greater completeness—that the great French chemist had gone beyond his English predecessor in degree only, and not in principle. In considering this achievement, it is impossible to avoid being struck by the circumstance that Berthelot came very near, without actually realising the main condition which makes possible the production of sulphovinic acid from ethylene as a practical and even a technical process, viz., the heating of the sulphuric acid. There is not a hint in any of Berthelot's descriptions of his experiments which, so far as I have been able to ascertain, indicates that he regarded the temperature of the acid as important in determining the absorption of ethylene. This, if my reading of his work is correct, is all the more remarkable because it was he who first distinctly called attention to the necessity for applying heat to insure complete combination between ethylene and hydrogen chloride or iodide.*

The discovery that the heating of the sulphuric acid promotes the absorption of ethylene and the consequent formation of ethylsulphuric acid was made, as Prof. Jungfleisch points out, by Gorisnoff and Butleroff in their paper of 1873 ("Ueber die Polylenen und die Umwandlung von Aethylen in Aethylalkohol").† It is hardly fair, however, to attribute to these chemists the absolute and unqualified condemnation of Faraday's result. All that is claimed is that the absorption is facilitated in their particular form of apparatus by raising the temperature; they introduced a new principle which raised the academic results of Faraday and Berthelot to the rank of a potential practical process, and they even suggested that the success of Berthelot's synthesis may have been due not simply to intimate mixture of the gas and acid brought about by the mechanical agitation, but to the rise of temperature caused by the agitation. May it not also be suggested that Faraday's impure ethylene containing alcohol and ether vapours, when brought into contact with sulphuric acid, was absorbed with a sufficient elevation of temperature to increase the percentage of "synthetical" sulphovinic acid?

Coming now to the first step, the synthesis of ethylene from acetylene, there is of course no difference of opinion between the contending parties. The palm must be awarded to Berthelot, and the chemical world in general has long acknowledged its indebtedness to the illustrious French chemist for the discovery of this elegant and fundamental point of departure in organic chemical synthesis. The question raised by me in 1895, however, centred entirely round the synthesis of *vital* products; ethyl alcohol being regarded as such a product in opposition to the views concerning its origin held in the time of Faraday and Hennell. Of course neither Faraday nor Hennell could have realised the significance of their discoveries from this point of view in 1828, and all arguments directed against this work on account of their silence on this point appear to me quite futile.‡ Berthelot in 1855 had of course fully realised his achievement, for he says in his paper of that year already referred to:—"C'est la première fois que l'alcool est obtenu sans l'intermédiaire d'une fermentation" (*loc. cit.* p. 104). Also in his "Leçons sur les Méthodes Générales de Synthèse" (1864):—

"Voilà donc la synthèse du premier composé qui puisse s'appeler naturel."

From this it would almost appear that he did not consider Wöhler's synthesis of urea as the synthesis of a natural product or else that it was not "complete" or "total" from his point of view. The whole question of what is understood by "complete synthesis" is thereby raised, and this, as I have previously said, is entirely a matter of definition. It is certainly due to the work and teaching of Berthelot that the term organic synthesis has become associated in modern chemistry with the idea of totality or completeness in the sense of setting out from a compound, such as a hydrocarbon, capable of being formed directly from its elements. From this point of view then, Hennell's

synthesis was not "total," because Faraday's sulphovinic acid had, as I contend, been formed at least partially from ethylene obtained from alcohol. But in this sense neither was Berthelot's first synthesis complete, because in the experiment above described it is clear from the admission that the ethylene used by him was purified so as to free it from ether vapour that his gas was also prepared from alcohol. It was not until he had repeated this experiment with ethylene from acetylene that the synthesis became total. A reference to this repetition with synthetical ethylene is made in his work "De la Synthèse en Chimie Organique" published in 1860 (p. 183), already quoted, but no description of the experiment is given. It would have been no easy task at that time to prepare 31–32 litres of ethylene from acetylene. Possibly the repetition was a small scale experiment. At any rate I have not been able to find any published details of this work, although I do not profess to have exhausted the literature, because the point appears to me quite immaterial. It is significant that in this same work he describes the separation of ethylene from illuminating (? coal) gas through the formation of the iodide and the decomposition of the crude product by potash. From 300–400 litres of the gas he obtained by this method ½-litre of olefiant gas.

