2½° to 3° Baume, and according to the size of the plate to be electrolyzed, two or four such cells are placed opposite one another. The top of the relief plate should coincide with the top of the zinc.

If, as is required in the treatment of delicate gelatine reliefs, the whole surface of the plate is to be quickly covered with precipitated copper, the porous cells should be replaced during the day with others, duly rinsed in water and freshly filled. Moreover, the ends of the wires must be maintained bright by frequent rubbing with emery cloth.

The plates to be electrolyzed, before being put into the trough, should be rinsed uniformly with strong spirit, so that the formation of air-bubbles is avoided, and then as quickly as possible immersed in the solution, and the wires connected up. If a plate has already received a deposit of copper over its surface, then, before a second treatment in the electrolyzing trough, it should be rinsed, and lightly brushed with dilute sulphuric acid to facilitate the binding of the new deposit of copper.

Sometimes certain parts of the surface of a plate are a long time getting covered, and an oxidizing action is set up; in this case some little attention is necessary. The plate is raised from the trough in a horizontal position and covered with a sheet of blotting-paper dipped in the sulphate of copper solution, a piece of the blotting-paper being torn off where the defective parts are, and these bare gelatine portions carefully dried with tissue paper. These same defective parts of the plate are then carefully covered with a solution of rubber in benzole. After this varnish has dried—it dries very rapidly—more graphite is applied, by dabbing carefully with a muslin bag containing this powder. The whole operation must be conducted with the greatest care, so that the surface afterward has the appearance of a perfect mirror.

The finest Siberian graphite, which has been rendered a good conductor by treatment in a chloride of gold solution, is the best that can be employed for the purpose. Moreover, it is desirable frequently to shift the plate in the trough and to turn it over, as otherwise there will not be an equal deposition of copper if the battery is a powerful one.

The depositing action may be accelerated by hanging inside the trough a little bag of sal-ammoniac crystals.

The cleaning of the relief plates, when of sufficient thickness, and freeing them from the gelatine film in hot water, is done by Seamoni with the aid of caustic potash, prepared chalk, oil, and charcoal powder; or in the case of very delicate objects by means of benzol and India-rubber.

In conclusion, it may be remarked that generally speaking, notwithstanding the enormous progress that has lately been made in electrotyping, and in the deposition of various metals, the moulding of fine gelatine reliefs still requires much experience, great skill, and extraordinary patience.—*Photo. News.*

A NEW RESIDUAL PRODUCT FROM THE DISTILLATION OF COAL.

By GEORGE E. DAVIS.

THE distillation of coal, or rather, as it is generally termed, its carbonization, is scarcely a century old, and from the very first days of the industry up to the present, nearly the sole aim has been the production of illuminating gas, the tar and the ammoniacal liquor being termed residual products. From time to time there have been proposals to turn the gas into a residual and go in for making tar-water only, and by increasing the yields of these, and by utilizing the gas under the retorts, to manufacture sulphate of ammonia and tar products simply. For some reason or another these projected schemes have never been successful, for the simple reason that tar distilled from coal at a low temperature is not as good as ordinary tar, and with low temperatures the yield of ammonia water is not usually so great as with higher heats. It is true that a considerable revenue may be derived from the sale of coke, but taking all things into consideration there has been so slight a margin, except in special situations, such as distillation on the pit-bank, that but few capitalists have been induced to embark in the undertakings.

The older analyses of coal-gas gave us a very poor insight into its real composition. Bunsen, in his "Gasometry," gave us elaborate methods and calculations respecting the analysis of coal-gas, but recent experiences have shown that many of the deductions are not worth the paper they are printed on. The real and most important illuminating constituents of ordinary coal-gas were entirely overlooked, and so many a practical worker was misled.

Our smoke and fog question, which has cropped up annually, has brought before our notice the gradually advancing use of gas for heating and cooking purposes, and it is evident that the gas for such purposes need not depend for its excellence upon its illuminating power. In fact, its power to deposit carbon during combustion is a defect for both cooking and heating and the use of a gas deprived of its more condensable hydro-carbon vapors would offer many advantages over the old system.

