the most important item. This we have always found to give extremely steadfast results when carried out in the Mahler bomb. This method of estimation is undoubtedly the recognised standard and presents little difficulty, whilst eliminating almost entirely the personal factor. I recently made a comparison on bituminous coal between tests conducted by the Mahler bomb and by Rosenhain's improved Thomson coal calorimeter, with the result that I found practically no difference therein.

The proximate analysis calls for the estimation of moisture, volatile matter, fixed carbon and ash. Of these the one calling for most comment is the estimation of volatile matter. By this we mean the volatile hydrocarbons, that is, a part of the carbon and nitrogen, the hydrogen and the oxygen, including the combined water or that not given off at 100° C., and a part of the sulphur. The various methods of estimating the volatile matter are liable to give varying results. Chiefly on account of the proximity of the assay muffle we make use of this and carry out our treatment there. The muffle is carefully dusted all round with bone-ash and the covered porcelain cup containing 1 gm. of the powdered coal is placed on a scorifying dish and put in the muffle. After a minute or so, the time being dependent on the heat of the muffle, the gases generated from the coal escape and burn more or less steadily. Notice is taken when this flame ceases, and within from $\frac{1}{4}$ to $\frac{1}{2}$ minute the crucible is withdrawn, the time in this case also being regulated by the heat of the muffle, a shorter time being required, of course, as the heat is greater. The difference, minus, of course, the hygroscopic water, represents the volatile matter. A further two hours in the muffle, with the cover removed, at a bright red heat is sufficient to get rid of the fixed carbon. Our custom is to place the crucibles for the estimation of the ash in the muffle at the end of the day's assaying and remove them first thing in the morning. In weighing under such conditions it is imperative to tip the ash prior to weighing, since the porcelain crucible is liable to increase in weight from absorption of the lead fumes, or to decrease in weight owing to chipping of the crucible. Since the calorific value of a coal is dependent largely on the amount of ash present therein, I would suggest that coal bought under contract should be sampled daily. This sample could be sent to the assay office during the afternoon and the ash estimated each day, whilst the complete proximate analysis and calorific value could be carried out on a weekly sample made up from the daily samples. A rough daily check

could thus be had on the coal supplied, and, as will be seen later, on the ash sent to the dump. It may be noted that in estimating the calorific values a pressure of about 15 atmospheres of oxygen is sufficient for burning our coals and the adoption of 25 atmospheres, the point usually marked on the pressure gauge, is unnecessary. The crushed coal is placed in the receiving vessel without drying, but the calorific value is estimated as the heating effect of 1 lb. of dry coal, this latter being calculated from the proximate analysis figure. In connection with this it may be noted that the oxygen cylinders bought locally, though imported from England, I believe, do not represent pure oxygen. One we tested a few weeks ago gave 2.2 per cent. CO₂ and 86.6 per cent. O₂.

Of great importance, too, to the engineer is the analysis of the ash sample for fixed carbon and evaporative power. As with the coal this sample ought to be thoroughly representative. There are, however, several considerations to be taken into account when basing conclusions on the figures obtained. First and foremost, one must reckon on the quality of the coal used. It is pretty generally acknowledged that a coal having an ash content of over 40 per cent. is not worth burning, and the nearer the coal used approaches this, the worse will be the ash discharged. There ought, however, to be little difficulty in obtaining an ash having a carbon content of under 30 per cent. when working with one of the better class of coals available here, viz., with about 15 per cent. of ash. The evaporative factor of the ash can be estimated in the Mahler bomb in the same way as that of the coal, but I find it advisable to use a higher pressure and to carefully spread out the sample in the platinum receptacle. The formula we use for estimating the percentage of extraction of the combustible matter is:—

$$100 - \frac{\text{E.F. of dry ashes} \times \% \text{ of ash in dry coal} \times 100}{\text{E.F. of dry coal} \times \% \text{ of ash in dry ashes.}}$$

On these figures an extraction of over 90 per cent. should be easily obtained. We have tried to get for these fields a formula on which to base our calorific value by calculation from the proximate analysis, but so far have failed. In several cases we can approximate it very closely, but practical results have over and over again foiled us, so that we place little reliance now on such calculated values. Indiscriminate mixing of the coal from various parts of the mine is undoubtedly the cause of failure.

Passing from the consideration of coal, the next most important subject, generally speaking, is the feed water supply for boilers. Some recent researches on the loss of heat due to boiler scale describe a loss up to 10 or 12 per cent. from a scale thickness of $\frac{1}{8}$ in. It has also been concluded

that the mechanical structure of the scale has quite as much, if not more, effect than the chemical composition. The engineers who get the use of town water are lucky. To the majority this is denied, and they have to make the best of the mine water obtainable either from their own or a neighbouring property. In very few cases is this water equal to boiler feed requirements, and I have little hesitation in affirming that a water purifier is an absolute essential on all producing properties here. Not a few have been erected but, through misuse or neglect, they have failed to give satisfaction and are now in many cases derelicts. The services of the chemist are undoubtedly of advantage here, and less attention should be given to the rule of thumb man. Summarising my experiences on these fields, I can safely affirm that the chief difficulty is to obtain a thorough mechanical mixing, either by injecting steam or stirring, between the soluble solids in the water and the chemical reagent added, so as to bring about the desired reaction.

I have found that the invariable result is the entrance of the alkali into the boiler, and this in time brings about a state of affairs worse than the first. It is almost useless to depend on the self-feeding mechanical arrangements which have been so generally adopted. Theoretically, they give splendid results, but rarely do they get the necessary attention that the highest purification can be guaranteed. What we require is a water softener, which will by heating or agitation bring about an intimate mixing of the reagents with the solids in solution, and a careful superintendence thereof. The water in the mine changes month by month, and there should be therefore a regular analysis of this and of the purified water if the best results are to be obtained. For comparative work, a rough and ready guide to the engineer who cannot avail himself of the more satisfactory complete analysis, is Clark's hardness test with the soap solution, though a little practice is necessary for carrying this out. The degree of hardness to be aimed at should be well under 4. The deleterious effect of oil on boiler plates is so well known that this should always be looked for in feed waters.

With regard to the addition of boiler fluids to the boiler I have little to say. They are but a makeshift at best and their use is soon discontinued.

Analyses of boiler scale are also of service to the engineer, and in estimating this it must be remembered that our mine waters contain gold in solution. Hence boiler scale may and does generally contain a considerable amount of gold and silver. The result is, however, more interesting than payable—at least let us hope so, for our engineers' peace of mind.

Let us next consider the question of lubricating oils. The most important points to be considered in the testing of these are the flash point, the percentage of fatty oils present, the acidity and the gumming. A high flash point is a desideratum which all engineers strive after, and especially with high class cylinder oils. For the latter there should be no difficulty in obtaining a compressor oil with a flash point of over 500° F., and this will undoubtedly be a necessary standard in the near future on these fields, since it is only by adopting the best class of oil that fouling of the compressor air can be avoided. The question of the percentage of fatty oils present and of acidity calls for little comment since all good oils seldom fail in this respect. The gumming test is, however, rather important, as we have found that oils failing in this test gave very unsatisfactory results in actual work. The film formed by some oils, and which prevents easy working of the lubricating surfaces, is due to the presence of rosin oil, to oxidation, or it may be to the presence of soaps formed by the chemical action of the free fatty acids on the metal of the bearing, dissolving in and thickening the oil. It may be noted that it is only by fractionating the oil that the presence of resins can in many of the lubricating oils be detected.

Although the lubricating power of an oil is closely related to the viscosity, we place little importance on the estimation of this, for all our engineers are good at estimating for their respective requirements the necessary viscosity. A set of figures giving the rate of flow of 50 c.c. through a Boverton Redwood viscometer conveys but little to the working engineer, though I may mention that the figures we do give are reckoned on the rate of flow at 60° F., 75° F., 100° F., and 150° F.

Rope greases also call for some attention. In this case the desired information would include primarily the presence or absence of grit or body likely to set up irritation or to interact chemically with the bearing surfaces. Acidity must also always be looked for, and more especially when there is present a disguising cloak in the shape of nitro-benzenes.

Of considerable use also to the engineer is the Orsat apparatus usually found in all laboratories. Many of the most up-to-date of our mines are equipped with self-registering carbonic acid apparatus, and the Orsat is periodically of use, not only in checking these but also in determining the carbon monoxide percentage. This latter engineers are occasionally inclined to overlook, forgetting that a high percentage of CO₂ is not always consistent with good work, since not infrequently CO may be present, and, of course, is not detected by the self-registering instrument.

The more accurate Hempel's pipettes are also necessary appliances, our greatest use for them so far being in the estimation of mine gases.

Apparatus for the registering of heat generated in furnaces is a decided gain to any laboratory. A set of Saeger cones we stock for reference and check work, but the most important, because handy, instrument we have for this purpose is the Féry pyrometer. The reading can be taken at a convenient distance from the furnace, since the appliance works by radiation, and there is little danger of destroying the instrument, provided ordinary care be taken.

The testing of candles is a most important one when we consider the enormous quantity used, the figures for the last twelve months, as shown by the Government Mining Engineer's report, pointing to a consumption in gold mines of £208,000 worth. In conducting this examination practical tests are conducted, the aim being to determine their value in actual working conditions as approximately as possible. The intensity of the light, the height of the flame and the melting point are, of course, undertaken, but as none of the candles so far tested by us have failed to pass the standard requirements, we put more importance on the actual burning, viz., the cost per hour consumed in still air and the price per hour consumed in a draught of about 60 ft. per minute, noting the guttering of the wick and the hardness or softness of the candle. Comparisons are made with the standard English sperm candle, and the draught is measured with an anemometer placed alongside the burning candles.

Primary importance also should be given to the testing of Portland cements. The quantity consumed in this country last year, amounting to £107,000 for our gold mines alone, is a sufficient excuse for giving this subject our earnest consideration. There is no doubt but that there have been imported, and still is being imported into the Transvaal, cements which have been shunted here, simply because they have failed to pass the requisite standard specifications in the land of their birth, and knowing that they are seldom tested here, they have been shipped with, let us say, the pious hope that the sea voyage would benefit them considerably. The engineer has experienced this to his sorrow, when he finds that to begin with they fail to unite with the accustomed proportion of rock and sand, and later on when he sees his foundations going. It does indeed seem strange that cement, the foundation work of much valuable plant and machinery, should have received so little attention, especially as the testing of this is so vigorously adhered to in European countries and the United States.

The tensile strength is undoubtedly a good test on all cements; but there is nothing in this test

which good cements have any difficulty in negotiating, indeed, my experience has been that the best imported cements seem to have been built to pass these tests. On the other hand, the poorer cements fail badly, the most common cases of failure being apparent in the sand test. I distinctly favour the adoption of the English standard specifications, since the hand ramming with a competent workman is not so liable to be overdone as mechanical ramming. With mechanical ramming, too, about one-fifth more material can be pressed into the mould, and this should call for a higher tensile strength. It may be urged that ramming is adopted in actual practice, but as against this we must remember that the necessary qualifications for a good cement are comparative, not with actual conditions, but with other laboratory results. If mechanical ramming be decided on, then it should be imperative that a machine giving the same number of blows and exerting the same pressure on all samples be used. The sand we employ in making these briquettes is clean quartzite passing a 20 mesh sieve and remaining on a 30 mesh and thereafter washed and dried prior to use. This gives a high percentage of voids, but the results obtained in using this sand are very satisfactory, though, of course, only comparative with other work done here.

There seems to be some dissatisfaction as to the value of the soundness test. The best test, of course, is time, but this is not always practicable, and we must resort to heat to give us the desired information. Pats of all descriptions are made, but sufficient notice is given to the subject when we mention the Le Chatelier tongs test, as recommended by the British standard specifications. This calls for a six hours' boiling of the cement, after 24 hours under water, and a consequent maximum of expansion of 12 mm. So far as we can learn there have been manufactured cements which have failed to pass this test and yet have withstood the lapse of time, but as against this we find the British Committee this year recommending the reduction of even the present maximum of expansion. The action is not well understood, but there is safety in the advice of Taylor, who concludes that it is well never to use a cement failing in the accelerated test, when one equally suitable can be obtained passing this test. German manufacturers and chemists, who have done much good pioneer work with cement and the testing thereof, favour heat tests. The specific gravity test, the fineness and the chemical tests, are all useful, but there are few Portland cements introduced into this country which fail to pass the called for requirements. Most of the cements met here are slow setting. A demand has set in for some quick setting

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cement, especially for tube mill setting of the blocks, but I do not know of any here to satisfy that class of work.

(Of considerable importance, too, to engineers is the quality of the rubber imported into this country and sold by the agents as pure. Recently we tested a lot of presumably best rubber which contained but some 10 per cent. of rubber. The filler or mineral matter of this material was composed of barytes, chosen perhaps on account of its high specific gravity and because rubber is sold or, is it better to say, bought by the pound avoirdupois. Also, some rubbers may contain as their filler a metallic compound which would be utterly unsuited for the work for which they are intended. For example, if zinc oxide is the inorganic ingredient in a rubber exposed to water containing sulphuric acid, the rubber deteriorates quickly, whereas the destructive action of all oils upon india-rubber is more particularly checked by the presence of zinc oxide.

Another most important item in mining economics is the supply of explosives on which well over £1,000,000 is annually spent. On account of the hardness of the rock composing the matrix of the gold, practically only the most powerful explosives are used on the Rand. The stringency of the laws governing the manufacture and importation of this commodity makes the testing of the same here of but secondary importance. The best blasting gelatine obtainable is at hand for the mines and failure of it is infrequent. The most common test carried out at the laboratory is the ascertaining of the relative blasting powers of different explosives. These we obtain with the aid of the lead cylinder or crusher gauge and the foot pounds machine diagram introduced by Lieutenant Quinan. In passing, I may say that we fitted up an apparatus for checking Quinan's curve, and our results, which we carried to a higher degree of compression, thereby elongating the curve, agreed very closely with his, any little differences apparent being, no doubt, due to our carrying out the tests in the open air. For the analysis of explosives the laboratory is also equipped, but the non-traceability of accidents to inferior or faulty explosives has rendered the work done in this direction so far of little service. It will have been noted, however, of late that mercury bichloride has been added to some high-class explosives with the ostensible purpose of preventing moulding, but incidentally masking the heat test results. Since great reliance is placed on this heat test, the presence of mercury should always be looked for.

Of more value is the testing of safety fuse. According to the figures of the Government Mining Engineer safety fuse, valued at £118,000, was imported into this country for use in gold

mines last statistical year. Fortunately for the industry the testing of this commodity is carefully superintended by Government inspectors, and only once have I had to condemn fuse which had recently been imported and supplied to our mines. We happened to strike a bad case. Apart from this we have had to report adversely on safety fuse which had been stored for some little time in this country. Chemical members will know from experience how quickly rubber goods perish here, and the waterproof material used in safety fuse very speedily deteriorates, with the consequent result that it fails to pass the pressure test under water. Only one fuse has, so far as my experience goes, stood the test of lying stored for a long period in this dry climate and altitude. Others of the same age have failed, some of them miserably. The value of this testing is appreciated by our underground managers, though there are some who claim that the best way to test fuse is in actual experience. I grant this entirely, just as the best way to get the true value of a gold mine is by crushing, amalgamating and cyaniding the ore. The laboratory method is quicker and less likely to lead to bad results in breaking ground, and though, of course, only a small part is tested, more attention is given to the carrying out of the test than is possible underground, thus rendering the result more conclusive and not exposing the fuse to undue prejudicial handling.

Recently we have introduced into our laboratory apparatus for the microscopic analysis of metals. On this subject I intend to say little at the present time, as on some future occasion I hope to bring before you details of some of the work already done on the microstructure of metals and alloys. As most of you are aware the examination of metals is conducted under three headings, chemical, mechanical, and structural. The latter science is but in its infancy, but its growth is extending rapidly, and the numbers of eager workers, who yearly contribute the results of their researches, augurs well for the future utility of the science. Engineers and mill managers in this country have often had some of their best work rendered nugatory by the breaking of some difficultly replaceable part of some machine, and an indignant—speaking mildly—reference to the agent, and through him to the maker leads to nothing satisfactory, if we except the satisfaction that the manufacturer has in writing that the man out here is not competent to judge whether the breakage is due to bad material or not. Microscopic analysis aims at getting at the root of this evil, and in combination with the chemical analysis and mechanical tests there is no reason why the mines should not get some of their

own back. In this connection I may say that so far we have been able to condemn, and demonstrate the cause of failure of, not a few of the castings and steel imported for use on our mines. Let me instance one case at the present time, and in doing so show you by means of the accompanying photo-micrographs what I mean. Two sets of shafting were supplied to one of our mines by different makers. The one lot has done and is doing extremely good work, whilst the other set broke after being in use for some days. Both were sold as low carbon steel, and the chemical analysis showed that both contained practically the same amount of carbon. A microscopic examination, however, revealed the cause of the trouble. The sample was polished on jeweller's rouge and examined under the microscope. It was then photographed under a magnification of about 70 diameters, and I have here for your inspection the results.

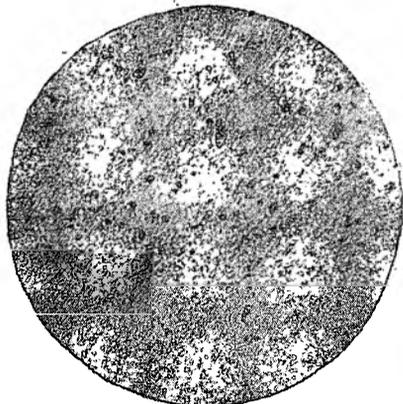


FIG. A.—V.

FIG. B.— $\times 70$ Diameters.

The first, Fig. A, represents the good steel, and the surface is seen to consist of the metal with very small particles of slag, all more or less of a rounded shape and evenly distributed, whereas the surface of the bad material, Fig.

B—in reality wrought iron—is broken up by numerous lakes of slag. This constitutes the cause of weakness, and as it thus proved that wrought iron has been supplied instead of steel, we are quite justified in demanding a replacement of the shafting. Other work connected with this subject I hope to place before you later, but this example may tend to illustrate the utility of photo-micrography in the detection of inferior steels and iron supplied.

Another branch to which we have devoted a considerable amount of attention in the past has been the treatment of ores from the outside districts. This has been attended with a very appreciable amount of success, since I am pleased to say that our recommendations have been adopted and have borne good fruit.

Again, one of the most important of the many questions to be decided in the laboratory is that of keeping our metallurgical ideas up to date. All of you know how often, on reading of some new development on these or other fields, the desire to test this on your own account, or to prove its applicability to some of your own problems, has assailed you. Necessary apparatus for conducting these trials on a small scale is usually wanting on the mine, and a request for trying them under working conditions generally results in a curt demand to keep down costs. Set-backs in this direction are certainly not conducive to individual effort, and the heads of our industry recognise that more concerted action in this direction would be advantageous, but hitherto the sum total of this recognition has been its recognition. An "experiments fund" was inaugurated some two or three years ago by our largest group here and, quoting a local weekly, it seems that already £15,000 has been expended in trying new devices, and that in one case an unsuitable device cost £4,500 prior to its being abandoned.