If it be conceded in accordance with the evidence submitted that some unknown proportion of Faraday's sulphovinic acid was synthetical, then the difference between Hennell and Berthelot resolves itself simply into the use by the former of ethylene derived from alcohol after Faraday had shown that the destructive distillation of oil gave rise to this gas among other products, whereas Berthelot showed that ethylene *could* be obtained from carbon and hydrogen, *via* acetylene, and that such ethylene *could* be converted into alcohol, although for practical experimental purposes he also used the gas obtained from alcohol. To maintain that the ethylene obtained by the destructive igneous decomposition of oil renders the synthesis of any compound therefrom "incomplete," because oil is an organic (i.e., vital) product to start with, seems to me to be both an unnecessary and an unscientific limitation of the term organic synthesis. At any rate, Wöhler's synthesis of urea was, as has already been pointed out, in precisely the same position with respect to its generators. It is open for any extreme advocate of the narrower interpretation of synthesis to claim this discovery also for Berthelot, since chemical science is again indebted to the late illustrious French chemist for the proof that cyanogen compounds can be directly synthesised from carbon, hydrogen, and nitrogen. From the biochemical point of view, however, any process which, like destructive distillation, yields products devoid of life supplies materials which, for synthetical purposes, may be regarded as legitimately "inorganic," as though they had been formed by the electric arc or the electric furnace. Hennell's synthesis of alcohol still appears to me to be as "complete" as the state of chemical science in his time admitted. With Wöhler he must, I think, be credited with having been the first to prepare a vital product by purely chemical methods, although, unlike Wöhler, he was unable to realise that he had achieved a biochemical triumph. This decision I should like to repeat in conclusion, is arrived at "without prejudice," and with the most profound and undiminished admiration for the beautifully perfected methods of the late M. Berthelot.

Yorkshire Section.

Meeting held at Station Hotel, York, on Monday, April 25th, 1910.

PROF. W. M. GARDNER IN THE CHAIR.

A COMPARISON OF DIFFERENT TYPES OF CALORIMETERS FOR SOLID FUELS.

BY L. L. LLOYD PH.D., AND G. W. PARR.

The experiments here recorded were carried out in order to compare the relative value of different types of calorimeters for the determination of the calorific power of solid fuels.

* See, for example, his "De la Synthèse en Chimie Organique," Soc. Chim. de Paris, 1860; *Leçons de Chimie*, etc., p. 184. There is some discrepancy between this and the published statements in Berthelot & Watts, both of whom state that H₂I and H₂Br combine with ethylene at 100°, but that H₂I does not.

† *Annalen*, Vol. CLXIX., p. 146.

‡ It is scarcely necessary to point out that the recognition of alcohol as a vital product is mainly due to the immortal work of Berthelot's great compatriot, Pasteur.

The following calorimeters were used—Bryan Donkin, Roland Wild, Darling, and the Lewis Thompson.

The Bryan Donkin Bomb Calorimeter.—The experiments were all carried out, using water in the calorimeter at the temperature of the room, and the combustion was effected in each case under a pressure of 20 atmos. Ignition of the fuel was made by means of fine platinum wire, and contact made for about three seconds, so that the heat developed by the current may be neglected. No correction has been made for the nitric acid formed during combustion.

The apparatus gives consistent results with finely powdered coals, but coke requires the passage of the current for some ten to fifteen seconds in order to start combustion, and consequently untrustworthy results are obtained. The coke is, however, completely combusted when mixed with a long flaming coal.

The Roland Wild Calorimeter is a simple modification of Parr's calorimeter, the fuel being burnt with sodium peroxide and ignition effected by means of a piece of nickel wire half an inch long, which is heated to redness and dropped through a valve into the combustion chamber, the valve being then closed as quickly as possible. The nickel wire being very small, no correction need be applied. As in Parr's calorimeter, the correction for the heat of combination of the combustion products of the fuel and sodium peroxide is assumed to be equal for all kinds of fuel; this amounts to 27 per cent. of the total heat produced, and therefore 0.73 grm. of fuel is taken instead of one grm.