Now reference to the published analyses of coal-gas would, in most cases, lead us to believe that such a task would be impracticable as a manufacturing operation. We are told that the greater portion of the illuminants were olefiant gas, diethyl, etc., or liquids of such low boiling-point as to be reckoned practically with the permanent gases. Investigations extending over a lengthened period have shown me that all the previous analyses of coal-gas are of no value and are moreover inaccurate. The chief illuminants are vapors of easily condensable hydrocarbons commencing to boil at below 18° C., and containing many hydrocarbons, even up to solid naphthalene. Now these "illuminants" are easily absorbed by olive oil or rape oil and even by hydrocarbon oils, and it has been found by many experiments that as much as five gallons of these condensed hydrocarbons can be obtained from the gas yielded by one ton of coal.

These hydrocarbons are worth much more than the original coal, plus materials and labor; therefore it seems a very profitable undertaking, when the gas is required for cooking and heating purposes, first to extract the illuminating hydrocarbons. This can be done so completely that the gas on combustion yields only a flame of exceedingly feeble luminosity, this being due to the small quantity of what we may call permanent gases still remaining.

It is clear, then, that in the treatment of coal for heating purposes we have another residual product, if the process is so conducted as to eliminate the illuminating vapors. I propose to call it *crude benzol* in contradistinction to the *crude* naphtha, or first runnings of coal-tar.

Many of you will be aware that a process of carbonization for the sake of these products is now most successfully carried on in the Midlands, and the process has a great future before it; but I have learned by experience that it is only

those who can carry on their processes in an exact and scientific manner, and who can obtain the most from their raw material, who are likely to succeed to the end. The differences of temperature, both in retorts and out of them, lead to such variation in the quality of the crude benzol produced as to clearly point to the fact that these operations must be more carefully attended to than in ordinary gas-making.

Ordinary gas-coal of good quality should yield over four gallons of this residual product, and it is very probable that before long a large quantity of it will be sent into the market. Buyers should, however, be cautious. If the heat is too low, more paraffins are formed, and this is only to be discovered by special tests. The ordinary process of distillation and catching all that which distills over at certain mean temperatures is inadequate to discover the presence of paraffins. These are, however, proved by the short yield of aniline oil produced from the benzol in question.

My object in making these few remarks is owing to having heard that carbonizing is a failure, which statement can only have been made by those unacquainted with what is really going on. I am sorry I cannot go more fully into details concerning my own process, but as I intend reading an exhaustive paper at a future date upon the subject, you will soon be able to read it in the admirable journal of our Society.—*Chem. News.*

THE ESTIMATION OF SULPHUR IN COAL GAS.

THE process recommended by Herr Th. Poleck, of Berlin, for the estimation of sulphur in coal gas consists, as usual, in the complete oxidation of the whole of the sulphur compounds to sulphur dioxide, the further oxidation of the dioxide to sulphuric acid by means of brominated caustic soda, and the estimation of the sulphuric acid as barium sulphate. Over a Bunsen burner, which is connected with a gas-meter, is placed a wide tube, open below, in such a manner that the burner penetrates about 2 cubic meters into the tube. The gas-flame should be non-luminous, and not too high. The products of combustion are drawn by means of a water-pump through three U-tubes; of which the first two contain brominated and the third unaltered caustic soda. At the conclusion of the experiment the volume of gas burned is read off at the meter; and the sulphuric acid estimated as barium sulphate. If the sulphureted hydrogen and carbon bisulphide have been separately estimated, the difference between the quantity of sulphur in these compounds and the total amount found gives the sulphur combined with hydrocarbons. The carbon disulphide is to be determined by converting it into the triethyl-phosphine compound. An investigation of the amount of sulphur present in samples of gas from different parts of the apparatus gave: In 100 liters immediately from the retort, 0.600 gramme; before the scrubbers, 0.540 gramme; after them, 0.464 gramme; behind the condenser, 0.440 gramme; and in the purified gas, free from sulphureted hydrogen, 0.276 gramme. The advantages claimed for the process are that it is continuous, does not require very careful attention, and allows of the combustion of large quantities of gas.

EQUIVALENTS OF METALS BY MEANS OF THEIR SULPHATES.