Now, I have no hesitation in saying that quite a considerable portion of this could have been saved had the initial tests been carried out on a laboratory scale, meaning thereby a mechanical as well as metallurgical laboratory. I may be permitted to draw the attention of members, however, to one source of false conclusions which are only too liable to crop up in initial experimental work, that is, the test must represent the absolute duplication of the working conditions. That many failures on the large scale are undoubtedly due to neglecting this is only too true, and an impartial test by a thorough practical man should always be insisted on prior to money being spent in large tests. We know how prone the inventor is to overlook small details, which in themselves may latterly cause the failure of the trial. From the point of view also of the originator we find

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that his grievance is, that on a mine sufficient attention is not given to the carrying out of his ideas to the best advantage. As against this, the engineer, mill manager and cyanide manager usually have sufficient daily worries of their own and are sometimes inclined to afford scant courtesy to the man who increases these worries, more especially if he be a non-practical worker.

A central laboratory would, I am sure, eventually save the industry some thousands of pounds annually, and this Society ought to take a leading part in its initiation. Personally, I am inclined to think that most of this work should be carried out under State supervision—financially I mean—for undoubtedly the solution of some of our problems would lead to the enhanced life of our mines and the opening up of reefs at present unpayable. In this connection I may point to the good work being done by our great rival in gold production, the United States of America. "There can be no doubt that," says a weekly journal devoted to mining, "the United States Geological Survey Department is doing an enormous amount of really excellent work, and if it costs money the authorities have at least the satisfaction of knowing that the value of their liberal policy is fully appreciated not only in America, but also by technical readers throughout the world." When will such a paragraph be written concerning the gold mining problems of the Transvaal?

Gentlemen, you may think this a long introduction to laboratory work, but I mean it only as an introduction. There are many more things I should like to lay before you in this connection, and I hope some of you would like to ask for further information. My object in writing this paper is to bring the laboratory before your notice, and if any of you feel that you would desire a more intimate knowledge of the work done there, I shall be pleased to see you, and by actual experiments demonstrate the value of laboratory test.

Before closing I should like to record my thanks to one of your Past-Presidents, Mr. W. A. Caldecott, for permission to write these notes and extend this invitation to you. He has been indefatigable in having the laboratory fitted up with the best and latest mechanical and chemical appliances, and the success which has attended it since its inauguration is due entirely not only to the keen interest he evolves in all the work there, but to his personal magnetism in keeping us all going.

The President: We have to thank Mr. McArthur Johnston and the Consolidated Gold Fields Company for a paper of exceptional

interest, which I am sure you have all appreciated just as much as myself, and that is saying a great deal. He has, I think you will admit, made out an ample case for the testing of all our materials and stores, and in view of the great sums involved I fail to see how it is possible to adopt any other course than to test everything thoroughly. I do not feel called upon to say more concerning this matter to-night, for I feel sure that we will have an interesting discussion.

Before sitting down I would point out that Mr. J. R. Williams is with us again to-night. It is a real pleasure to see him back again and looking so well and hearty.

Mr. J. R. Williams: I beg to thank you, Mr. President, for your kind remarks, and I can only say that I am sure I am equally delighted to be back again in the Transvaal.

NOTES ON SMALL STOPE DRILLS.

By E. M. WESTON (Associate).

Facts must always be of more interest to this Society than opinions. I have, I am afraid, few new experimental facts to bring forward, and this paper must be considered merely as an attempt to collect and bring up to date our knowledge of this stoping problem, and to furnish material for discussion. One cannot help being amused at the fact that it is taking the efforts of a painstaking and very inquisitive Government Commission to rediscover facts about the Mining Industry, and especially about the conditions of rock drill work here, that were fully discussed by this Society in connection with a former paper of mine, read two years ago. The Commission will certainly not suffer for want of opinions, even if, among other things, they are of a radically contradictory nature regarding small drills. In a former paper I advocated the use of higher air pressures, and expressed my belief in the capabilities of small drills from 2 $\frac{3}{4}$ in. down. I also contended that three 2 $\frac{3}{4}$ in. drills worked with the same air pressures would do more work than two 3 $\frac{1}{4}$ in. drills, even in stoping in large stopes. This was generally denied. I believe the experiment was tried by Mr. Ford on the Meyer and Charlton mine, and the final result was that it was found better and cheaper to run with two 2 $\frac{3}{4}$ in. machines than with two 3 $\frac{1}{4}$ in. machines. Six holes are now drilled, per shift, with each machine. This experiment throws a great light on much of the evidence that has been given before the Commission. It shows that where in any mine the men are given three machines and told to work them, regardless of

the fact that thereby the air pressure is lowered 10 or 15 lb. per sq. in., the efficiency doubtless does fall off. Practical miners who have given evidence before the Commission that they could get as much work out of three machines as out of two were, no doubt, in many cases, right; but such evidence had really nothing to do with the real question, which was, could a white miner supervise three machines efficiently in certain cases? Two years ago Mr. Polglase pointed out that increase of fathomes broken did not correspond in the same ratio with increase of drills used per man; but he did not, I think, point out the real reason. This is not, I think, due to the fact that the supervision is necessarily poorer, but to the progressive lowering of air pressures. This trouble has been partly due to the big mill policy. A manager had a battery of 100 stamps to feed, and he could break enough rock with the machines at work and yet keep his air at some reasonable pressure. It was decided to put up another 20 or 50 stamps—to reduce costs, of course. No doubt, theoretically, the mine's compressor was supposed to be able to run enough extra drills; but leakage losses may have been anything from 10 to 60 per cent. The manager had to put extra drills to work. He gave his men three, who had before been working two, and had to put a few extra machines on a pipe line already too small for those working on it. The drilling pressures dropped far below efficiency point, and directors wondered why the extra stamps did not reduce their costs to the extent expected. It is thus evident that in many cases the working of three machines per man may show a loss, and in some mines it would doubtless pay to stop 10 per cent. of the machines at present at work, or to replace them with smaller sized ones, or ones having a lower air consumption. To give engine room pressures as working pressures underground on the Rand, and to estimate the compressor power on the Rand, without allowing for leakage losses, is not considering all the facts.

Practical miners naturally take advantage of such conditions to tell the Commission that they cannot efficiently supervise three machines. This, I know from actual experience, can be done where the conditions are favourable, and where six holes, if necessary, can be drilled from one rigging up, or where only four or five holes are put in per machine. All this apparently irrelevant discussion has a very direct bearing on the small drill question; because if small drills of any type are to be a success, and to compete with hand labour, or replace it, one white man must supervise three or more; or by using the highest practicable air pressures, the rate of boring must be so increased that it will pay to employ one

white man alone with each machine. In most mines, to try and displace hand labour with small machines, with the present compressor power and pipe lines, would spell disaster. Very conflicting evidence has been given before the Commission regarding the value of small drills. Generally, they have been condemned on past performances, though good results have been claimed in certain cases. The piston drill has done good work on the Crown Deep, the Geldenhuis Estate, and the Jumpers mine, and others, and the Gordon drill has had success at Randfontein, Kleinfontein, and other mines. It is urged against the present types of small piston machines, that maintenance costs are as high, or higher, than large machines, owing to the endeavour to reduce weight at the cost of strength; that operating costs are very little different from those of large machines, as the miner in the past grumbled at being asked to look after more than two of them; that power costs came out the same as with large drills, because in small stopes a greater footage of boring must be done to break the same tonnage of rock as would be required in large stopes. Costs are stated to be as high, or higher, than with hand labour on the average, though it is conceded, that in soft ground at the Jumpers, the Geldenhuis Estate No. 3 level, the Nourse Mines, etc., they may show an apparent economy.

To me, there seems to have been various obvious reasons for the failure of small drills in the past in many mines. Apart from the hindrances to good work, touched upon in a former paper, the chief reason was that the running of small drills in narrow stopes calls for more skill, and what is perhaps more important, harder work, more running about, and closer supervision than running two large drills in large stopes. Hence, under the conditions prevailing before the strike, it was considered pretty well a disgrace to run small machines. It was scarcely likely good miners would bother with them, when they could earn more money sitting down comfortably watching two machines at work in a large stope. They preferred often to throw the small machines down the stope. The machines were thus very soon handed over to the tender mercies of the inefficient and the loafer. No one was told off to study their working and effect needful improvements in their operation. Then again, many of the machines were poorly designed and made of poor material.

Messrs. Holman are now making an improved small drill of totally different design, so they will perhaps forgive me for saying that their old $2\frac{1}{2}$ in. diameter tappet machine, was about as bad a machine as could be made. I worked three of them for a year, and perhaps an account of

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some of their ways of breaking down may be of value, so I give it. First, the machine had a taper chuck, in which the drill tightened itself and had to be knocked out with a drift. The drill bits used in them had to be tempered hard on the corresponding tapered shank. This made them brittle, and they were always breaking in or near the chuck. If they were not sufficiently hardened, they burred up or twisted on the end and could not be taken out. Sometimes the chucks themselves split, owing to poor metal. The worst was, however, that the bits were always sticking tight in the chuck. It was quite a common thing to have one stick fast just as one was hurriedly trying to finish a hole at the end of a shift. It was impossible to use them at all without liners of tin, which the rock drill fitter made for us, or which we cut out of old oil tins; but even with these I have spent hours trying to extract a drill. I have lit fires of candle ends and candle boxes in the stope to heat the chuck, and have, even in desperation, blown out a drill end with a detonator and a shaving of gelatine. Both springs and ratchet were of bad steel and constantly broke. The cradles broke at the neck of the seat. It was almost impossible to keep the valve chest packed tight, and one often had to stop and repack it with a time card. When U bolt chucks were obtained, the U bolt was too weak and was always breaking, and the pads, or keys, were too soft, and always wearing out. With all these drawbacks, and only moderate air pressures, these machines could bore well. They broke half a fathom each, per shift, in average Main Reef Leader ground at 1,000 ft. deep. All these defects could have been easily remedied, and I consider it nonsense to say that a machine for work in 38 in. stopes cannot be built to stand up to the work, even in the hardest rock. My quarrel with the manufacturers of small piston drills is that they do not seem to know what they want, or what the mines want. Are the machines they sell to-day, one man or two men machines? If one man machines, why not arrange them so that one man can run them? If two men machines, what is the sense of cutting down a pound's weight here, and another there, and weakening the machine to breaking point? Surely, they must know that a machine weighing 140 lb., is practically just as easy for two natives to handle as machines of 90 and 100 lb., and can bore quicker, and cost less for repairs.

Until very recently, the 2½ in. small drill was the standard small stope drill all over America and the United States, and is still used in development largely in U.S.A., Mexico, and Canada. The drill is worked by one man, with perhaps occasional assistance in rigging up, and,

theoretically, there is no reason why a small drill cannot be employed as economically as a large one; for if the parts are smaller, the stresses met with are also reduced.

Two systems of work can be employed in narrow stopes. It has generally been denied that holes over 5 ft. long can be economically employed in small stopes of under 42 in. Indeed, some appear to believe that 48 in. is the limiting width for stoping economically with machines. This I know to be a mistake, as we employed holes up to 7 ft. long, and yet kept a stoping width of 38 or 40 in. This method involves the use of 2½ to 2¾ in. machines, and two natives to a machine, with one white man supervising three machines. Four would be too many in ordinary situations. In stopes where the reef itself is 30 or 36 in. wide, I think this method of stoping, with good air pressures and working the stope as previously described, can be made the most economical known, especially if a 2¾ in. Konomax drill with decreased air consumption was found suitable and could be employed. The machine used can easily be made as solid and heavy as necessary, and the repair bill be kept down. One white man can supervise three machines, even with a shifting labour supply, if he takes the trouble to teach his natives, and to explain what is required of them, and why it is required. I always found the Kafir as intelligent as the Chinese, if pains were taken to instruct him. When two or three leading natives left, I always had spanner boys competent to take their place, and thus always had skilled boys under me. The steel used in average ground would be, starter, 18 in. long, star bit, 2 in. diameter. Second, 36 in. long, star bit, 1½ in. diameter. Third, chisel or star bit, 1½ in. of 1¼ in. steel, 1½ in. bit, 4 ft. 6 in. long. Fourth, chisel, 1 in. steel, 6 ft. long, 1¼ in. bit. Fifth, chisel 7/8 in. steel, 1 in. bit, 7 ft. 6 in. long. In hard ground, sizes of bits would be ½ in. or ¼ in. larger all round, and the final steel would be 1 in. in diameter with an 1½ in. bit. To employ such long steel requires greater skill, and even then, stope widths in certain ground would tend to be too high, but should in most cases be easily kept below 42 in., which recently was given as the hand stope widths on the Main Reef Leader in a group of mines. In heavy ground, requiring timbering, it is, of course, an impossible method. It is sometimes forgotten that lashing should be 6d. a ton cheaper in a stope of 40 in. than in one of 30 in., and that the excess widths of machine stoping are generally caused by large blocks falling out of foot or hanging wall, and these cost very little to stack, or roll down out of the way. The other system calls for the use of the one man machine proper, and such machines must be made as light as possible. They may in

the future be used in two ways. It may be found economical to run with one white man, gaining increased efficiency and lesser maintenance costs, to make up for higher labour charges. The number of feet that such a machine must bore per shift, to allow of paying for the labour of a white miner, and to make costs compare with those of hand stopping, can easily be worked out for each individual case. In considering whether this can be done, we must remember that records made in the shallower levels of the Geldenhuys Estate and Jumpers mines are no criterion of what can be accomplished in the harder reef of the deeper levels. This also, of course, applies to hammer drill performances at Randfontein and Kleinfontein.

One man drills, run either by natives with four or more in charge of one white, or perhaps, with one white per machine, and boring short holes in stopes of minimum width, are undoubtedly needed to break much of the thin, rich reef on the Rand. With the method mentioned of employing long holes, one rigging up per shift, will generally suffice, as five or six 6 ft. to 7 ft. holes can be bored from one bar, if the stope is properly benched and the bar properly rigged up, as previously described. The problem of taking down and rigging up again, becomes important with one man drills. A miner who is going to bore ten 3 ft. or 4 ft. holes must rig up 3 or 4 times. Even

with a 3 in. bar and a clamp and arm arranged as in the Gordon drill, this takes time. A miner, in charge of such machines, should be provided with a selection of spare bars of different lengths, and can then set up fresh bars, ready for his workers to move on to, while his machines are at work. A change from one bench to another can then be made with only ten minutes' delay. This matter is often neglected with big machines. The battle of types between the various small drills is only beginning, and there are yet no definite results to base comparisons on. The question of the ultimate supremacy of the piston or hammer type for work on these fields remains to be solved. Undoubtedly the feature of mining work in America has been the rise and development of the small pneumatic hammer drill. It has undoubtedly displaced the small 2½ in. piston machine to a very great extent in stoping work there and in shaft sinking. In England record footages in sinking in limestone have already been recorded by their use.

Hammer drills may roughly be divided first into two main classes. Those which are valveless, with the differential piston or hammer itself acting as the valve. Machines of this class are the Little Imp, the Hardsog Wonder, the Murphy, the Sinclair, and the Leyner Rock Terrier drills (Fig. I).

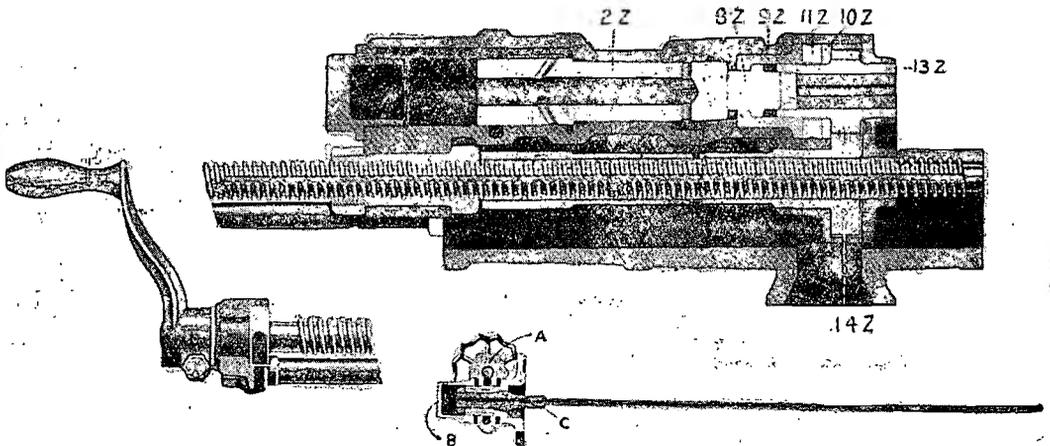


FIG. I.—LEYNER ROCK TERRIER DRILL.

No. 13z is the chuck, formed to receive drill steel of the ordinary cross type.

No. 11z is a buffer, 10z a buffer plate, and 9z the bearing for chuck and for hammer pin. These three parts together serve as a cushion for the striking pin and hammer upon the forward stroke. On its backward stroke the piston cushions upon air.

No. 2z is the piston and 8z the striking pin. Through the latter the blow of the piston is transmitted to the steel.

The outer part of the chuck (13z) forms a pinion, which meshes with the rotating gear (14z). Within this rotating gear are two feathers, corresponding to the two opposite longitudinal grooves in the feed screw.

The turning of the crank, operating on the feed screw, and thus, through the rotating gear, upon the pinion teeth of the chuck, effects the rotation of the chuck and steel.

WATER FITTINGS UPON THE DRILL.—Entering the water connection (A), the water passes into the water connection cap (B) and thence into the water tube (C), which conveys it, without contact with the working parts of the machine, through the drill into the shank of the hollow drill steel. The water tube is much smaller than the passage through the steel. Around the tube, through the same passage, mingled with the water, compressed air is discharged through the steel. A simple needle valve (hand wheel of which is shown in cut) controls the discharge of water.

The large Leyner machine is worked by a piston valve (Fig. II.), and the Little Jap by an axial

block to take up and transmit the blow of the hammer, as in both Leyner machines and the Gordon drill, and nearly all the rest which strike the blow directly on the steel itself. They might also be divided into short and long stroke machines, but I will return to this feature later.

The favourite small machines in America are certainly those without a separate valve, and which strike the steel direct. Their advantage consists in their extreme simplicity and lightness. They transmit the force of this blow more directly to the rock, having to overcome the inertia of the drill steel only; but they work generally on a short stroke and rely on a high velocity to give momentum to the hammer, which is light, as otherwise trouble might occur through the burring up of the shank of the drill bit, and, as they are often worked held in the miner's hand only, heavy blows would cause too much jar on the operator's muscles. The pistons can be expected to last a long time without undue leakage, as they are ground into a case hardened cylinder; but with grit getting in, leakage losses may be developed quickly.

In the valve machines of small size trouble has been caused, and doubtless will be caused, despite the use of strainers, by small pieces of grit or scale introduced from air pipes or by careless handling getting in and choking the narrow parts or sticking the valve. Some mines in America had to throw out the Little Jap machine on this account. Valve moved, anvil block machines are heavier and more complicated, but they have a better valve motion, and air can, if necessary, be used expansively. They do not burr up the end of the steel, but they require anvil blocks of special quality steel or are troubled by constant breakages. Differences are also found in the manner in which rotation is affected. The large Leyner machine uses a ratchet and rifle bar like those of piston drills. The Leyner Rock Terrier, drill uses a positive geared rotation worked off the feed screw. The Gordon machine has a positive rotation worked by a central spindle ending in a handle and ratchet at the rear of the machine. Most of the other drills are pivoted on the handle or air feed, and are oscillated or revolved by a handle on the front which often contains the air valve. The drawings show a typical make of a simple American hammer drill and its air feed attachment (see Fig. III.). Nearly all these makes of drills are working with success in America, and most of them have been tried here without success. The big Leyner drill is at present, I believe, at work on only two mines, the Jupiter and the Consolidated Main Reef. The Rock Terrier drill is not at work anywhere here. What are the causes of failure? Why is a machine that is

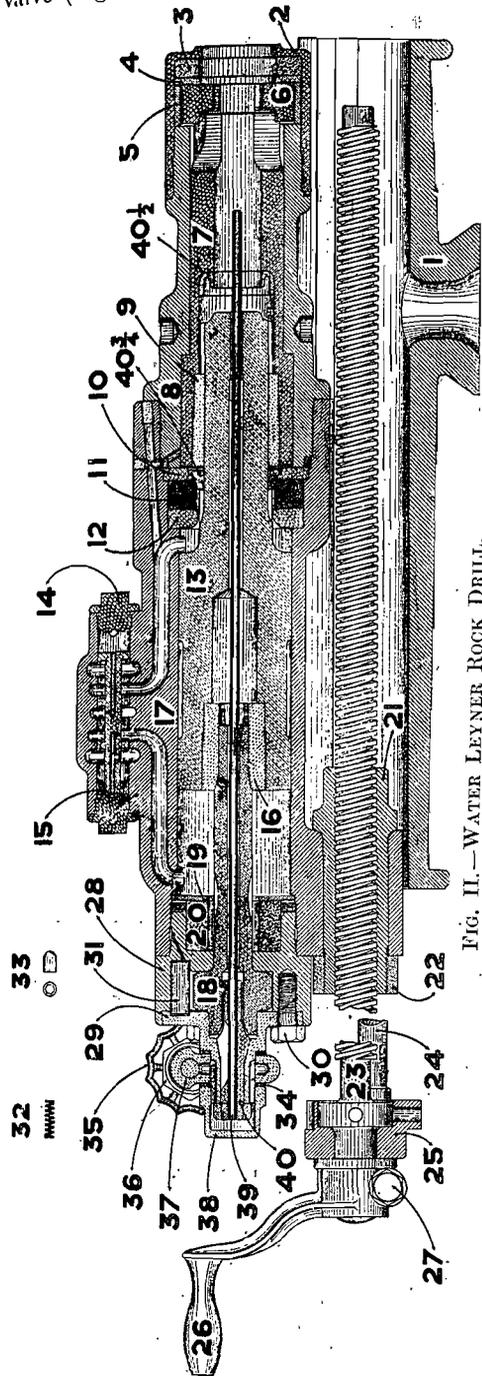


FIG. II.—WATER LEYNER ROCK DRILL.

valve, and the Gordon drill by a piston or spool valve at right angles to piston. They may again be divided into those drills that employ an anvil

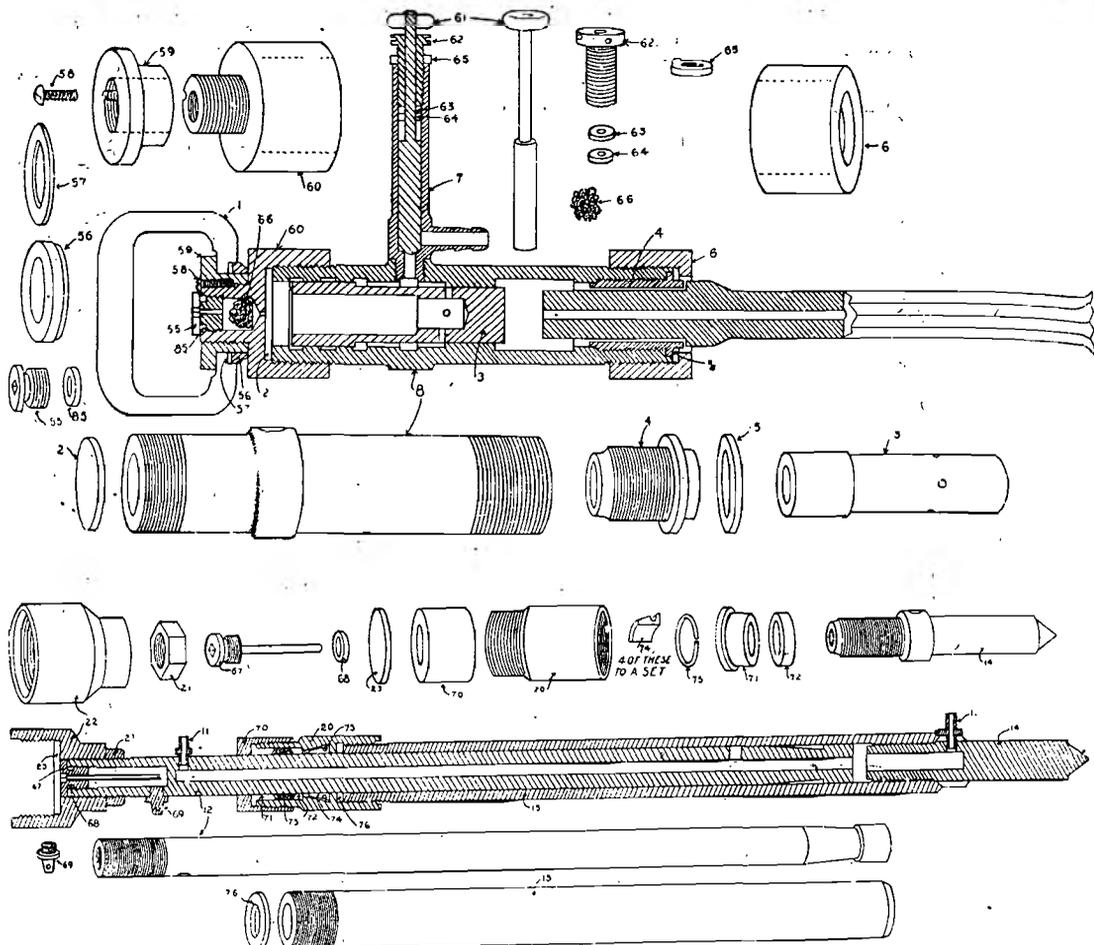


FIG. III.—MURPHY DRILL.

- | | | | |
|--------------------------------|------------------------------|----------------------------|----------------------------------|
| 1. Handle. | 12. Piston rod. | 58. Lock screw for handle. | 68. End oil plug washer. |
| 2. Leather washer for handle. | 13. Cylinder. | 59. Nut for handle. | 69. Side oil plug. |
| 3. Hammer. | 14. Tail piece. | 60. Back head. | 70. Feeder stuffing box nut. |
| 4. Bushing. | 20. Feeder head. | 61. Valve stem. | 71. Stuffing box gland. |
| 5. Leather washer for bushing. | 21. Feeder lock nut. | 62. Stuffing box gland. | 72. Stuffing box ring. |
| 6. Cap. | 22. Feeder cap. | 63. Stuffing box ring. | 73. Packing. |
| 7. Valve casting. | 23. Leather washer. | 64. Stuffing box ring. | 74. Dogs. |
| 8. Cylinder. | 55. Oil plug for handle. | 65. Lock nut. | 75. Spring for dogs. |
| 11. Tool steel nipple. | 58. Rubber buffer. | 66. Sponge. | 76. Leather washer, feeder head. |
| | 57. Steel washer for handle. | | |

a success elsewhere a failure here? In trying to answer this question I must borrow largely from current American technical literature. The working air pressures on this field are only 40 to 75 lb. per sq. in. These machines were all made to be operated by air at 90 to 100 lb. pressure. G. Leyner, the pioneer of hammer drills, is reported to have said that it is impossible to get satisfactory work from a hammer drill with air pressures below 70 lb. The reason of the failure of these hammer drills is now obvious, apart from certain other defects. They were all provided with a light piston weighing ½ lb. to 1 lb., with a very short stroke, and they

relied for the kinetic energy of the blow (the product of half the mass and the square of the velocity $\frac{MV^2}{2}$) on the high velocity caused by the high air pressures, first to overcome the inertia of the steel, and secondly, to cut the rock; and they also relied on striking 1,500 to 2,000 blows per minute. Here the rock wanted more cutting, and, owing to the low pressure there was no velocity on the hammer, thus they could not bore well; the number of blows per minute was reduced also. Then all the other drill operators, except those connected with the Gordon drill, were troubled with the supply of

suitable hollow steel, and even the latter had some trouble, I believe. G. Leyner at first had to use welded bits that were always breaking, and the steel sent from America for his small machine and also for the Hardsog Wonder machine, though reported to be satisfactory there, was almost useless here. Both these machines are hung up at present, partly for this cause, though both makers would, I think, have to lengthen the stroke on their machines before they could be adapted for use here. Then no machine tried, except the water Leyner, had any suitable water feed attachment for use with hollow steel, and here a hammer drill is useless without water being led through hollow steel to the cutting edge of the drill bit. Fig. IV. is a

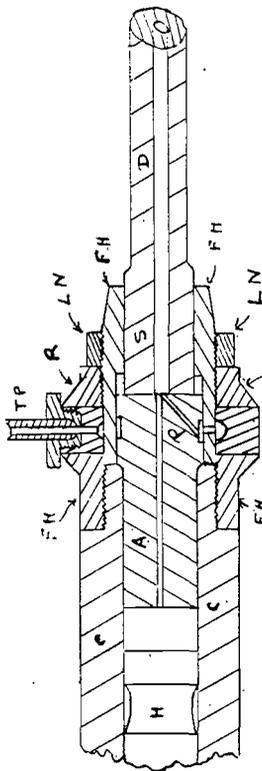


FIG. IV.—GORDON DRILL.

sketch showing the arrangement of the front head, water feed device and anvil block of the Gordon drill. C is the cylinder wall, H is the striking hammer, A is the anvil block, FH is the front head in one piece; this latter has to take up and resist the blows of the hammer should the steel not do so. WR is a hollow water ring secured by the ring R and the lock nut LN. S is the shank, and O the stem of the hollow drill bit. Air is exhausted through A, and meets the water coming from the ring along the hole P, and carries it down the hollow steel. The designers of the Gordon drill were in a

position to understand the bearing these failures had on the solution of the problem. They decided to use a stroke of 4 in., and a piston weighing 2 lb., and to strike a heavier blow, relying more on the mass than on the velocity (which after all must be increased four times to double the kinetic energy) for the cutting force of the blow. Without an anvil block such a blow might have given trouble in burring up the steel, and in their system of water feed an anvil block is a necessity, hence an anvil block is used. Heavy blows are, however, very much severer on the steel, and on the anvil block and on the machine itself, especially in hard ground. It is claimed that suitable steel to withstand these stresses has been procured. The makers of the Murphy drill were also not discouraged by their first failure, and they claim also to have produced a water feed machine of simpler construction than the Gordon drill, and one that will bore twice as rapidly as their former machine did at 60 lb. pressure. Some of these drills are due to arrive shortly, and other American makers and also an English maker are also adapting machines for this market, so that we may be sure of getting a variety of types. When I speak of boring at 40 or 50 lb., I quite recognise that it does not pay to work any machine either of hammer or piston type at this pressure, and I believe that air pressures will have to be improved all round if any type is to be a permanent success.

In turning to a comparison of hammer and piston drills, we must now ask why the hammer drill beats the 2 1/4 in. piston drill stopping in America with air pressures of 90 or 100 lb. In Cripple Creek, the rock is mostly fairly hard granite intersected by, I think, phonolite dykes. This rock cuts better than our quartzites and conglomerates. A miner experienced there told me that the general difference of gauge between piston drill steel was nearer 1/8 in. than the 1/4 in. used in hard ground here. All holes drilled in stopes there are almost vertical up holes. The law does not bother about dust production; the miners manage to wear respirators, or die. Then, even at 100 lb. pressure, the hammer drill does not bore as fast as the piston drill; but owing to the introduction of the air feed as shown in drawing (Fig. III.) no arm bar or clamp is required for these 2 in. diameter hammer drills. No time is lost in rigging up, as the tail of the air feed is placed on a board in the stope filling, and drilling is started within 15 minutes of entering the stope. It is well known that only about 50 per cent. of actual working time is employed in boring with piston drills, the rest being taken up in rigging and adjusting machines, and in changing drill bits, etc. In boring under these conditions with ham-

mer drills most of this time is saved, hence better work is done. In an eight-hour shift, 40 ft. is bored by a 2 in. Murphy drill, against 25 ft. with a 2½ in. piston drill, even though they do not bore as rapidly when actually running. Repairs are cheaper than with piston drills, working out at about half. The air consumption is less per foot drilled, though not much less per shift, as the hammer drill is, as before pointed out, working more continuously. Solid steel is used in these up holes, hence in this class of work the cost of drill steel runs about the same as with piston drills.

What are the conditions here? Are they equally favourable, and are they such that we can expect the same advantages from hammer drills? Here the rock is harder to cut, and heavier blows must be delivered on hollow steel, which is itself more expensive in first costs—cost of upkeep, and cost of sharpening, than the solid steel used in most piston machines. No welding can, I think, be permitted. The hammer drill cannot be used with the air feed alone, as all stopes are inclined, though it might be possible to work steep stopes at Randfontein and elsewhere with sand filling on this method; but it is doubtful if the water feeds would not give trouble. Here then, as in a piston machine, a bar with some kind of arm and clamp must be employed, hence not much time is saved here in this respect and not much time is saved in changing bits. A water feed with its great advantage and corresponding expense must be used, and nearly all the holes will be bored a few degrees on each side of the horizontal.

As far as I can learn from others, and from experience in mining with one of the smaller types of hand hammer drills, some of the disadvantages of this type are that, as the holes drilled have very little clearance, great care must be taken in alignment, or the hole will get out of truth and be lost. Even in boring vertical and up holes the drills are liable to stick, and in America several patent and special tools are employed to release such drills. Here in boring down holes any temporary stoppage of the water, due to grit choking cocks, or passages, is almost sure to result in a stopped drill. The same trouble is caused by a soft drill burring up on the ends, choking the central hollow core, and wedging itself by burring up its ends. The chief troubles, met with so far, have been owing to breakages, and this, of course, may be got over by improved design and material. The types of big Leyner drill, however, at work here, cost £6 to £7 per month for repairs. The advantages proved for the hammer drill, with water feed to date, are a very fair boring speed in ground of moderate hardness, capability to be handled by one operator, the power of drilling at the same rate of speed in any directions, power to work in small stopes, and

absence of dust trouble. Pillars can be more easily cut out and stopes run more parallel to the levels, thus allowing rock to be shot always towards box holes. I am certain that this type of rock borer with its few wearing parts is going to be largely used here, and I am pleased that the advantages of passing water to the cutting edge of the drill bit down hollow steel are likely to be demonstrated. The Leyner people in America, claim to be beating 3¼ piston drills in footage bored per shift, and I am convinced that any success this machine and others may have had, is due to the aid thus given to boring progress. If this system can be successfully applied to piston operated machines, a great increase in efficiency will be at once shown. In attempting in my last paper to forecast the development of rock drilling on this field, I anticipated the rise of the hammer drill for short holes.

With regard to piston drills, I thought that the practice of using air expansively was scarcely likely to be in the long run economical. Since then, the drill known as the Konomax, the invention of Mr. Mauss, of the Brakpan Electric Works, has come into prominence. As its design, and the principles on which it works has never yet, I think, been brought before a technical society; I here give figures showing the general design (see Fig. V.). The piston consists of two

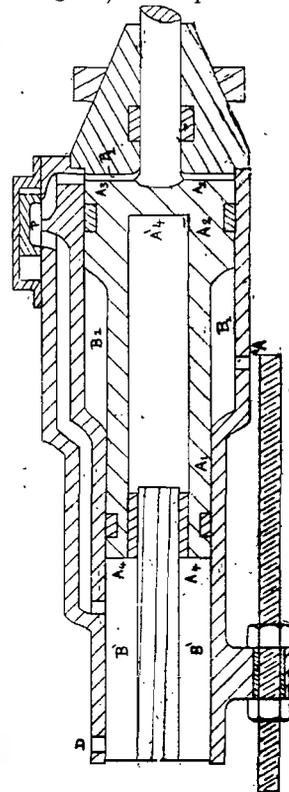


FIG. V.—KONOMAX DRILL.

portions, A_1 and A_2 , working in a corresponding cylinder. The effective area of the face A_3 being double that of the face A_4 . The effective area of A_1 is much greater than that of other piston drills of similar diameter, as passages are made past the rifle bar, so that the air pressure acts effectively on the face, A_1^4 . This cannot be done with ordinary types, hence this machine has a somewhat more powerful blow for the same piston diameter. The air space of the smaller cylinder, B_1 , is in constant communication with the air supply in the hose pipe through the inlet D. The supply of air to the fore part of the large cylinder air space B_2 is taken from B_1 , and is controlled by a piston spool valve P, worked by fluid air pressure to effect the following cycles. The piston being in the position shown, live air is admitted from B_1 to the front of the piston, and acting on the face A_3 (which is double the area of A_4) starts to drive this piston back against the lesser total pressure on the face A_4 . At a predetermined point in the back stroke, the valve cuts this supply off, and the air is allowed to expand. This point is generally at half stroke, giving an expansion of 2 to 1. As expansion occurs, the pressure on the face A_3 is diminished until it is only equal to that on A_4 . This pressure on A_4 is then able to overcome the momentum of the piston and bring it to rest. At this instant the air is exhausted from the front end of the piston by the movement of the valve, and the piston is driven forward, and makes a free stroke. Air is again supplied to A_3 , and the cycle repeated. Atmospheric pressure is maintained between the piston rings by means of an aperture A in the cylinder walls. This is in connection with the arrangements for moving the valve. The machine thus exhausts air once in the cycle and that upon the backward stroke, instead of twice as in the ordinary machine. The air is transferred from B_1 to drive the cylinder back. The pressure is always maintained behind the rear end and allows the valve P being placed right over the front end of the cylinder, thus making the exhaust part short and straight. There is thus no air lost in clearance spaces. The cold exhausted and expanded air having a direct exhaust, freezing up (which would otherwise occur) is obviated. The following is a statement of the air consumed by the Konomax drill and one of the usual type with, say, 3 in. dia. piston :

ORDINARY TYPE.

On Front Stroke.—Piston area = 7 sq. in.; stroke = 6 in., cub. measurement of air used 42 cub. in. *On Back Stroke.*—Piston area less $1\frac{1}{2}$ in. piston rod = $5\frac{1}{4}$ sq. in., stroke 6 in., cub. in. air used = $31\frac{1}{2}$. Area of valve ports and clearance $18 \times 1 \times \frac{1}{2}$ in. = 9, therefore air used per double stroke about 82 cub. in.

KONOMAX DRILL.

Area of A_3 (see Fig. V.) = 14 sq. in., stroke 6 in., cut off at half stroke = $14 \times 3 = 42$ cub. in.