By H. BAUBIGNY.

IF the determination of the equivalents of the metals by means of their sulphates has already been attempted several times, many of these determinations have not presented the agreement to be desired. The cause of these discrepancies lies in the difficulty often found in obtaining sulphates chemically neutral and always constant in composition. By successive crystallizations they can but rarely be completely freed from the last traces of free acid; some of them are even decomposed by water, and for expelling the excess of acid, heat is necessary. But if it is too high, there may be partial decomposition of the sulphates, and the experiment may be vitiated by errors in the opposite direction. The employment of a method of heating by which the sulphates may be submitted indefinitely to a temperature constant and relatively high, but below that of their decomposition, is the first condition for securing to this kind of determination all the precision which it requires.

The use of the sulphur bottle realizes this condition. By numerous experiments, the author has shown that, with the exception of the sulphates of gold and of the platinum group, which he has not studied, this class of salts possesses a stability so great that they may be kept for entire days at a temperature notably higher than the boiling point of sulphuric acid, and that even at this temperature of 440° certain sulphates only lose the last traces of free acid very slowly, and that there is room for employing certain artifices for accelerating the elimination of this acid.

This process constitutes a precise and exact method, since it enables us to avoid any overheating capable of producing incipient decomposition, and removes all doubt as to the state of the salt to be operated upon. The author has thus obtained without difficulty ferric sulphate as a perfectly definite salt of a rosy white.

The preparation of neutral anhydrous sulphates becomes under these conditions as rapid and perhaps even more certain than that of the chlorides, which are also capable of retaining excess of acid or, if their hydrates are heated, of forming basic salts, unless we confine ourselves to the most laborious conditions of preparation.

When the sulphates are thus obtained, their analysis constitutes the second and last stage of the operation. If we take account of the decomposition of these salts by heat by the loss of their acid, as Boussingault demonstrated in 1867, by operating in a furnace heated with the blowpipe, this analysis may reach a high degree of perfection, for it is reduced to a calcination and two weighings, that of the sulphate with the boat in which it has been prepared (the tare of which is known), and that after calcination, which shows the loss of the acid and the weight of the oxide which remains, *i.e.* the proportion of the weights of the acid and the base.

This method of analysis by the dry way, which does not require any manipulation of the matter outside the boat which contains it, thus excluding all causes of error inherent in determinations by the moist way, permits of a greater precision than the processes of the latter kind. This analytical method is also of more easy execution than the determination of equivalents by the synthetic method, *i.e.* the transformation of a known weight of metal or oxide into sulphate—an operation in which there is danger of losses, as Berzelius and Stas declare in their remarks on the extreme difficulty of bringing such a synthesis to a good conclusion.

This process is defective only when the sulphates themselves are volatile with or without decomposition; as those of potassium, sodium, thallium, and mercury, or when the

bases themselves are volatile at high temperatures, as baryta and strontia. The determination by the moist way, or that by synthesis, are then the only expedients possible, and in the latter case the last traces of free acid are expelled, before weighing the salt, by heating in the sulphur-stove.

As for the oxides capable of becoming oxidized in the air under the influence of heat, the way of dealing with this property is sufficiently well known.

The solution of the pure sulphate is evaporated in a platinum vessel, protected from the dust of the air, as recommended by M. Stas. Part of the salt is introduced into a tared platinum boat, which is allowed to slide into a glass tube, the open extremity of which alone projects out of the sulphur-stove. It is kept at this temperature until two consecutive weighings, made at an interval of several hours, are constant. For these weighings the sulphate is let cool, protected from the moisture of the air, by cleaning the glass tube at the moment it is withdrawn from the stove.

The time which is required for the elimination of the excess of acid varies according to the kind of sulphate and the quantity operated upon. In general, a constant weight is reached more rapidly if the sulphate is in a pulverulent state. When this last point is reached the boat is put at the bottom of a platinum tube, which is placed in a muffle, so that the open end of the tube projects slightly from the mouth of the muffle. Heat is then gradually applied up to the temperature necessary for the total decomposition of the sulphate.