This shows a theoretical saving of about 50 per cent. air, and tests on new machines are said to show this to be obtained. Owing to economy in the valve chest and air port weights, despite the difference of size of cylinder, the weight of the drill compares favourably with that of drills of the ordinary types. Its detractors and competitors question its power of rapid boring and state that any theoretical saving of air will be offset in practice by increased leakage. Increased leakage is bound to occur past the piston when worn, as compared with that of an ordinary machine. In such a machine each end is under pressure half its time and the total leakages surface is twice C where C = circumference of a 3 in. circle = 9.4 in. for the whole working time. In the Konomax drill B_1 is under pressure all working time = 9.4 in. leaking surface, and also the 6 in. diameter piston ring under pressure half its time = 9.4 in. Total 18.8, or twice that of an ordinary drill. This may not, however, prove very serious, or very greatly reduce the saving of air. This drill certainly presents great possibilities in air economies for piston rock drills and also for hammer drills. It has one great defect that must mitigate against its use in certain work, such as putting down long holes in shafts and winzes. The length of the stroke is determined by the gradual equalisation of two opposing forces, the air expanding on the front area and the constant pressure on the back. Any addition to the last force means that the stroke will be shortened and the drill steel be inclined to stick about this point. This must occur where the weight of long steel is added to the pressure on the back of the piston, or also in flat dry holes where there is great friction on the steel. Hence in such work the ordinary type of machine must more than hold its own. However, in general free boring, stope holes inclined below the horizontal, I think it proved that it bores about as rapidly as any drill at half air consumption, costing, say, 3s. to 4s. less per shift for power. Where I, however, think the great promise of the machine lies is in the case of those mines wallowing in the slough of low air pressures. I am pretty certain that the Konomax drill boring at 70 lb. pressure, in most cases, could beat any other drill boring at 60 lb. Take the case of such a mine throwing out all other machines and installing an equal number of this make, they should find the average air pressure improved to such a degree that efficiency in boring would be increased 20 per cent.

I have no interest whatever in this drill, and there still remains the doubt of its efficiency over long periods; but I think, if adopted in this

way, it will prove a success, provided that work and material are first class. If, however, it is introduced in twos and threes in large mines, it may fail, as the men prefer, I think, the old makes. What prospects does it hold out for a small stope drill? For working with long holes in 40 in. stopes I should certainly like to try a 2½ in. Konomax of regular pattern and would expect to save 2s. per shift in air consumption by its use.

Mr. Mauss has, however, adopted this principle to a most ingenious one man stope drill of 2 in. piston diameter. This machine is only in its trial stages, but is certainly the most novel attempt to solve the small stope problem I have met with. The valve is placed in the piston itself. The forward feed is by a hydraulic ram, and the waste water from this feed is driven down a hollow piston and hollow steel by a portion of the exhaust, to damp the cuttings in the hole. The rest of the exhaust and water form a spray from the front of the chuck. The machine is said to be incapable of knocking the front head and to work almost automatically. The air consumption is about half that of any other 2 in. machine, and there is, of course, no feed screw. Doubtless, difficulties will be met with in practice, but the whole idea is most ingenious and promising.

Fig. VI. shows the construction of the Temple-Ingersoll electric air drill, which I mentioned in a former paper. This is now, I believe, about to be introduced on the Rand. It appears to possess many striking features of simplicity and economy, and should be most useful for certain work here where mines have a supply of current available for underground work. Where drives can be run with one machine working one shift, and where ventilation is good it should prove economical, as it was only one-third of the power necessary for 3¼ in. air drill, and it might easily be taken into very flat stopes and used there.

The Bantam drill is of only 1½ in. diameter piston. In this also the valve is contained in the piston. The air is not worked expansively, and generally the design is the same as that of ordinary types. Its weight is about 56 lb.

The Kid drill, made by the Rand Drill Co., is at work on the Crown Deep. It is stated to be stopping ground for 5s. 9½d. per ton against 6s. per ton by hand labour. The plain slide valve moved by a tappet makes the valve action very positive and has several advantages. The wear on the guides is taken up by hammering in the malleable casting; but this does not seem an altogether satisfactory arrangement.

The Holman small drill has also a 2 in. cylinder. The designers have used the ball regulating valves as in the large machines, but have discarded the air

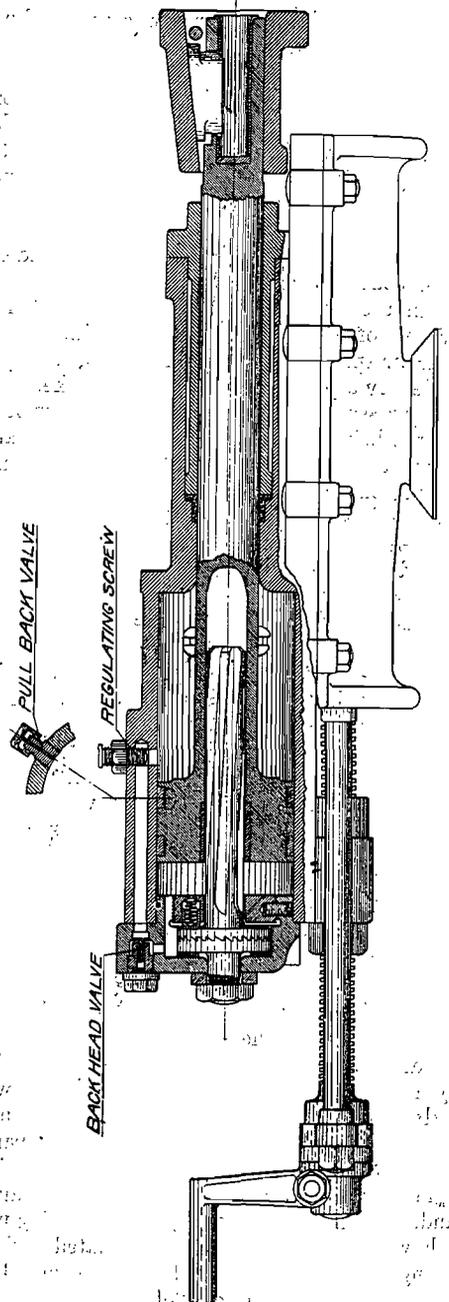


FIG. VI.—TEMPLE-INGERSOLL ELECTRIC AIR DRILL.

moved valve with the D slide combined, which I adversely criticised before, in favour of a spool piston valve like that on the Ingersoll machine. The chuck is said to be a much improved one, enabling unshanked steel to be used, allowing rapid changing and easy rebiting. The stroke is a long one for this size of drill, being 5 in. The weight of drill is 93 lb. and length of feed 17 in.

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The Little Wonder and Imperial drills made by Stephens & Co. are fluid moved piston valve drills of the usual type. The chuck used on these machines is claimed to be a great improvement on the ordinary U bolt chuck in cost of upkeep and rapidity of changing. The Ingersoll Company are not pushing the sale of their $2\frac{1}{4}$ in. Ingersoll-Sergeant machine, evidently pinning their faith on the Gordon drill.

The Imperial stoping drill is the best out and out attempt to construct a one man machine. It weighs only 75 lb. and has novel features deserving notice. The piston diameter is $1\frac{1}{2}$ in. and stroke 4 in. An attempt has been made to enable drilling to be performed with equal rapidity in all directions by passing air down hollow steel to the cutting face. The benefit of this in boring up holes is obvious, as generally the bit has to cut the rock over and over again. Whether there is a perceptible increase in the rate of boring wet holes is more doubtful. Mr. Stephens, the manufacturer, has had very good results in practice, and has obtained hollow steel that appears to stand the severe work in piston drills. Recently his $2\frac{1}{2}$ in. machine bored a flat hole in one-third of the time taken by a rival machine of the same diameter using solid steel. Undoubtedly it will do better and quicker all round boring work than any others at present at work, and if it will stand up to the work will, I think, be a success. The trouble with it is that a spray must be used with it when uppers are bored; and miners do not like sprays, as they do not allay all the dust, and the only water often available for them is foul water. Then the cost of upkeep of hollow steel, and of sharpening, is, of course, more than with solid steel, but a very small increase of boring rate will soon pay for that.

I still believe the real need of the Rand is for a small water feed piston drill, if these small drills are going to hold their own against the water feed hammer drills. From my remarks on hammer drills, it will be evident that I think they are largely the coming solution of the stoping problem, as undoubtedly, they should be easier to keep in repair, and cheaper to maintain, than most piston drills. The causes of failure in the past are, I think, largely removable and, provided with water feed and hollow steel, they should bore almost as rapidly as piston drills with solid steel and are easier one man machines than most piston drills.

I have now on order a number of sets of hollow steel, provided with water attachment for use with any make of piston drill, and these will be sufficient, should the quality of the hollow steel come up to expectations, to prove whether an ordinary machine can be got to drill with

increased speed in any direction. If these are a success, I should like to try them in a special machine designed for them.

As there would be no trouble with mudding any hole, up or down, I should like to try the effect of shortening the stroke to 2 or $2\frac{1}{2}$ in. using a larger cylinder diameter. The present long stroke on machines is adapted for low velocities of piston, and to allow of the steel kicking out the mud from the hole, and also to allow of the blunted steel, which designers know is often used, crushing the rock instead of cutting it. Of course, a drill bit must get more or less blunted; but by using mostly star bits with a water feed, and working with lengths of steel only a foot in difference, it should be possible to put down a 4 ft. hole all the way with sharp steel. The long stroke tends to loss of power, owing to vibrations on the machine, causing the drill bit to strike obliquely on the sides of the hole, thus wearing the corners off the bits and destroying some of the force of impact on the bottom of the hole. The striking energy of the blow depends more on the mass moved than on the velocity, as to double the energy developed we have to increase the velocity four times. Therefore, I should think, that such a drill with a short stroke, provided it was always supplied with sharp steel, should be able to combine some of the advantages of both hammer and piston drills. By shortening the stroke, a valve or valves might be arranged to allow direct exhaust of air at both ends, and very short air entrance ports. With air from 70 to 90 lb. pressure, the number of blows per minute should be run up well above 900, the rock being then cut by a number of lighter blows. Such a machine would not be of much use with pressures under 60 lb., and no use at all where any drill bits had to be used twice; but with high air pressures (which will surely be arranged for in all new mines by putting up compressors to work at 100 lb. or over, and by scientifically designing the underground pipe lines and putting them in under supervision with the best material and labour) it should make an ideal stoping drill, as a simple arrangement could be devised to make it easily controlled by one man. It would avoid the trouble of needing great care in alignment that hammer drills have, and no bits should stick, even if the water feed were cut off. The stroke being shorter, the edges of the steel would be less worn, and steels could be run on a smaller difference of gauge. Working with sharp steel, and a shorter stroke, there would be much less wear and tear, as it is pounding blunt steel on the bottom of a hole, that knocks piston machines out, apart from carelessness in starting holes. This is, of course, only my idea founded on, at present, an unproved

hypothesis, that a water feed can be successfully used in a piston drill for a long period under working conditions, and my reasoning might prove quite wrong in practice, and such a machine not bore at all rapidly.

Really, after all, we know but little of the science of rock cutting and boring here on the Rand. With all our boasted "brains to the square inch" we have worked disgracefully in the past and wasted power wholesale. Are we going to be content in the future to leave the solving of this problem to manufacturers who very likely have no proper data to go on themselves? We have thousands of horse-power applied to boring rock, and yet we are working very much in the dark regarding the true economy of the process. For instance, we know it cannot pay to use a drill bit after it has been blunted to a certain degree; yet thousands are used twice, and we really know nothing of the amount of loss in boring, say, a foot with blunt steel against a foot with sharp steel.

In mechanical engineering the exact angle of the cutting bit is calculated for different work in different material. Conditions are, of course, different in rock boring; but no experiments are made to find the best angle for cutting edges of drill bits, and what difference a cutting edge of 90° makes to one of 60° in hard and soft rock respectively. Taking into consideration the extra cost of sharpening, providing, and removing extra steel, time lost in changing bits, would it or would it not pay to run steels in big machines with a difference of 12 or 15 in. instead of 18 or 24 in.? In boring average reef, what is the best ratio to start with of foot pounds of energy per inch length of cutting edge for the blow given by a rock drill? What is the best relation between mass and velocity for the striking body? For instance, would not a 2½ in. machine drilling at 80 lb. pressure bore faster, even with the same sized bit, than a 3½ in. machine working at 60 lb.? All these and a host of other questions are, I think, worth answering. Some few experts and some manufacturers may know all about them; but I doubt if they do. Surely, it would be worth the industry's while to have a portion of a mine set apart for testing all these questions in our own rock. It would be easy to prepare a large syllabus of experiments worth trying, amongst others to find out what is the exact increase of boring speed with increased air pressures. The various types and makes of machines could then be tested by impartial investigators and their relative efficiencies under working conditions compared.

I have not dwelt much upon relative air consumptions, for it is evident that this is a relatively unimportant question, because even the saving of 50 per cent. of air in a 3½ in. drill means

only about 3s. to 4s. per shift, and, therefore, a drill with double the air consumption is a better drill to use, if it can break only, say, ½ ton more per shift, or, say, bore from 2 to 3 ft. more. The difference is, of course, less with 2 in. drills.

The same principle applies to cost of maintenance. How much more must a machine that costs £4 per month for maintenance bore than one that costs £2 per month? Obviously supposing rock broken costs 6s. per ton, and that a 4 ft. hole will break 1 ton, it has only to bore enough to break 7 tons extra per month, or an average of 6 in. per shift to pay for itself. Similarly, to pay for one extra labourer at 3s. per day it must break about ½ ton extra, or bore 2 ft. more per day. Therefore, it will pay better to use a small machine requiring two labourers, using twice as much air, and costing twice as much in maintenance as another, if it will bore 4 to 5 ft. extra per shift. If the cost per ton broken is higher than this, it will need to bore less than this. I have not taken into consideration questions of capital outlay for air compressors in this discussion.

We see, therefore, that our real need is for the quickest boring tool procurable and the one that will bore the most footage in a given time will justify its existence in the long run. I think the most economical known way of breaking a 36 to 42 in. stope is by using 2¾ in. machines, boring 6 ft. holes and working with three machines and seven skilled natives under one white man, where conditions are favourable and air pressures 60 lb. and over, and with machines all working on the same face, otherwise two machines per man must be used.

The hammer drill for work in narrow stopes has, I think, come to stay. It must have, however, a water feed causing extra expense for piping and hollow steel against a lesser cost of upkeep. Whether any type will finally displace the small piston drill boring with solid or hollow steel I do not like to predict. If hollow steel is coming in for piston drills, star bits will have to be made from the solid, and I am certain this always pays in hard ground. Hence, drill sharpening machines, like the local Kimber, or that made by Word Bros., of San Francisco, which can make machine steel as well as sharpen it, must come into use more largely.

I am more and more convinced that the question of working air pressures is a vital one. I am at present employing 90 lb. pressure with 3½ in. Ingersoll drills in shaft sinking in quartzites as hard or harder than any reef on the fields. Three 8 ft. holes are easily bored in two hours. Maintenance costs do not reach £5 per month. This shows that in stoping each machine at present on the Rand

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should under such air pressures bore easily 6 or 8 holes 8 ft. long per shift. One white man per machine might then be employed and costs reduced greatly. The record driving of 225 ft. per month, recently done by Mr. F. Girdler Brown, at the Cinderella Deep, was accomplished with air at 80 to 90 lb. pressure at the face.

The President: Machine drills have been so much to the front of late that this paper of Mr. Weston's appears at a very opportune moment. After hearing him to-night I think you will agree with me that the subject could not have been dealt with more comprehensively. The paper, I am sure, will in the circumstances lead to a very lively discussion; there is much debatable matter in it. Mr. Weston says that we want the quickest drill procurable. There is a great deal in that, and it brings in the question of the efficiency of drills—of which we have heard rather much of late. It may be said that there are two kinds of efficiency—efficiency from the mechanical engineer's point of view, and efficiency from the miner's point of view. Now these two points of view are absolutely different. The mechanical engineer talks of a drill being efficient when the drill uses little air, and the miner believes a drill to be efficient when it has this combined with many other qualities. As a matter of fact, the miner's definition of an efficient drill is "a drill which does its work at the smallest cost, everything included," and I think that definition was running in Mr. Weston's mind when writing his paper. A miner may prefer a drill which consumes quite a lot of air if, as Mr. Weston says, it does more work, and therefore more than pays for the slight excess of air. I hope those of you who deal with drills will come forward in the discussion. It is certainly a matter of satisfaction that there are quite a number of new drills on the market at the present moment, but it is too early to express anything like a definite opinion regarding their merits as practical mining machines; however, I hope that out of the number we have before us one or two will be found to be improvements.

A FEW NOTES ON THE REFINING OF BASE BULLION.

(Read at May Meeting, 1907.)

By C. W. LEE and W. O. BRUNTON (Members).

REPLY TO DISCUSSION.

Messrs. Lee and Brunton: Referring to Mr. Johnson's query, as to whether we found that the silver passed into the slag disproportionately to the gold, we have made the following assays, and found that in the case of bringing 670 oz. of

bullion, assaying 761 fine up to a fineness of 802, that 1.529 oz. of gold and 2.46 oz. of silver was contained in the slag, and that quite 60 per cent. of this bullion was in a metallic state, having been unavoidably removed in the process of skimming.

Mr. Lane Carter's remarks, on the fineness of the bullion on the Western Rand section, bear out our own experience; on the Windsor the mill gold used to assay about 925 fineness as compared with the Eastern Central section about 890, and still further east about 870. Our difficulty of base cyanide bullion has disappeared as mysteriously as it came, the bullion averaging between 880 and 890 fine. We may add that no change in the treatment during the clean-up or the smelting has taken place, so that the origin of base bullion is still undetermined.

In reply to Dr. Kirke Rose, we find that the wear of the $\frac{1}{8}$ bore clay tubes is very slight, but that they occasionally open out at the joint of the two sections.

We regret that for Prof. Wilkinson's information we had not an anemometer attached to the air supply, nor were able to note varying temperatures with the pyrometer, but must remind him that from the moment the bullion is received from the cyanide department, until the actual fine gold contents and bar are handed to the secretary, the assayer has regrettably not even time for the refreshments so urgently required, much less for taking scientific observations or working out formulæ.

NOTES ON WAIHI ORE TREATMENT.

(Read at July Meeting, 1907.)

By RALPH STOKES (Member).

DISCUSSION.

Mr. F. F. Alexander: Mr. Ralph Stokes' contribution on ore treatment at the Waihi mines of New Zealand opens up at least one point of considerable interest to us at the present; namely, the Barry honeycomb liner for tube mills; this method of lining a tube mill has not been tried here, as far as I know, although a complete set of frames are now ready, charged with broken silex, flint pebbles, and a mixture of Transvaal cement and sand as a setting; we hope shortly to have this set running in one of the mills on the Crown Deep.

I cannot agree with the author that there is any parallel between his description of tube milling at the Waihi and common practice on the Rand, for in referring to the Davidsen type of mill,

of which we have several at work, he gives so little detail that criticism becomes most difficult from a comparative point of view. If, for example, only 80 tons per day of 24 hours is passed through the mill the comparison becomes more odious, as against our 240 to 300 tons of a coarser material. I conclude from the mention of spitzkasten underflow, that the Waihi mine is wet crushing, as with dry crushing 6 tons per hour to about 1 per cent. + 60 (material used for cement), the life of a 2½ in. silix liner in constant use night and day is about two years. I mention this fact to show that what is quite suitable for one class of ore may not meet with success everywhere, and I am of opinion that a cast iron Barry honeycomb liner will not run 200 days on the Crown Deep, but may do so, say, at Roodepoort. I have often noticed when examining a well worn mill how the iron wedges (used for keying a last course) protect the adjacent flint blocks, and this led me to try several courses of grizzly bars longitudinally through the mill. The 5 in. grizzly bars wore away practically as fast as the silix blocks, and after a second trial I came to the conclusion that they were a failure; the honeycomb set I have on the mine is 15½ in. square inside, 6 in. deep, in four compartments, and 1 in. to 7⁄8 in thick, and, filled with silix concrete, will weigh a little over 19 tons without the ends, which, by the way, we shall not attempt, owing to the number of bolt holes tending to weaken the ends of the mill.

Had Mr. Stokes given more detail in connection with this part of his paper, such as internal diameter of mill, ratio of water by weight to solids fed, consumption of pebbles per 24 hours, rise of temperature, if any, and so on, then one could judge whether particularly good work is being done, as, for instance, the tables of grading on these fields compare somewhat as follows:—

Waihi decreases + 60 from 31.03 tons to	1.28 = 29.75 = 95.87 per cent.
Crown Deep decreases + 60 from 67.87 tons to	8.00 = 59.87 = 88.21 per cent.
Waihi increases - 60 from 68.97 tons to	98.72 = 29.75 = 43.13 per cent.
Crown Deep increases - 60 from 32.13 tons to	92.00 = 59.87 = 186.33 per cent.

It is clearly seen from the above figures that if the Waihi ore, as Mr. Stokes says, is comparable with our banket deposits there is considerable room for improvement.

Mr. Stokes' description of the vacuum filtering of slimes is most interesting, and I am sure our members would welcome a carefully written paper on this method of slime treatment, as the simplicity of the apparatus would, I feel sure, recommend itself to small concerns with high slime values.

NOTES ON THE ESTIMATION OF CAUSTIC LIME.

(Read at August Meeting, 1907.)

By EDW. H. CROGHAN (Member).

DISCUSSION.

Mr. W. Taylor: Before making any remarks on this paper, from the seller's point of view, I wish to thank him for the thorough manner in which he has gone into the question of the value of the caustic lime as supplied by the various lime companies to the mines. It had been thought, in the absence of any satisfactory explanation, that when the majority of the mines elected to analyse the lime by the sugar method, though the gravimetric method invariably gave much higher results, that an unfair advantage was being taken, which the lime companies, owing to excessive competition for business, were unable to resent. Mr. Croghan's exhaustive analysis now shows us that practically the same results can be reached by the two, or including the acid method, three different methods.

But in spite of the fact that these minute investigations dispose of any doubts as to the accuracy of the method in vogue for estimating the commercial value of the lime, I still think the sellers have a grievance, and a very distinct grievance, which was referred to by Mr. McArthur Johnston in his remarks at the last meeting, that is, the manner in which the sample is taken. He says, the best method is to grind 10 bags from each truck; a most excellent scheme, but one which it is extremely improbable any mine would systematically carry out.

Let me suggest a less ambitious scheme, one that has proved satisfactory in practice and occupies a very short time to carry out. On one occasion, feeling very sore at continued deductions being made, I went out to a mine and with the assayer took at random one bag of lime from a truck which had just arrived. This was thrown on to a ½ in. mesh screen, the larger pieces crushed in the sample rock breaker until all passed through, and two cross sections were taken with a shovel giving enough to fill a bucket; this amount again was crushed by hand to pass a 900 mesh screen, and then quartered down until sufficient was left to fill an ordinary assay sample tin. The whole operation took something like half-an-hour, and the final analysis proved satisfactory to all parties. If such a system of sampling were carried out thoroughly and the sample immediately transferred to a small glass jar, the cover of which could be screwed down

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tight on to a caoutchouc ring, thereby ensuring complete protection from the air," to quote Mr. Croghan, there should not be the curious differences between the value of the caustic lime; as shown by the analysis and actual consumption. For instance, if a truck of lime analysing 80 per cent. CaO will keep the solutions in the desired state of alkalinity for ten days, a truck analysing 40 per cent. should surely last only five. In point of fact, further supplies are not required a moment sooner; from which must be deduced, that either great latitude is allowed in the degree of alkalinity to be maintained, or the samples are not taken with that considerate care for the pockets of the lime burners, that our cyanide managers would have us believe.

It is a well-known fact that lime absorbs moisture very rapidly, but from a commercial point of view, this seems to be ignored, the blame for inferior lime during the wet season being put entirely on the lime companies. I find, for instance, from December, 1905, to April, 1906, the lime supplied by my company averaged according to returns from several mines, 68 per cent., whereas from May to September, 1906, the analysis showed 76 per cent. caustic lime. I mention these figures to show that even supposing the samples are promptly and accurately taken, and the analysis carefully carried out, it does not necessarily prove that the lime produced by any particular company is either better or worse than any other lime, unless such conditions, as the state of the weather, the time that has elapsed between the burning and sample taking, etc., are taken into consideration.

I must apologise for travelling rather far from the points raised in Mr. Croghan's paper, but am inclined to the view that as in assaying for gold, so in analysing for caustic lime, all parties are at the mercy of the sampler.

Prof. G. H. Stanley: I think Mr. Croghan deserves the thanks of this Society for giving us the benefit of his work on this question. His paper calls less for criticism than mere comment, there being little room for the former.

I differ, however, with his definition of the term available lime, as I think it would be preferable to understand by the term that lime which is soluble in water and therefore available for precipitating slimes, etc., *i.e.*, that which is available and not that which might become available. The carbonate, of course, is not available in this sense, only the free or caustic lime being included, the user caring nothing for insoluble compounds. It seems to me, therefore, that "available lime" or "free lime" would be the most suitable term to employ.

I should like to draw your attention to a possible cause of the discrepancies in results after allowing for every possible lime compound which could be formed under the circumstances. I think the explanation, at least in some cases, say, 1, 2, 3, 4 and 7, lies in the determination of soluble silica. Some years ago I had a number of cements and mortars for analysis, soluble silica being one of the determinations I had to make, and in the course of my analyses I found that the percentage of silica soluble in acid depended to a considerable extent on the strength and temperature of the acid employed. After this experience I looked the matter up and found that this behaviour of the silica was well known, and consequently a certain strength of acid—I believe cold 10 per cent. hydrochloric—was recommended as a standard for sole use in this determination in order that results by different analysts, or of different analyses by the same operator, might be comparable. Free lime was determined by the ordinary gravimetric method as outlined by Mr. Croghan, after calculation. These variable soluble silica results were due to some of the silica from the decomposed silicates remaining insoluble, although the lime with which it had been combined went into solution and formed part of the total lime determined. I see the same fact referred to at the beginning of the paper, and it seems possible that in his analyses the same thing occurred, and that therefore his percentage of soluble silica obtained does not represent the full amount of silica which ought to be calculated to silicate and subtracted from total lime, thus leaving the free lime result too high.

No query can be raised as to the propriety of calculating CO_2 and SO_3 to calcium carbonate and sulphate, and it seems probable that the most basic silicate of the series would also be formed. The figures presented certainly lend colour to the view that an aluminate is also formed, though whether the formula given is correct appears to be a moot point—it is certainly a curious coincidence that three out of four samples, *i.e.*, 5, 6 and 9, which give the highest comparative results by the sugar method should also be the highest in alumina. I may point out in this connection that the words "high" and "low" in the third table on p. 39 appear to be transposed, and that the printer appears to have made another slip with reference to the relative insolubility of magnesia on p. 41, and that lastly the author is apparently responsible for a slip on p. 37 in stating that manganese carbonate becomes converted to MnO_2 ; should it not be Mn_2O_4 ?

Dr. J. Moir: One of the benefits which will have accrued from this paper is, I imagine, the illumination thrown on one of the dark places of

analytical chemistry. To those of us here who have, so to speak, "seen the light" regarding the problem involved in determining caustic alkalinity available for protecting cyanide, it seems positively marvellous that a process so unscientific as the differential gravimetric one should ever have found anyone to believe in it. It is, therefore, the more satisfactory to find that Mr. Croghan has worked out the cause of the error—namely, the combination, during burning, of the lime with most of the impurities present in ordinary limestone.

I have my doubts, however, about his calcium aluminate—not that the formation of $\text{Ca}_3\text{Al}_2\text{O}_6$ in lime-burning is unlikely, but because I think the supposition is an unnecessary one for the present, inasmuch as the distinction which he makes between quartz and soluble silica is perhaps in reality not so definite as he makes it out to be. What I mean is that the quantity of soluble silica obtainable from a particular specimen of lime is liable to variation according to the strength of acid or soda employed and the temperature and time of extraction; consequently its true value may be rather higher than that given by Mr. Croghan in each case, and the calculation of the gravimetric figures may "square" with the direct methods without assuming lime to be bound up as aluminate. Of course, if the author did succeed in dissolving every possible trace of silica, then I am wrong, and some such supposition as that of the existence of $\text{Ca}_3\text{Al}_2\text{O}_6$ becomes necessary; but, for the present, the small discrepancy between theory and experiment, on account of which he makes this supposition, seems to me to be more likely due to the accumulation of experimental errors than to the real presence of $\text{Ca}_3\text{Al}_2\text{O}_6$ in the original lime.

My chief reason for addressing you, however, is to call your attention to the fact that in this matter of the estimation of caustic alkalinity we seem to have got ahead of the rest of the world by a few years. I am led to this conclusion by reading a paper on this subject published in *The Analyst* for September, 1907. The author is Mr. Jas. Hendrick, F.I.C., B.Sc. (lecturer in Agricultural Chemistry at Aberdeen University), and his paper is practically a rediscovery of the application of the sugar method brought to light here by Mr. Crosse a number of years ago, and in daily use in many Transvaal laboratories. The preamble of Mr. Hendrick's paper is practically the same as that of Mr. Croghan:—

"Methods of estimating rapidly and accurately the caustic lime in burnt limes and other substances are not given in the ordinary text-books, and do not appear to be in common use. In the past, analyses of limes have generally been 'complete' analyses, in which the caustic lime has

been determined by estimating the total lime and certain other constituents of the sample, and assuming that the lime which is left after subtracting from the total what has been calculated to be present in certain compounds is caustic lime. This indirect method is liable to great inaccuracy. For instance, a considerable amount of lime is sometimes present as silicate in burnt limes. It appears to be common to include such lime in the caustic lime. It is probable that estimations of caustic lime in this way are generally in excess of the truth, especially in the case of impure limes prepared from limestones which contain a considerable percentage of fine silicious matter. In any case, such a complete analysis is usually not wanted, and is time-consuming and troublesome." "The amount of calcium oxide present in a mixture of the oxide with calcium carbonate can be estimated by direct titration with standard hydrochloric or nitric acid, using phenolphthalein or phenacetolin as indicator (Sutton, *Volumetric Analysis*, 8th edition, p. 75). In the case of commercial limes this method is rendered only approximately accurate by the presence of magnesia, of phosphates, and of iron and aluminium oxides, which render the end-point indefinite. With experience, an approximate estimation can be made in this way which is probably as accurate as those made by the complete analysis method, and, like it, errs on the side of excess. This method has been used to a large extent in comparison with the one described.

TABLE I.

Caustic Lime.		Total Lime.	Total Magnesia.	Total Silica.
By Direct Titration.	By Sugar Extraction.			
Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.
49·5	48·8	—	—	—
57·5	54·7	71·9	1·6	14·8
67·2	66·0	76·5	5·5	5·5
75·5	75·5	—	—	—
79·1	78·3	—	—	—
84·9	82·4	88·0	5·1	2·4
86·8	85·2	90·1	2·2	1·2

Methods in which solutions of ammonium nitrate are used to extract caustic lime are employed to some extent on the Continent. G. Berju and W. Kosinenko (*Landw. Versuchstationen*, 1904, 60, 419) investigated these, and suggested some improvements. One great defect is that a solution of ammonium nitrate dissolves calcium carbonate to a certain extent. Another is that the lime cannot be estimated volumetrically directly after extraction, but must be precipitated as oxalate."

"It occurred to me that the lime might be extracted by a solution of ordinary sugar, and

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directly titrated in such a solution by standard acid. After I had commenced work on these lines it was pointed out to me that a method similar in principle had already been published by W. E. Stone and F. C. Scheuch (*Journal of the American Chemical Society*, 1894, 16, 721; *the Analyst*, 1895, 20, 19). I have, however, as the result of my experiments, made certain alterations in the procedure of Stone and Scheuch which materially enhance the value of the process. "In my experiments, with pure lime and carbonate of lime, Iceland spar, and what was purchased as pure precipitated carbonate of lime, were used. The Iceland spar was practically pure calcium carbonate, but the precipitated carbonate, after drying contained only 98.9 per cent. of calcium carbonate. Lime and mixtures of lime and carbonate were prepared by heating these substances over the blowpipe. The amount of caustic lime present was estimated from the loss of weight after ignition, and by complete titration with standard acid and methyl orange.

"The solubility of calcium oxide in presence of calcium carbonate was studied. For each gram of the original carbonate of lime taken, 100 c.c. of a 10 per cent. solution of sugar was used. After the carbonate had been ignited, it was shaken with the sugar solution for at least four hours in a Wagner rotary shaker. It was then rapidly filtered and titrated with standard acid. The results are shown in Table II.

TABLE II.

(a) *Iceland Spar.*

Calculated from Loss of Weight.		Found by Titration of Sugar Solution.
Calcium Carbonate.	Calcium Oxide.	Calcium Oxide.
Gram.	Gram.	Gram.
1.000	0	Mere trace.
0.777	0.189	0.187
0.301	0.451	0.488
0.020	0.549	0.547
0.010	0.555	0.557
<i>(b) Precipitated Calcium Carbonate.</i>		
1.000	0	Mere trace.
0.534	0.341	0.325
0.044	0.535	0.517

In the case of Iceland spar, the agreement between the estimated and found calcium oxide is as close as could be expected. In all caustic lime estimations, unless special precautions are adopted to exclude carbon dioxide, there is a small loss due to the formation of carbonate. For the purpose in view, such minute losses are of no consequence, as they will in any case be less than those which are liable to occur in the commercial handling of the lime.

With precipitated lime the agreement is not so good, differences occurring which are by no means negligible. It was found that the 'pure' precipitated lime contained about 1 per cent. of silica, which, on ignition, enters into combination with a part of the lime, and prevents its solution in the sugar. This helps to explain what takes place in the case of commercial limes, which often contain considerable percentages of silica.

"*Strength of Sugar Solution.*—The effect of the concentration of the sugar solution was investigated. For this purpose a high grade commercial line, of which a complete analysis had been made, was used. In each case 5 gm. of lime and 500 c.c. of the sugar solution were taken. The solutions were shaken side by side for four hours. The results are given in Table III.

TABLE III.

Strength of Sugar Solution.	100 c.c. required N. HCl.	Calcium Oxide.
5 per cent.	24.5 c.c.	0.6860
10 "	29.5 "	0.8260
20 "	29.6 "	0.8288
30 "	29.7 "	0.8316

The figures show that whilst 5 per cent. of sugar is too low a concentration, there is little difference in the results obtained from the stronger solutions.

"*Amount of Shaking Necessary.*—In the method described by Stone and Scheuch it is recommended that the lime be shaken with the sugar solution for 20 minutes. I have not found this sufficient for complete extraction. Experiments were made with two samples of commercial lime, one of high quality and the other of poor quality. These were finely powdered and shaken with a 10 per cent. sugar solution, 100 c.c. per gram of lime, for different lengths of time, as shown in Table IV.

TABLE IV.

Time of Shaking.	High Quality Lime.		Low Quality Lime.	
	100 c.c. required N. HCl.	Per cent. CaO.	100 c.c. required N. HCl.	Per cent. CaO.
	c.c.		c.c.	
$\frac{1}{2}$ hour	29.8	83.44	21.2	59.36
1 "	30.3	84.84	21.7	60.76
2 hours	30.8	86.24	22.0	61.60
4 "	31.0	86.80	22.3	62.44
6 "	31.0	86.80	22.3	62.44

The results show that about four hours' shaking is necessary. The shaking-machine was driven at about 50 or 60 revolutions a minute. If the limes are at all coarse and lumpy it has been found that four hours' shaking is not suffi-

cient. At the same time, no special precaution was taken to grind very finely, and they were certainly not ground to an impalpable powder.

It is most important that the shaking should be started immediately after the lime and sugar solutions are mixed. If they are allowed to stand the lime soon forms a cake of sucrate, and it is then almost impossible to obtain complete extraction. There were many erratic results obtained in some of our earlier experiments owing to this cause. . . . Even after four hours' shaking there was less lime extracted, if allowed to cake before shaking, than in half-an-hour when the shaking was commenced at once. If 2 c.c. of alcohol per gram of lime is placed in the flask before adding the lime and sugar solution the formation of a cake is prevented.

"*Effect of Magnesia.*—A sample of crystallised magnesium carbonate of the composition— MgO , 31.1 per cent.; CO_2 , 31.5 per cent.; and water, 37.1 per cent. was used. It was free from lime, sulphates, chlorides, and nitrates. Five grams of the finely powdered carbonate were shaken for four hours with 500 c.c. of a 10 per cent. sugar solution, and filtered; 100 c.c. required for neutralisation hydrochloric acid equal to 0.0866 gm. $MgCO_3$, equal to 0.0412 gm. MgO . The shaking was continued in another portion for six hours, but exactly the same amount of acid was required for neutralisation. Magnesium carbonate in this form appears to be dissolved to a considerable extent by the sugar solution. A portion of the carbonate ignited to oxide was shaken with a similar sugar solution for four hours. The sugar solution was used at the rate of 100 c.c. per gram of the original carbonate taken. It required for neutralisation only 0.3 c.c. $N. HCl$ per 100 c.c., equal to 0.0059 gm. MgO , or 0.0123 gm. $MgCO_3$. The oxide, therefore, is almost insoluble in sugar solution. A similar result was found by Stone and Scheuch.

It is generally assumed that all the magnesia in burnt limes is present in the form of oxide. If this be so, the small percentage present in ordinary lime will not affect the result to any appreciable extent."

"This method, therefore, furnishes a rapid means of estimating caustic lime in such substances as burnt limes, which contain carbonate of lime, magnesia, and other substances. It is more accurate than the method of direct titration, which is interfered with by the presence of magnesia and carbonate, and gives high results, and is also more accurate, as well as much less troublesome, than the method of complete analysis.

The process which I use in carrying out the sugar method for ordinary burnt limes is as follows:—

In a 500 c.c. flask is placed 10 c.c. of alcohol. Five grams of the powdered lime are rapidly weighed and added. The flask is filled to the mark with 10 per cent. sugar solution, and at once placed on the shaker, and shaken for at least four hours. A portion is then rapidly filtered into a 100 c.c. flask, and titrated with standard hydrochloric acid, using methyl orange as indicator. When the residue readily settles clear after the shaking, as is often the case, the solution can be poured off directly into the 100 c.c. flask." (See Table I. for results.)

"It will be seen that in every case but one the direct titration gives higher results than extraction by sugar. In four of these limes complete analyses were made, and the total lime, magnesia, and silica found are also given in the table for comparison.

When the caustic lime was calculated from the complete analysis, it was always found to be higher than that given by the sugar method, and generally also higher than that found by direct titration. In the case of low-quality limes which contain a considerable percentage of silica, it is not unusual to find that the caustic lime estimated from the complete analysis is from 5 to 10 per cent. higher than that found by the sugar method, unless it is assumed that a considerable part of the lime was combined with silica and this amount deducted."