The use of the platinum tube has a double purpose: 1, to ascertain by examination of this tube if there has been any loss of spirting; 2, to withdraw the oxide, if reducible, from reducing action by permitting the access of the external air around the oxide; the internal atmosphere of a muffle heated with gas and closed having almost always a reducing action.

The author has never met with a trace of spirting in his determinations, and considers that if the temperature is raised gradually it never occurs with sulphates free from excess of acid.

The elimination of the acid is only considered as complete when the weighings, after two successive calcinations, give the same results. This does not exclude a counter-experiment by the moist way, *i.e.*, the resolution of the oxide in hydrochloric acid, and the application of a few drops of barium chloride to the diluted and heated solution.

In order to weigh the oxide the muffle is let cool down to dull redness, and at this moment the boat is rapidly withdrawn from the platinum tube and let slide into a glass case, previously heated in the sulphur-bottle. The case is closed and the boat weighed when cold.

In a further paper the author proposes to show the application of this method to copper, zinc, nickel, iron, aluminum, and chromium.—*Comptes Rendus; Chem. News.*

SEWER GAS AND ITS DANGERS.

By G. W. McCASKEY, Ph.B., M.D., Professor of Theory and Practice of Medicine in the Fort Wayne College of Medicine, Fort Wayne, Ind.

THE amount of skepticism that exists both inside and out of the profession with reference to the pernicious influences of sewage emanations seems to furnish a sufficient reason for collecting and presenting the most important evidence bearing upon this subject. The evidence is, it must be admitted at the outset, rather contradictory in many instances; but this disagreement is common to questions having to deal with the endlessly varying conditions of living beings, still further complicated by the intricacies of the social organization; and indeed, in order to establish its position among the dangerous environments of life, it is only necessary to prove that it is sometimes pernicious and fatal, however frequently it may appear to be innocuous. There are instances where sewer air is breathed for an indefinite period without any appreciably injurious effects; on the other hand, exposure to its influences even for a short time is occasionally followed by most disastrous results. Contradictory as these observations are, it is believed that they are susceptible of harmonization.

The germ theory of disease, which is constantly approaching more nearly to the status of an accepted dictum, furnishes a ready and rational explanation of the *modus operandi* by which sewer gas conveys the principles of disease; and also explains why it sometimes fails to do so. While it cannot be regarded as demonstrated in any other disease than relapsing fever, and possibly also diphtheria, there is certainly no other theory that accounts for the phenomena of a certain class of diseases in so complete and satisfactory a manner. The evidence in its support, drawn largely from analogy, is so overwhelming as to not only warrant, but challenge, our acceptance, at least provisionally. The researches of Tyndall, Beale, and others have shown that in an atmosphere of ordinary purity there are floating countless germs of living organisms, which only require sufficient nourishment and a favorable environment to develop into organized bodies. For certain organisms these conditions are naturally fulfilled in the atmosphere itself. Usually, however, there is required either an entirely different medium from that ordinarily respired, or a great modification of it before very favorable conditions are presented.

Now it is very well known that the development of micro-organisms in a liquid medium depends, *ceteris paribus*, upon the amount of decomposing or decomposable organic matter which it contains. Pasteur's fluid, for example, contains tartrate of ammonium, 100 parts; cane sugar, 1,500 parts; phosphate of calcium and sulphate of magnesium, of each 2 parts; phosphate of potassium, 20 parts; and water 85.76 parts—the sugar and ammonium furnishing by decomposition the nascent H₂N and C which are required for the building up of organic structures. These same materials can be supplied in an equally efficient form, but with less precision as regards quantity, by a simple infusion of animal or vegetable tissues. In such media as these microscopic germs find an abundant nutrition, and develop with a prolificness that is astonishing, and almost inconceivable; while in water free from organic contamination, development and propagation are alike impossible.

What is true here is also true of micro-organisms in the air, especially because when polluted it is usually owing to contact with putrescent liquids. Germs may be mechanically introduced into either pure air or pure water, but their growth ceases at once. But if, on the other hand, the air contains in adequate amount those elements which go to make up the material basis of life, then they will, under favorable conditions, grow and multiply.

If these biological principles are correctly stated, the next thing to ascertain is whether or not the air contaminated with