This is, no doubt, a case of great minds thinking alike; it is certainly a curious coincidence at 6,000 miles distance. Since the two authors agree so closely, I shall only make a few remarks on the points where they disagree. The chief one is the strength of the sugar solution required, which Mr. Croghan puts at only 2 per cent., whereas Mr. Hendrick finds a large error when he uses a solution weaker than 10 per cent. This point can only be settled by experiment, but in the meantime I wish to point out that the ratio $CaO : C_{12}H_{22}O_{11}$, namely, that required to give the particular calcium sucrate which is freely soluble in cold water, is 1 : 6.1; consequently 5 gm. of pure lime require at least 610 c.c. of 5 per cent. sugar solution for complete reaction; so that Mr. Hendrick, in using only 500 c.c., did not apparently give the 5 per cent. concentration quite a fair trial. Though Mr. Croghan used only a 2 per cent. solution of sugar, yet by using a large quantity of it, he employed an actual ratio of 10 parts of sugar to 1 of lime, and hence probably, if he shook long enough, he got a complete enough reaction. It is also possible, of course, that the two experimenters used limes of very different solubility or fineness of grinding—anyway, more experiment is needed.

The same criticism, viz., use of too small a weight of total sugar applies to Mr. Hendrick's

experiments made in regard to time of shaking required.

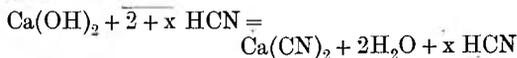
The other point is that whilst Mr. Hendrick filters the mixture and uses methyl orange as indicator, Mr. Croghan titrates direct with phenolphthalein. The latter process is more correct theoretically, as I shall show later on, and has in addition the advantage of making no exposure of the mixture to the carbonic acid in the laboratory air. It has, however, the drawback that the end point is unsatisfactory, and indeed hard to see at all in the case of coloured limes. Most analysts, however, do not use enough phenolphthalein in this case to give themselves a fair chance of seeing the end point. In this connection I am inclined to think that Mr. Croghan, in adopting the process of direct titration, has been lucky. I mean that by titrating in presence of the undissolved substance he compensates for the error which he has made (according to Mr. Hendrick's work) of working with a too dilute sugar solution. In fact, what has happened is that Mr. Croghan's actual process is a combination of the "sugar" and the "acid" methods; for even if he failed to convert all the CaO into calcium succrate, yet on titrating, the end point he obtained from the unfiltered mixture in presence of acid and phenolphthalein was the true end point due to total caustic alkalinity.

Finally, I should like to make a suggestion in regard to what I believe is the only scientific way of evaluating lime as a protector of cyanide. This suggestion is, briefly, to make hydrocyanic acid itself the testing reagent. All that would be necessary would be to treat a weighed quantity of the lime with excess of pure aqueous HCN and then titrate the cyanide formed with silver nitrate and KI in the usual manner. I make this suggestion because I am convinced that not even the "sugar" or the "acid" method is absolutely correct. In the first place, the sugar method takes no account of magnesium oxide—which certainly has a slight protective action on cyanide. The argument from its insolubility is not worth much, because (1) the solubility is as much as 1 oz. per ton, and (2) if the dissolved part is being continuously used up, the undissolved part will gradually go into solution. For example, MgO is easily capable of reddening phenolphthalein (which is an acid of about the same "avidity" as HCN). Even calcium carbonate is not absolutely inactive as "protective alkali," though I believe it can be almost neglected in practice. It is certainly a curious accident that the indicator phenolphthalein should have originally been adopted for the "sugar" or "acid" method, since I find on investigation of the literature, that it is the correct indicator, namely, the one which most closely approaches HCN in avidity. The

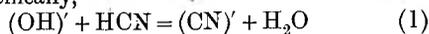
dissociation-constant of HCN is about $13 \div 10^{10}$ (Walker & Cormack, *Trans. Chem. Soc., London*, 1900, p. 5) whereas that of phenolphthalein is $8 \div 10^{10}$, that of paranitrophenol is $2.3 \div 10^7$, that of methyl orange is $4.6 \div 10^4$, that of rosolic acid is $1.1 \div 10^8$, that of alizarin is $8.8 \div 10^9$ and that of cyanine is $4.2 \div 10^6$, whilst dimethylaminoazobenzene is the only indicator with a weaker "avidity" than HCN, its dissociation-constant being $1.45 \div 10^{11}$ (E. Salm, *Zeitsch. f. Elektroch.*, 1906, pp. 99-101). Poirrier's blue is similar to the latter, but I am unable to find figures relating to it.

It will probably be said that the method I propose is too troublesome and unpleasant for technical use. Aqueous HCN is certainly a dangerous neighbour in a close laboratory, but it has often been used in research before without risk. For this special purpose it should be prepared by action of dilute H_2SO_4 on a 20 per cent. KCN solution, passing the gas over dry 100 per cent. KCN in a tower so as to remove traces of stronger acids (such as CO_2 or cyanic acid), and collected in ice-cold distilled water. It must be kept in the dark and should sometimes be tested with silver nitrate to make sure that it does not contain NH_4CN (which is usually developed on long keeping of HCN).

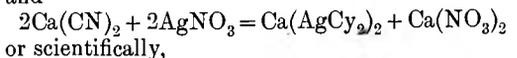
The equations involved in the process I propose are:—



or scientifically,



and



or scientifically,



Hence 1 c.c. decinormal silver used = 2 c.c. decinormal cyanidion $(CN)' = 2$ c.c. decinormal hydroxidion $(OH)' = 0.0034$ gm. hydroxidion. Of course, if, despite many protests, we must be old-fashioned and stick to a prehistoric and misleading nomenclature, then the result is that 1 c.c. $\frac{N}{10}$ silver nitrate corresponds to 0.0056 gm. of caustic alkaline substances calculated as CaO.

The meeting then closed.

Obituary.

The death is announced with deep regret of Mr. TOM FRANCIS, the Cyanide Manager of the Nellie Mine, Insizwe, Rhodesia.

He was elected a member on the 21st July, 1906.

* See also this number; abstracts, p. 128, under "A Study of Indicators."

Notices and Abstracts of Articles and Papers.

CHEMISTRY.

THE PHILOSOPHER'S STONE.*—"THE CHEMICAL ACTION OF RADIUM EMANATION, Part II." The authors have apparently discovered the "philosopher's stone" sought for by the ancients, which has the property of breaking up the "elements" into simpler bodies. The "stone" has turned out to be a gas, namely, radium-emanation, which has the atomic weight 216 and apparently belongs to the argon series. It was discovered last year that the emanation when left to itself breaks down, giving apparently lead and helium, and earlier in this year Ramsay showed that in presence of water it gives instead hydrogen and neon, without helium.

The present authors have now tried the effect of the emanation on solutions of copper salts and have obtained the marvellous result that lithium, sodium, and argon have been produced in small quantity from these materials. There is, of course, some doubt attaching to the creation of sodium since the experiments were partly done in glass vessels, but the creation of lithium from copper has now been done in four experiments, in one of which extreme precautions to avoid impurity were taken. Two other preliminary experiments have also given equally startling results, viz., (1) lead nitrate gave no lithium, but gave sodium and signs of two heavy metals other than lead; (2) thorium nitrate appears to give CO₂ as one of its degradation-products.

The following are some of the details of the careful experiment with copper nitrate: pure CuSO₄ was electrolysed on to a smooth rotating platinum cathode and the copper was washed with the purest water and dissolved in the purest HNO₃ and taken to dryness in platinum. This copper nitrate was dissolved and the solution divided into two similar bulbs, one for the experiment and the other for a "blank," the latter being simply exhausted and sealed; the other after exhausting received four small doses of radium-emanation in the course of a fortnight, amounting in all only to about 1.6 cub. mm. of the gas. The treated bulb became cloudy and finally green and turbid. Four weeks afterwards the experiment was stopped, the mixture frozen with liquid air and the new "copper-gas" pumped off. Analysis showed it to consist essentially of NO and argon with a little N₂, CO₂ and H₂ (origin doubtful); helium and neon were absent. The thawed liquid part was centrifugalised in a silica tube, but the greenish precipitate was mainly Cu(OH)NO₃. The green liquor was worked up with H₂S, and the residue on evaporation weighed 1.6 mg. and gave strong Li and Na lines with the spectroscope, and also traces of calcium. The CuS precipitate on working up showed signs of another metal—too small to identify.

The "blank" was worked up in the same way. Its residue weighed about 0.8 mg. and showed Na and traces of Ca but no Li. The "blank" CuS gave nothing.

The abstractor suggests, in view of these results obtained with the metals of the first group of the periodic table, that it is now quite a promising experiment to try to make gold from thallium, since both are at the head of the same group.—SIR W. RAMSAY and A. T. CAMERON, M.A., B.Sc.—*Journal of the Chemical Society, London*, Sept., 1907, p. 1593. (J. M.)

HYDROGEN PEROXIDE ON POTASSIUM CYANIDE.—"The author finds that (1) hydrogen peroxide and potassium cyanide react exothermically in aqueous solution at the ordinary temperature.

(2) The peroxide is reduced without escape of oxygen so long as residual cyanide is present, but any excess of peroxide is afterwards decomposed into water and oxygen.

(3) The products of the oxidation of the cyanide, which do not include oxamide or oxalate, are potassium cyanate and potassium and ammonium carbonates. Cyanate is the primary product and tends to be converted by hydrolysis into carbonate through the catalytic agency of the peroxide.

(4) A part of the cyanide is converted by hydrolysis, without oxidation, into potassium formate and ammonia.

(5) The total oxidation product (cyanate plus carbonate) accounts for about four-fifths of the original cyanide, and the hydrolysis product (formate) for about one-fifth.

(6) The curve representing the progress of the reduction of peroxide, so long as unchanged cyanide remains, is of the second order and accords with the equation $\frac{dx}{dt} = k(A-x)(B-x)$, where A is the initial

concentration of the peroxide, B that of the cyanide, and x the measure of the peroxide reduced, or cyanide oxidised (*i.e.*, cyanate formed) at time t . $B-x$ is the sum of the residual cyanide and its hydrolysed product (w).

(7) The hydrolysed product (w) reaches its maximum and final value ($W=0.2B$, nearly) before the completion of the oxidising action, and thereafter the slopes of the peroxide and cyanide curves are equal. The rate of hydrolysis is expressed by the empirical rule that $\frac{dw}{dt} = k_1(A-x)(W-w)$.

(8) If the solutions contain, at the outset, the ultimate products of the action itself, as well as fresh cyanide and peroxide, these products do not exert the influence indicated by the above equations. Hence some unknown intermediate condition probably occurs while the action is in progress.

(9) The addition of caustic potash or sulphuric acid at the outset greatly retards the whole action, but the addition of ammonia has very little effect.—ORME MASSON.—*Proceedings of Chemical Society*, April 29, 1907, p. 117. (A. MCA. J.)

A STUDY OF INDICATORS.—"A set of seventeen solutions whose hydrogen ion concentration varied progressively from 2-N (strong acid) to 10⁻¹⁶N (strong base) was prepared. For the concentration range, 2-N to 10⁻⁴N inclusive, hydrochloric acid solutions were used. The solutions of concentrations between 10⁻⁵ and 10⁻¹¹N inclusive were prepared by mixing solutions of the three sodium orthophosphates in the proper proportions. Solutions of potassium hydroxide were used for the rest of the series. The hydrogen ion concentration in each solution was determined by the measurement of the E. M. F. of the H+ concentration cell. The colour reaction of each solution with each of a large number of indicators was determined, and the results tabulated. The indicator solutions were prepared according to Glaser (*Indicatoren der Acidimeter und Alkalimeter, Wiesbaden, 1901*); the concentration being 0.1 to 0.15 gm. per 100 c.c. of solvent. 0.1 c.c. of the indicator solution was added to 10 c.c. of the solution to be tested. The colours were observed by transmitted light. For each adjacent pair of solutions, some indicator (frequently several) was found which gave a pronounced difference

* See this *Journal*, Sept., 1907, p. 88.

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in colour for the difference in H⁺-concentration existing between the two solutions. By the use of a colorimeter the author was able to determine this concentration accurately enough to calculate the dissociation constants of several weak acids. His results agree very well with Ostwald's values from the conductivity method. The dissociation constants of the following indicators were determined:—

Acids.		Alkalis.	
Methyl orange	4.6×10^{-4}	Cyanine	4.2×10^{-8}
Paranitrophenol	2.3×10^{-7}	Dimethylami-	
Rosolic acid	1.1×10^{-6}	noazobenzene	1.4×10^{-11}
Alizarin	8.8×10^{-6}		
Phenolphthalein	8.0×10^{-10}		

In choosing an indicator for a titration, one should be taken which gives the colour change not at the neutral point but at the 'equivalent' point, that is, when an equal number of equivalents of acid and base have been used. The H⁺ concentrations in 0.1-N solutions of the following salts are, NaH₂PO₄, 9.3×10^{-5} , Na₂HPO₄, 1.3×10^{-9} , and Na₃PO₄, 4.3×10^{-12} . The first hydrogen of phosphoric acid can be titrated with sodium hydroxide, using methyl orange as indicator, the second using phenolphthalein, and the third using trinitrobenzene. The proper indicator to use in any titration is easily determined from the dissociation constants of the acid and base."—E. SALM, *Z. physik. Chem.*, 1906, 57, 471—501.—*Journal of the Society of Chemical Industry*, June 15, 1907, pp. 642—3. (A. W.)

QUANTITATIVE SEPARATION OF CHLORIDES AND BROMIDES.—“For the quantitative separation of chlorides and bromides, the author finds that the most accurate results are obtained by the following modification of Bugarszky's method (*Z. anorg. Chem.*, 1895, 10, 387), based upon the use of iodic acid as an oxidising agent. In carrying out the method, the quantities of potassium iodate and nitric acid, employed as oxidising agent, are so regulated that the excess of iodic acid left after the oxidation is approximately constant. Also, the quantity of substance taken, should be greater the higher the proportion of bromide present. For example, in the case of the determination of chloride in crude potassium bromide, the following quantities will prove satisfactory:—

Percentage KCl in the Crude KBr.	Weight of crude KBr. taken.	N/5 KIO ₃ solution.	2N-Nitric acid.
5 - 10	gm.	c.c.	c.c.
1.5 - 5.0	0.6	36	20
0.25 - 1.5	1.8	96	26
	3.6	186	35

The mixture of chloride and bromide is treated with the potassium iodate and nitric acid in a 500 c.c. Kjeldahl flask having a neck 20—25 cm. long, and the solution is diluted to 200—250 c.c. The flask is then supported at an angle of 30° from the vertical, and the contents boiled gently, until the volume is reduced to about 90 c.c. in about 30—40 minutes. 100 c.c. of water are then added, and the boiling is continued until 50 c.c. have passed off. The escaping vapour is now tested by means of a piece of fluorescein paper held across the mouth of the flask for five minutes. When the test paper is no longer coloured orange, the escaping steam is passed through 10 c.c.

of a 2 per cent. solution of potassium iodide rendered faintly acid with hydrochloric acid, until the solution becomes hot. This test is stated to be extremely delicate, and when the iodide solution shows no yellow colour on viewing it against a white background, it may be accepted that all the bromine has been expelled. From 1 to 1.5 c.c. of a 25 per cent. solution of phosphorous acid are now added to the solution in the flask, without interrupting the boiling, and the boiling continued for 5 minutes after all the iodine has been expelled. The solution is then cooled, a moderate excess of N/20 or N/50 silver nitrate solution added, together with a piece of filter paper, and the whole shaken until the paper is reduced to pulp. After filtering and washing, the excess of silver nitrate is titrated with ammonium thiocyanate in presence of ferric nitrate according to Volhard's method, and then back again with silver nitrate solution.

For the determination of chlorine in crude bromine about 3 gm. of the sample (or more, if the chlorine content is below 0.5 per cent.) are added to a solution of 2 gm. of potassium iodide in 50 c.c. of water contained in a stoppered flask. After well shaking, the contents are introduced into a 400 c.c. Kjeldahl flask, 60 c.c. of N/5 potassium iodate solution and 24 c.c. of nitric acid added, and the chlorine determined as described above. The author draws attention to the need of precautions in taking samples of crude bromine, as the chlorine content is appreciably altered even by pouring from one vessel to another. The sample should be withdrawn by means of a special pipette from beneath the level of the crude bromine."—L. W. ANDREWS; *J. Amer. Chem. Soc.*, 1907, 29, 275—283.—*Journal of the Society of Chemical Industry*, May 31, 1907, pp. 554-5. (A. W.)

STRENGTH OF HYDROFLUORIC ACID.—“For the determination of hydrofluoric acid, a known quantity is mixed with neutral, nearly saturated potassium nitrate solution, a precipitate of potassium fluosilicate being produced. The cold solution with precipitate is now titrated with sodium hydroxide solution and phenolphthalein, to obtain the total hydrofluoric and hydrofluosilicic acids. The whole is then heated to boiling, and again titrated with sodium hydroxide solution. From the result of the second titration, the quantity of hydrofluosilicic acid is calculated according to the equation:— $K_2SiF_6 + 4NaOH = 2KF + 4NaF + SiO_2 + 2H_2O$.”—A. G. BETTS, *Engineering and Mining Journal*, 1907, 83, 753.—*Journal of the Society of Chemical Industry*, June 15, 1907, p. 607. (A. W.)

THE RATE OF COMBUSTION AND PRESSURE DEVELOPED IN A CALORIMETRIC BOMB.—The apparatus used in these tests was a Berthelot-Atwater bomb calorimeter with a valve opening connected with a sensitive pressure gauge. A special top was made to minimize the resistance between bomb and gauge. Experiments were carried out with gun-cotton, cellulose, benzoic acid, fat extracted from feces, and a pure vegetable proteid, edestin. The results of the investigations are summarised as follows:—

1. With an initial pressure of 300 lb., the pressures in the bomb rarely exceeded 700 lb.
2. The greater the quantity of material, the greater the pressure, although the pressure is not proportional to the weight of the substance.
3. The greater the initial pressure, the greater the maximum pressure, although regularity and proportionality are not observed.

4. The medium size capsule used in these experiments has a distinct tendency to decrease the maximum pressure, and to retard combustion. The other capsules have a much less marked, but similar effect.

5. The pellet form of material in all cases markedly decreases the maximum pressure and retards the combustion.

6. The incorporation of inert material is without appreciable effect on the maximum pressure, but markedly retards the rate of combustion.

7. Anthracite coal results in low maximum pressures, but the rate of combustion is extremely slow, sharply contrasting with results from bituminous coal.—F. G. BENEDICT and F. P. FLETCHER.—*Journal American Chemical Society*, vol. xxix., No. 5, p. 739, May, 1907. (J. A. W.)

THE DETERMINATION OF THE ORGANIC NITROGEN IN SEWAGE BY THE KJELDAHL PROCESS.—(1) The direct nesslerisation of Kjeldahl digestates is greatly to be desired, but unless special precautions are taken such a procedure will result in a precipitation of the Nessler's reagent by certain constituents of the digestate.

(2) The present study indicates that under ordinary conditions no trouble will be experienced on account of calcium, no special treatment for its removal being necessary unless the calcium content of the sewage is greater than fifty times the nitrogen content.

(3) Magnesium, on the other hand, even in small amounts, will cause a turbidity in the tube and render a correct reading impossible.

(4) Magnesium can be satisfactorily removed, but to do this requires an excess of sodium hydrate equivalent to a concentration of 1.5 gm. per 100 c.c.

(5) The excess of sodium hydrate necessary to remove the magnesium is not great enough to interfere with the reading. In fact, this reading is not appreciably affected unless the concentration of hydrate is such as itself to produce turbidity, or about 0.35 gm. per 50 c.c. Nessler tube.

(6) It is not at all certain that the turbid tube results solely from the presence of magnesium. It has been determined, however, that under the conditions required for the removal of magnesium any other deleterious constituent is also eliminated.

(7) A process is recommended whereby all disturbing constituents can be removed and the Kjeldahl digestate nesslerised directly. By filtering the neutralised digestate a saving of several hours can be effected over heretofore proposed methods.—LEYLAND WHIPPLE.—*Technology Quarterly*, June, 1907, p. 162. (H. A. W.)

ESTIMATION OF TUNGSTEN IN STEEL.—“From 2 to 10 gm. of the steel are dissolved in about 30 c.c. of nitric acid of sp. gr. 1.2, 1–2 gm. of potassium chlorate being added if necessary. The solution is evaporated to dryness, the residue treated with 15–20 c.c. of concentrated hydrochloric acid, evaporated nearly to dryness, and a further small quantity of concentrated hydrochloric acid added. The whole is now diluted to 150–200 c.c., boiled for a few minutes and allowed to settle. After decanting off the clear liquid through a filter, the residue of silica and tungstic acid is washed repeatedly by decantation with hot 5–10 per cent. hydrochloric acid till free from iron (by thiocyanate test), and then washed free from acid with 3–5 per cent. solution of sodium nitrate. The residue and filter are treated with excess of *N/5* potassium hydroxide solution, and heated; if necessary, to complete the solution of the

tungstic oxide; after cooling, the excess of alkali is titrated with *N/5* hydrochloric acid and phenolphthalein.”—S. C. LIND and B. C. TRUEBLOOD, *Journal of the American Chemical Society*, 1907, 29, 477–481. —*Journal of the Society of Chemical Industry*, June 29, 1907, p. 695. (A. W.)

ATOMIC WEIGHT OF SULPHUR.—The most important parts of this research may be briefly summed up as follows:—

1. A method for the preparation of pure silver sulphate was devised.

2. The specific gravity of silver sulphate, previously fused, was found to be 5.45.

3. Indication was obtained that Stas was unable to reduce silver sulphate wholly in hydrogen.

4. Silver sulphate was found to be occluded by silver chloride from solutions containing an excess of sulphuric acid.

5. It was proved that silver sulphate can be completely converted into silver chloride by heating in a current of hydrochloric acid gas.

6. 100,000 parts of silver sulphate were thus found to yield 91,933 parts of silver chloride.

7. The atomic weight of sulphur as calculated from this ratio, if oxygen is taken as 16.000 with several assumed values for silver, is:—

Ag. = 107.93 S. = 32.113

Ag. = 107.89 S. = 32.078

Ag. = 107.88 S. = 32.069

—T. W. RICHARDS and G. JONES.—*Journal American Chemical Society*, vol. xxix., No. 6, p. 826, June, 1907. (J. A. W.)

METALLURGY.

STANDARDISATION OF SCREENS.*—“The committee on standardisation of screens; appointed by the Council of the Institution of Mining and Metallurgy, London, has reported by its chairman, Mr. Walter McDermott, that it recommends the adoption of the following standard laboratory screens:—

Mesh.	Diam. of Wire.	Size of Aperture	Screening Area
	Inch.	Inch.	Per cent.
5	.1	.1	25.00
8	.063	.062	24.60
10	.05	.05	25.00
12	.0417	.0416	24.92
16	.0313	.0312	24.92
20	.025	.025	25.00
25	.02	.02	25.00
30	.0167	.0166	24.80
35	.0143	.0142	24.70
40	.0125	.0125	25.00
50	.01	.01	25.00
60	.0083	.0083	24.80
70	.0071	.0071	24.70
80	.0063	.0062	24.60
100	.005	.005	25.00
150	.0033	.0033	24.50
200	.0025	.0025	25.00

Owing to difficulties in wire drawing and in the weaving of wire cloth, absolute accuracy to the fourth place of decimals of an inch is unattainable with uniformity of 25 per cent. of screening area; but the above table is so near to theoretical perfection, and the unavoidable irregularities of screening tests themselves are so wide that any inaccuracies in the table would be absolutely immaterial in practice. It is not possible to weave the 200 mesh screen except in what

* See this *Journal*, vol. vii., April, 1907, p. 346.

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is known as 'twilled' or double wire, and it is questionable whether the table should not stop at 150 mesh as being fine enough for all practical sizing tests by screen.

The advantages of the series are:—

1. That a definite ratio between aperture and mesh is secured.

2. That by adopting a screening area of 25 per cent., the wires are absolutely 'locked' in position, thereby preventing shifting and consequent irregularity in the size of aperture.

3. That the ratio between wire and aperture in all meshes being constant, the 'cup' factor is also constant.

4. That as the table is based on the curve of seriation, any other sizes of mesh can be readily determined.

The committee appends a memorandum explaining in some detail the grounds and arguments upon which their recommendations are based, and they desire to add that these recommendations are the result of the most careful consideration of the suggestions of users of screens in all parts of the world and of the views of British and American manufacturers."—W. McDERMOTT. — *Mines and Minerals*, July, 1907, p. 554. (A. McA. J.)

VOLATILISATION OF GOLD DURING MELTING.—“It has long been known that gold is lost by volatilisation during melting in mints, but the amount of the loss has not been determined exactly. Moissan has recently shown (*Comptes Rendus*, vol. 141, p. 977) that gold can be distilled rapidly in an electric arc-furnace, and has given reasons for believing that its boiling point at atmospheric pressure is about 2,530° C. At such temperatures specimens of gold have been found to lose as much as 50 per cent. of their weight in a few minutes. In 1893 I found that pure gold (*Journal of Chemical Society*, vol. 63, p. 714) begins to volatilise in ordinary furnaces at a temperature just below 1,100°, and that the loss per minute at 1,250° is four times as great as that at 1,100°. I also showed that the standard gold-copper alloy is more volatile than pure gold, and that the alloy loses proportionately more copper than gold, although owing to the absorption of gases or other impurities during melting, an increase of fineness may not be observable in all cases. The temperature of the gold pots in the melting house furnaces has been found to vary between 1,100° and 1,200°. The melting point of standard gold is 951°.

Assuming that the loss by volatilisation varies directly as the area of the exposed surface of the molten metal, then, judging from the results of experiments on a small scale the loss would amount to about 0.2 per 1,000 per hour at 1,200° on a charge of 1,200 oz. of gold.

The rate of loss during melting does not depend entirely on the temperature and the length of time in the furnace. The passage of a current of air over the surface of the molten metal is very deleterious in increasing the amount of volatilisation, so that if the lid of the crucible is removed for any purpose the loss is increased. The presence of volatile impurities in bullion also causes increased loss. When the bullion is melted, they pass off, carrying gold and copper with them. It is probably due to this cause that a number of pots were rejected on assay in the past year as incorrect in composition. By the methods of assay any lack of uniformity in the composition of the fine ingots used for making standard bars would be detected, but the presence of volatile impurities would not be detected.

Volatilised gold is condensed and solidified almost at once and is carried forward by the furnace gases in the form of fine dust. Where the draft impinges on any solid body some of the gold is deposited, but it is difficult to collect the whole of it. In the mint furnaces, the brickwork surrounding the flues is coloured purple with deposited gold, and a sample of 103 oz. of dust recently taken from inside the flues was found to contain 1.39 per cent. of gold. At the Sydney mint some years ago Leibius found that the sweepings taken from the coping stone of the melting house stack contained 1.46 per cent. of gold and 6.06 per cent. of silver. It is evident that in this case some of the volatilised gold must have been carried away into the air and irrecoverably lost.

In order to reduce the loss of volatilised gold, collecting chambers have been attached to the flues of melting furnaces in a number of mints. A chamber of this kind was recently built at a cost of \$900 at the Philadelphia mint. In the course of little more than six months after it was built the dust collected in the chamber produced a bar containing gold to the value of over \$4,500, or nearly 1 oz. in 10,000 oz. melted.

The total amount of loss of gold by volatilisation in ordinary melting furnaces is never larger and probably seldom exceeds 0.1 per 1,000 in melting a charge of 1,200 oz., but it represents a considerable percentage of the final waste after allowance has been made for the gold in the sweepings. An unrecovered melting loss of 0.1 per 1,000 would correspond to over £200 on a coinage of £1,000,000, as on an average gold is melted somewhat more than twice in being converted into coin.”—T. KIRKE ROSE. — *Engineering and Mining Journal*, August 17, 1907, p. 297. (G. H. S.)

A COMPOUND ASSAY FURNACE.—“W. J. Fleck (*Mining and Scientific Press*, May 4, 1907) describes an arrangement whereby the capacity of a portable gasolene furnace may be increased materially when the cost of fuel, the expense of building, dust and slow heating operate against the building of a larger furnace. The arrangement consists of a large standard charcoal furnace erected beside the small gasolene furnace, and fired with dry wood. The charged crucibles are first placed in the charcoal furnace and heated until slow fusion begins. The crucibles are then quickly transferred to the gasolene, which is kept at a high heat, when quiet fusion begins at once, and in 10 minutes the crucibles are usually ready to pour. With this arrangement 16 assays may be melted and cupelled in a single crucible combustion No. 5 Hoskens furnace in four hours, or 50 assays in a No. 6 four-crucible furnace in the same time.

The flux used is first dried in open pans at a temperature of 200° F., and then intimately ground and mixed with the ore in a small wedgewood or glass mortar. The mixture is then transferred to the crucible, and a quick quiet fusion and a clean homogeneous slag is the result, the crucible being only slightly attacked on account of the short time of fusion. By this simple elimination of moisture and gases the need of salt or any other cover is obviated.”—*Engineering and Mining Journal*, Aug. 10, 1907, p. 249. (G. H. S.)

DANGERS OF FERRO-SILICON.—“By reason of the death of four persons on a Swedish ship which carried a cargo of ferro-silicon the Home Office has deemed it necessary to issue a memorandum pointing out the danger of such a cargo, based on the investigation

conducted by Prof. Cronquist into the disaster on board the Olaf Wijk. The opinion arrived at in course of the inquiry was that the fatalities referred to were due to gases given off by the ferro-silicon. The iron ore and quartz often contain phosphates, which in presence of carbon, and at the high temperature of the electric furnace, would, no doubt, be converted into phosphides, combining with the lime to form calcium phosphide. In the same way any arsenic present would yield calcium arsenide. These would be decomposed in the presence of water and evolve phosphuretted and arseniuretted hydrogen, both of which possess powerful poisoning properties. The explosions that have occasionally occurred are more difficult to account for, and various theories have been put forward to explain them. It seems that the presence of moisture is the cause of the accidents, and every effort should be made to ensure that the material is in a dry condition when packed, and that there is no possibility of moisture gaining access to the receptacles containing it."—*Times Engineering Supplement*, September 4, 1907. (J. A. W.)

CARBURISING IRON:—"The scarcity of manganese, which afflicts the world since the Russian disorders, has led to the invention of a very original process to carbonise iron at the end of the Bessemer operation for which manganese is usually employed. Sea-salt is now scattered on the molten metal, which augments the temperature, and gives special fluidity to the slag. In such circumstances it suffices to throw a handful of powdered coke into the metal about every four seconds, the coke being absorbed with wonderful rapidity and regularity. This does not dispense with manganese, but the amount is reduced, and results of the experiment made at Homecourt about a year ago seem very satisfactory."—*La Nature*.—*New Zealand Mines Record*, July 16, 1907, p. 551. (A. R.)

MINING.

RESCUE WORK IN MINES.*—The disaster at Courrières has been made the occasion of reviving the subject of rescue work in mines, and of again enforcing upon mineowners and medical men engaged in colliery practice the necessity of coming to some conclusions as to the best methods to be adopted in trying to reach and to remove miners imprisoned in a pit after an explosion. While after a colliery explosion many of the miners must have been killed by the shock and by the flame, as indicated by the marks of burning, and its destructive effects, as exhibited by the corpses, by far the larger number have fallen victims to carbon monoxide poisoning. When an explosion occurs there is first a deflagration or a sudden combustion attended with flame and gas. There is an enormous gaseous expansion, owing to the heat produced, and this is followed by contraction. The hot gases pass along the way of least resistance, and having exhausted the air there is created a void behind them. An explosion of firedamp and oxygen is followed by a contraction, so that when the watery vapour is condensed the volume of the gas is found to have diminished two-thirds. The explosion determines for a brief period a very elevated temperature, and this is followed by a sudden fall. Dr. Firmin Dervieux, in discussing the medico-legal aspects of the Courrières catastrophe, says that on the bodies of miners killed in explosions the effects of two different causes may be found—viz., wounds

produced from without inwards and consequent upon the enormous pressure of gas at the moment of the conflagration, also wounds produced from within outwards during the cooling and contraction of the gases. These latter wounds are present only upon the bodies of dead miners found a little distance from the site of the explosion or in a gallery ending in a *cul-de-sac*. In this way may be explained the ecchymoses and multiple injuries when the wounds have been caused from without inwards and the tearing and rupture of the tissues and organs when developed from within outwards. In the blood of coalminers killed in this manner no trace of carbon monoxide hæmoglobin is found on spectroscopic examination. Many of these men are killed by the column of gas suddenly striking them with all the force of a projectile. It is this shock which kills. Apart from the severity of the wounds, the proof that the miners under these circumstances are killed suddenly is the absence of carbon monoxide hæmoglobin in their blood. Is it possible for absorption of carbon monoxide by the blood to occur in a dead body? I have exposed the dead bodies of animals (rats and mice) to fairly strong percentages of carbon monoxide gas for 24 hours and for as long as a week without finding any trace of carbon monoxide in the blood. The probability, therefore, is that no absorption takes place. In the Courrières mine the largest number of deaths was due to carbon monoxide poisoning. When the percentage of this gas is enormous, as it must be after an explosion in a coal mine, and poisoning occurs on a large scale, convulsions are absent. It is in the slowly induced forms of carbon monoxide poisoning that the locomotive powers of men who have been exposed to it become enfeebled. Of these slower forms of poisoning there were also evidences in Courrières, for owing to the small percentage of the gas in some places the men detecting the 'bad air' and experiencing headache and vertigo had tried to save themselves, but as the gas travelled quicker than they could, it continued to exert its evil effects. Some of the miners thus overpowered had fallen on their knees, others overcome by coma had fallen prostrate.

It appears that several of the wounded miners who were brought to the surface only lived a few days. Nearly all of them died from pneumonia either due to their having respired the hot air in the mine or as a complication of carbon monoxide poisoning. Dr. Lourties states that several of the miners died from pneumonia from 50 to 60 hours after the explosion. The inflammation of the lungs developed in each instance with great rapidity and was accompanied by high temperatures. The lesions in the lungs were extensive; they were lobar and not lobular and there was a greater tendency for the right than the left lung to be affected. The proof that some of these miners who subsequently succumbed to pneumonia had respired carbon monoxide is to be found in the symptoms they complained of. The men said they had headache, vertigo, and an overpowering inclination to sleep, while in other patients the limbs were paralysed. In some of my own experiments with carbon monoxide gas I have found animals die from pneumonia and small hæmorrhages into the lungs two and three days after exposure to the gas. Pneumonia is occasionally, therefore, a sequel to the inhalation of carbon monoxide gas."—*Colliery Guardian*, August 23, 1907, p. 353. (A. R.)

HYDRAULIC AIR COMPRESSOR.—A singularly direct method of converting water power into compressed air power for use in driving the various

* See this *Journal*, vol. vii., Aug., 1906, p. 60; also vol. vii., Sept., 1903, p. 100.

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machinery of a mine is in operation at the Victoria Mine, Michigan. Instead of the water pressure being utilised to drive a turbine, and thereby actuate an air compressing plant, the water itself compresses the air directly without the mediation of any machinery whatever. In this way much expense of maintenance is avoided, and a far higher degree of efficiency obtained. The *Scientific American* states that a loss with a water turbine of 10 or 12 per cent., would be remarkably good efficiency, while there would be a further loss of at least 30 per cent., in converting the power into compressed air. But in the hydraulic air compressor in question, the total loss is said to be only 18 per cent. At a point on the Ontonagon River, where there is an available head of water of 77 ft., a vertical shaft has been sunk through the solid rock to a depth of about 350 ft., where it ends in a vast subterranean chamber. From the bottom of this immense underground cavern, another tube slopes upwards to the tail race some 280 ft. above. Through the first mentioned vertical shaft the water pours in headlong torrents, carrying with it myriads of air-bubbles drawn from the atmosphere above. As the water traverses the subterranean lake in the cavern the air-bubbles rise to the surface, and the air is trapped in the domed roof of the chamber, while the water flows away up the inclined tunnel into the tail race above. It will be seen that the vast quantity of trapped air in the chamber is held at a pressure of 114 lb. to the sq. in., due to the difference in level of the water in the cave and that in the tail race. An ingenious device serves to cut off the supply of air at the intake when the pressure in the air reservoir exceeds this amount. The compressed air is led from the chamber through a 24 in. pipe to the mine, when it is distributed to the various points at which the power is required. At the Victoria Mine, there are three intake shafts, when all are working—5,000 h. p. is developed and available for use in driving the machinery of the mine. One great advantage of this system of compression is that the large amount of heat always liberated when air is subjected to pressure is carried off by the surrounding water, so that the system is practically isothermal.—*Scientific American*.—*Chambers' Journal*, August, 1907, p. 558. (A. L. E.)

PUMPING DEVICE.—“A very ingenious method has been adopted to extract liquids from boreholes, invented by Mr. Bruno Leinweber, of Vienna, for extraction of petroleum. It is utilised in Galicia at the deposits of the Galician Carpathian Petroleum Company in Kryg, near Maryampolski. The method consists in dipping a band or rope into the borehole, which absorbs the liquid; the saturated rope is then compressed between cylinders or rolls at the surface, to collect the oil. The apparatus utilised at Kryg was constructed under the management of Mr. Oscar Förster, at the Reichenberg Railway Workshops, Bohemia, where it is mounted on two ‘I’ girders for shipment; thus it arrives ready for use. The hemp band employed, 80 mm. wide and 8 thick, is similar to a mine cage cable, covered with a fibrous or plushy material to make it permeable to liquid. To a depth of 500 metres, Mr. Leinweber recommends a flat hemp band, and beyond this a steel cable. It is in three sections, jointed by stitching with shoemakers’ waxed thread, which demonstrates the simplicity of the device and the facility of repairs. As the band is continuous, the only weight to lift is that of the liquid or oil on the ascending rope. At Kryg, owing to local circumstances, the apparatus is worked by hand. The amount of oil thus poured into the tank

by compressing the rope is about 415 gm. per current meter. The method can also be utilised for brine, acid solutions, or even drainage of mines.”—*Revue Industrielle*, July 27.—*London Mining Journal*, Aug. 3, 1907, p. 145. (A. R.)

GOOD TIMBERING AND DEATH RATES.—“Past records show that France has the lowest death rate resulting from the fall of roof in coal mines. This is due to the thorough method used in timbering gangways and working faces. The system briefly described is as follows:—Bars 10 to 15 ft. in length are supported upon posts set 1 metre (39·48 in.) apart. The bars are placed parallel to the working faces and are set 1 metre apart. They are made of iron, either an I-beam or square in the cross section, and are driven above the last cross-bar at the face, leaving about 1½ ft. of the face unprotected between the ends of the bars. As the face advances, short poles are set up to support the last bar before the next bar is put up. The miners are not allowed to work under unprotected roofs. The coal is mined and broken down by wedges, no blasting powder being used, which enables timbering close to the face.”—*Science and Art of Mining*, Aug. 10, 1907, p. 370. (T. L. C.)

FERRO-CONCRETE IN MINE WORK.—“Up to the present comparatively little use of ferro-concrete has been made by colliery proprietors in Great Britain; notwithstanding the extensive application of that material on the Continent and in America for supports in workings and galleries as an economical and durable substitute for timber. It is interesting to observe, however, that in the collieries of the Harton Coal Company at South Shields some underground tanks have recently been completed in accordance with the Mouchel-Hennebique system, for the collection and storage of water accumulating in the workings. The tanks in question are situated some 1,600 ft. below surface level, and their construction presented some difficult problems for solution, owing to the fact that the gallery was but 18 in. wider and 15 in. higher than the tanks themselves. In a situation such as this iron or steel tanks were not to be considered, for even if their erection were practicable, the impossibility of dealing with future leakages would render their use out of the question.”—*Colliery Guardian*, Aug. 30, 1907, p. 401. (A. R.)

MISCELLANEOUS.

CORROSION OF IRON.—“Dr. A. S. Cushman, of the United States Bureau of Agriculture, discussed in an able paper the corrosion of iron. He explained why neither the carbonic acid theory nor the hydrogen peroxide theory could be regarded as adequate explanations of the rusting of iron. He presented a new theory, which considers the rusting of iron as an electrolytic phenomenon. Oxygen is not the primary cause of rusting, but hydrogen ions. The rôle of oxygen is only secondary. What really always happens when iron rusts is this: there is some short-circuited galvanic cell (due to impurities contained within and distributed throughout the iron) and hydrogen ions are set free, iron passing into solution as a ferrous ion and replacing the hydrogen. The ferrous ion is then oxidised by the oxygen in the air to ferric, and the formation of a hydrated oxide results.

All soluble inhibitors of rusting act in either of the following two ways:—Alkaline solutions prevent rusting because there are no hydrogen ions in alkaline solutions. Chromic acid and its salts, which are strong oxidising agents, prevent rusting on account

of the formation of an oxygen film (not an oxide film), so that the iron becomes polarised in the sense of becoming an oxygen electrode. The author stated that a solution of potassium bichromate, not stronger than $\frac{1}{100}$ normal, will indefinitely prevent the rusting of polished specimens of metal in cold water, even if free access of air and carbonic acid is provided for. If iron in any of its forms is immersed in strong solutions of bichromate for a few hours the surface becomes passive, even after it is removed from the solution, washed and wiped.

Since the rusting of iron is the result of the existence of galvanic couples, electric potential differences exist, and the places of cathodes and anodes can be made visible by means of a special indicator which is called by the author 'ferroxyl.' Agar-agar and gelatine jellies impregnated with phenolphthalein and potassium ferricyanide show red and blue nodes on specimens of iron and steel imbedded in them; the blue nodes appear where the iron is anode and is passing into solution, while the red nodes indicate cathodes. If a certain point continues to be anode for a length of time pitting occurs. The author urged that experiments should be made to try the prevention of iron rust by means of bichromate on a practical scale."—Dr. A. S. CUSHMAN.—U.S. Bureau of Agriculture in *Electro-Chemical and Metallurgical Industry*, July, 1907, p. 257. (A. McA. J.)

DURABILITY OF WOODEN WATER PIPE.—"The use of wooden water pipe, for mine and mill water supply is a subject of considerable interest to mining men, owing to the present high price of steel and cast iron pipe, and the frequent inability to secure such pipe in a reasonable time, owing to the great demand from municipalities, water companies, and others. Therefore, the following excerpts from a discussion by Mr. T. Chalkley Hatton, at a recent meeting of the American Society of Civil Engineers, on the relative merits of different types of wooden pipe are of value to our readers. Mr. Hatton's observations were based on investigations carried on during the past three years, which covered much of the irrigating work in the west, where continuous wooden stave pipe has been used for many years, and several of the municipal and industrial plants where both continuous and machine-made wooden stave pipe have been in use for at least 40 years.

His investigation of continuous wooden stave pipe led him to believe that there were too many uncertainties in its manufacture, that while the wood would undoubtedly outlast steel or iron pipe, yet its construction must be necessarily left to irresponsible, partially unskilled workmen, who in many instances had to prosecute their work under the most unfavourable conditions. He therefore turned his attention to a study of machine-made wooden stave pipe.

Here he found a wooden pipe being made, out of the same kind of materials as composed the continuous wooden pipe, and banded together by automatic machines run by capable workmen, and every stave, band, coating, and all workmanship entering into the construction of said pipe open to inspection under the most favourable conditions. The inner and outer surface of the pipe made perfect circles; the steel band, which was either of continuous band steel of any width required by the pressure to which it was to be subjected, and of about No. 16 gauge, or of steel or copper wire of any gauge demanded, was run through a bath of warm asphaltum pitch while bright and free from rust and then spirally wound upon the pipe carrying with it a heavy coating of the pitch,

thus preventing the inner surface of the band from coming in direct contact with the outer surface of the wooden staves. Each end of the band was well secured by a double band around each end of the 7-ft. or 8-ft. length, in which lengths it is usually made. As soon as the banding is completed, the section of pipe is run over two rolls moving through a warm bath of asphaltum pitch, thus coating the outside surface of the wood and bands with a full protective coating of from $\frac{1}{4}$ in. to $\frac{3}{8}$ in. in thickness. To prevent this coating from running, the pipe is immediately rolled in sawdust, which adheres to the coating and prevents it from being knocked off or abraded in handling. An extra coating can be readily applied to each section by repeating the operation, which in all instances occurring under his notice covered the bands and pipe to a thickness of nearly $\frac{1}{2}$ in. This coating, as soon as cool, became very tough and could only be removed by a chisel, was not injured in shipping and handling, and came upon the work in as good condition as when it left the factory.

This machine-made wooden stave pipe, as made by one manufacturer, is provided with a wooden stave coupling about 8 in. long that fits over the spigot end, this spigot end being driven into it, making a strong and water-tight joint. The machine-made wooden stave pipe as made by the Wyckoff Co., of Elmira, N. Y., differs from this in having a tenon 4 in. in length cut on one end and a mortise of the same depth cut on the other end. This latter is in the form of a truncated cone, so that when driving the pipe home the joint becomes tighter the farther it is driven. The inner and outer surfaces of the pipe when formed by this method are uninterrupted throughout and without any projections, and the swelling of the wood by saturation of the water makes the joints as good as the body of the pipe. These pipes can be made of any diameter from 6 in. to 60 in. to withstand any pressure up to 300 lb. according to the banding and can be laid by unskilled workmen in either a wet or dry ditch."—T. C. HATTON.—American Institute of Civil Engineers from *Mines and Minerals*, July, 1907, p. 544. (A. McA. J.)

NEW SOURCE OF POWER.—The possible future exhaustion of our coal supplies, as indicated by various Commissions on the subject, has led to the search for other sources of a supply of power. One of the most promising of these seemed to be the utilisation of the rise and fall of the tide, for in this there is a practically inexhaustible amount of available energy. Hitherto the tapping of this limited source has seemed beyond the range of practical engineering and mechanics. And this, perhaps, chiefly because the abundance of our coal supply has prevented the attention of inventors being turned in real earnest to the subject. A practical attempt, however, is now about to be made to harness the sea in the Bay of Fundy. This well-known inlet between New Brunswick and Nova Scotia has long been famous for its tidal bore. Up this tunnel-shaped inlet, 48 miles wide at its mouth, the tide rushes with savage impetuosity. At high tide the water rises to a height variously estimated at from 42 to 70 ft., the lower figures being probably more nearly correct, and during spring tide the water in the Bay of Fundy stands 19 ft. higher than in Bay Verte, only 15 miles off; while on the opposite coast, Halifax harbour has a rise of only 7 or 8 ft. compared with some 50 ft. in the former bay. Here, then, seemed to be an ideal spot for the first serious attempt to harness the tides. A company has been formed, and it is proposed to build three dams, each

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400 ft. high and 1,000 ft. long, across the river Tanager, near Sackville. In this way reservoirs and discharge basins will be formed. Turbines placed in the dams will be set in motion as these are filled and emptied by the flowing and ebbing tides.—*Science and Art of Mining*. (T. L. C.)

PASSIVE IRON.—“It is shown that the formation and properties of passive iron, and the process of passivifying is always electrolytic, and that when no external current is used local currents are generated from one part of the surface to another. Iron may also be rendered passive by the immersion of two electrodes of this metal in 16 per cent. sulphuric acid and the application of an e.m.f. of more than 0.65 volts. The current density must exceed a certain value for passivity to ensue. An iron rod which was being energetically attacked by dilute nitric acid became passively attacked by the applied e.m.f. (sp. gr. 1.2) became passive when the applied e.m.f. was ten volts and the current density 0.75 ampères per sq. cm. This change occurs at the anode, and may be reversed when the passive iron is made the cathode. The author describes other processes whereby passive iron may be rendered active, and concludes that the one process is the reverse of the other. After reviewing the various theories proposed to explain the phenomenon of passive iron. The author inclines to the view that the magnetic oxide theory affords the most satisfactory explanation of the observed facts.”—H. L. HEATHCOTE, *Journal of the Society of Chemical Industry*.—*Times Engineering Supplement*, Sept. 11, 1907. (J. A. W.)

DEFLOCCULATED GRAPHITE.—“Edward G. Achenbach delivered an experimental lecture at the Niagara Falls Convention of the American Institute of Electrical Engineers, illustrating the properties of a recently discovered form of graphite. This graphite is produced by adding water, galloannic acid and ammonia to nunctuous graphite as produced in the electrical furnace. Under these conditions the graphite is miscible with the water. When it is mixed with water, tannin and ammonia the graphite assumes what is called a deflocculated condition, a condition of fineness beyond that obtainable by mechanical means—one approaching the molecular state. Deflocculated graphite in water has been used successfully instead of oil in sight-drop feed oilers and with chain-feed oilers. It possesses the remarkable property of preventing rust or corrosion of iron or steel. The deflocculated graphite has also been successfully employed with kerosene oil as an effective lubricant.”—*Engineering and Mining Journal*, Aug. 24, 1907, p. 354. (G. H. S.)

TRANSVAAL CYANIDE.—“Last year the mines of the Rand used some 8,780,550 lb. of cyanide, valued at £344,599, and the fact would seem to promise a prosperous future for the latest infant industry of the Transvaal which is concerned in nothing less than the local manufacture of cyanide from chemical deposits. The new venture is styled the South African Cyanide Company, Ltd., has a nominal capital of £50,000, of which £35,000 in shares goes to the vendors (for nine patents), £10,000 has been subscribed for working capital, and 5,000 shares are held in reserve. The working capital, we are informed, has been privately subscribed, and the Company has in contemplation the erection of a substantial factory in proximity to the Johannesburg-Vereeniging Railway. The main point, of course, is the price at which the new product can be sold, and this, we are assured, will be at least 20 per cent. below the cost

of the imported article. The promoters inform us that one group of mines estimate that a saving of £50,000 may be made by it if the local cyanide comes up to sample. In addition to cyanide of potassium, the Company contemplates the manufacture of cyanide of sodium and magnesium, soda crystals, bi-carbonate of soda, caustic soda, and sodium borate, by patent processes. If the new Company can substantiate its claim to reduce the cost of cyanide by one-fifth, and that by the local manufacture of the article from local materials, with the aid of capital furnished locally, no one will begrudge it success.”—*South African Mines*, June 1, 1907, p. 267. (J. Y.)

Reviews and New Books.

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

SHAFT SINKING IN DIFFICULT CASES. By J. RIEMER. Translated by J. W. BROUGH. Price 10s. 6d. net. (London: Charles Griffin & Co. 1907.)

“The present translation by Mr. J. W. Brough, Assoc. M. Inst. C.E., of an authoritative work by an acknowledged expert with full knowledge and practical experience of his subject, is a welcome addition to the library of the student, and it will be valuable to the mining engineer, at it appeals directly to those in charge of mining undertakings, giving them a review of the various methods that may be used in difficult cases of sinking and providing data to facilitate the selection of the best method for any particular case under consideration.

The book is divided into four parts. The first briefly describes what may be called hand sinking, passing in rapid review the various methods of dealing with water by barrels and giving an interesting description of under-hanging tubing which has been practised with such success in Zollern I. mine. Part 2 deals with shaft-sinking by boring on the original method of Kind and Chaudron. Part 3 gives a complete *résumé* of the present position of the freezing process, and the author's remarks under this head are particularly valuable, as he is well aware of its limits and of the cases in which its use is advisable. The final part deals with the sinking drum process, discusses the probable explanations of the cause of cracked drums, the Pattberg compound drum and the sack borer of Sassenberg and Clermont.

A bibliography of recent papers on shaft-sinking and an index have been added to the translation. The work is well printed and is provided with eighteen figures in the text and nineteen folding plates. It is an undoubted acquisition to the literature on the subject and is worthy of careful perusal and study by all mining men.”—*Colliery Guardian*, Aug. 23, 1907, p. 355. (A. R.)

ELECTRICITY IN MINING. By SYDNEY F. WALKER. Price 9s. net. (London and New York: Harper Brothers.)

“In this book the author has evidently addressed himself to the mining engineer. Mining engineers in the past, with the caution which arises from the nature of their calling, have been somewhat chary of the use of a power with which they were not closely familiar. There has been a marked improvement in this respect within recent years, but there still

remains plenty of scope for works on the subject. In seven chapters Mr. Walker deals with the principles and terms of electrical practice; signals and telephones; lighting apparatus; the generation of power; distribution; application to driving machinery, etc.; and faults in electrical apparatus. The book is illustrated by upwards of 250 drawings and photographs, and is excellently printed." — *Colliery Guardian*, Aug. 23, 1907, p. 356. (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.) 390/07. W. E. Kimber. Improvements in rock drilling machines. 7.9.07.
 (P.) 391/07. G. Bonatto. Improvements in safety catches for mine cages and other hoisting apparatus. 7.9.07.
 (P.) 392/07. J. Hosking. Improvements in and relating to rocks drills. 9.9.07.
 (P.) 393/07. J. Hosking. Improvements in and relating to ore crushing mills. 9.9.07.
 (P.) 394/07. A. L. Marthinsen. Improvements in the driving of gravity stamps. 10.9.07.
 (P.) 395/07. G. Nocosia. Improved artificial stone and the process of manufacture thereof. 10.9.07.
 (P.) 396/07. E. M. Hewlett. Improvements in electric insulators. 13.9.07.
 (P.) 397/07. W. C. Boyd. Improvements in the fixing of collars, bosses and other similar pieces to shafts or their equivalent by means of set screws and the like. 14.9.07.
 (P.) 398/07. J. H. Rowe. Improvements in the treatment of crushed ore products and apparatus therefor. 14.9.07.
 (P.) 399/07. J. van Waart. A new device or apparatus for the production of power or energy (motion created by gravitation). 29.6.07.
 (P.) 400/07. J. van Waart. A new device or apparatus for the production of power or energy by magnetism. 29.6.07.
 (P.) 401/07. J. van Waart. A new device or apparatus for the production of power or energy. 29.6.07.
 (P.) 402/07. T. Rasmussen. Improvements in rock drilling machines. 16.9.07.
 (P.) 403/07. T. Rasmussen. Improvements in rock drilling machines. 16.9.07.
 (P.) 404/07. T. O. Bryne. Improvements in internal combustion engines or motors. 16.9.07.
 (P.) 405/07. R. H. Harriss. Improvements in disintegrating diamond bearing matter by chemical methods. 16.9.07.
 (P.) 406/07. H. Goddard. A new and improved sun and mosquito screen. 17.9.07.
 (P.) 407/07. J. G. Laidlaw. Improvements in the manufacture of artificial compressed stone. 18.9.07.

- (C.) 408/07. J. H. Holman (1), J. M. Holman (2). Improvements in or connected with rock drills. 20.9.07.
 (C.) 409/07. A. C. Bartlett. Apparatus for preventing the escapement of dust caused by mining drills of the pneumatic type. 20.9.07.
 (P.) 410/07. J. E. H. Grose. Improvements in rock drilling machines. 21.9.07.
 (P.) 411/07. E. H. Martin (1), B. Chew (2). Improvements relating to ore stamps. 21.9.07.
 (C.) 412/07. A. R. Wojciechowski. An improved process and means for disintegrating and rendering friable diamond bearing grounds. 25.9.07.
 (P.) 413/07. W. Wanius. Improvements in rock drilling machines. 26.9.07.
 (C.) 414/07. T. L. Lockhart. Improvements in the extraction of gold and silver from their ores. 26.9.07.
 (P.) 415/07. C. E. D. Usher (1), Adair-Usher Process, Ltd. (2). Improvements in the treatment of sands or the like with liquid. 26.9.07.
 (P.) 416/07. W. Burgess. Improvements in means for neutralising noxious fumes produced in blasting. 26.9.07.
 (C.) 417/07. T. Parker. Improvements in and relating to the production of fuel. 27.9.07.
 (C.) 418/07. N. Ceipek. Improvements in the manufacture of an improved safety explosive. 27.9.07.
 (C.) 419/07. W. T. Smith. Improvements in and relating to process of extracting metals from their ores. 27.9.07.

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- ADENEY, G. B.; *l/o* Germiston; Lancashire College, Manchester.
 CLARKSON, R., *to* New Rietfontein Estate G. M., Ltd., P. O. Rietfontein Mines.
 COLLINGS, B. I., *l/o* Johannesburg; P. O. Box 16, Salisburg, Rhodesia.
 DURHAM, B. W., *l/o* Johannesburg; P. O. Cleveland.
 GUEST, I. A. M., *l/o* Rhodesia; P. O. Box 1, Klerksdorp.
 HELLMANN, FRED., *l/o* East Rand; *c/o* Messrs. F. Huth & Co., 12, Tokenhouse Yard, London, E.C.
 KLINE, R. C., *l/o* Mexico; La Jolla, San Diego Co., Cal., U.S.A.
 KYLE, J., *l/o* Cleveland; P. O. Denver.
 LEYSON, WM., *l/o* Boksburg; P. O. Box 74, Barber-ton.
 MACQUEEN, W. P. O., *l/o* Johannesburg; Crown Deep, Ltd., P. O. Box 102, Fordsburg.
 MANNHEIM, H. C., *l/o* Salisbury; P. O. Box 445, Bulawayo.
 PEDRINI, J., *l/o* Switzerland; P. O. Box 53, Krugersdorp.
 PEIRCE, A. W. K., *to* Victoria Falls Power Co., Ltd., P. O. Box 2671, Johannesburg.
 SIMPSON, T.; *l/o* Luipaardsvlei; Ferreira G. M. Co., Ltd., P. O. Box 1021, Johannesburg.
 SURMON, C. B., *l/o* Germiston; Shangani Siding, Rhodesia.
 WILSON, R. A., *l/o* Jeppestown; Eldorado G. M. Co., Lomagundi, Rhodesia.