With long flaming coals a flash is produced in the form of a flame projected through the valve, but with non-bituminous coals the combustion takes place quietly. Coke, unless very finely powdered, is with difficulty ignited; this, however, may be overcome by mixing the coke with a known quantity of dry tartaric acid for which a correction must be made.

The sulphur and phosphorus in the fuels are completely oxidised, and may be estimated in the melt by the usual methods.

The Darling Calorimeter.—The fuel was combusted as completely as possible, the time required for combustion was about 10 minutes, from 3 to 4 litres of oxygen being used, no corrections being made for unburnt fuel.

The Lewis Thompson Calorimeter.—No corrections were made for unburnt fuel.

A summary of the results is given in Table I., and the results compared, in percentages, with the Bryan-Donkin as standard in Table II.

Lewis Thompson Calorimeter.—This apparatus gives fairly comparative results for bituminous coals. The best results appear to be obtained by diluting the fuel as largely as possible with the chlorate mixture; a practical limit is, however, reached, at which point the combustion starts with difficulty, and probably owing to the presence of fused chloride, the charge soon goes out. For non-flaming coals, the results are untrustworthy, the amount of unburnt coal in many cases being very high.

Darling Calorimeter.—This calorimeter is only of value for bituminous coals, the complete combustion of non-flaming coals being very difficult.

Roland Wild Calorimeter.—The results for bituminous coals are in all cases higher in value than those obtained with the Bryan Donkin; this may possibly be due to the combustion of the hydrogen in the fuels. With anthracite and coke, practically similar results are obtained with the two forms of calorimeter mentioned above.

Brame and Cowan (this J., 1903, 1230) have obtained, with the Lewis Thompson calorimeter, results which are lower by 1.8 to 6.2 per cent. than those obtained with the Mahler bomb calorimeter, and Gray and Robertson (this J., 1904, 704) quote results which are similarly 0.9 to 1.3 per cent. lower.

The combustion of pure substances by means of the Bryan-Donkin and the Roland Wild calorimeters is being carried out.

TABLE I.

Coal.	Bryan Donkin.	Roland Wild.	Darling	Lewis Thompson.
A. Long flaming ...	M. 7539(7) H. 7556 L. 7402	7624(8) 7664 7400	7453(6) 7562 7325	7276(6) 7522 6814
B. Long flaming ...	M. 6318(6) H. 6370 L. 6243	6567(6) 6583 6527	6287(6) 6384 6107	6324(6) 6476 6232
C. Anthracite.	M. 7321(6) H. 7360 L. 7301	7316(6) 7389 7278	6530(7) 6080 6120	• • •
Di. Coke in very fine powder.	M. • H. • L. •	7122(6) 7160 6933	• • •	• • •
Dil. Coke results from mixture.	M. 7045(6) H. 7108 L. 6972	7092(6) 7114 7056	6638(6) 6844 6048	• • •
E. Salicylic acid ...	M. 4732(6) H. 4884 L. 4935	4976(6) 4999 4935	• • •	• • •

TABLE II.

A.		+1.13	— 1.14	—3.49
B.		+3.92	— 0.49	—0.1
C.		—0.068	—10.81	
Dil.		+0.67	— 5.78	
E.		+5.1		

A & B. Bituminous Coals.

C. Anthracite.

Di. Finely powdered coke.

Dil. The results were calculated for coke, but the experiments carried out with coke and a bituminous coal of known value.

E. Salicylic acid was used as a standard.

• Results in these cases were so variable as to be unreliable.

DISCUSSION.

Mr. S. H. DAVIES asked if any other substance besides salicylic acid had been used for the experiments, and if so had a separate calculation been made for every different kind of substance used.

Mr. T. FAIRLEY asked what time was taken for the maximum rise of temperature. A correction should be introduced for the heat of decomposition of such substances as sodium peroxide and potassium chlorate where these were used in the combustion. This might be done by burning something of definite composition like pure carbon.

Mr. W. GATHORNE YOUNG asked if the author had any experience of the Cook modification of the Mahler bomb. It was generally recognised as the most reliable instrument, and was used at the National Physical Laboratory. He welcomed the appearance of a reliable instrument at a low price, which would make it more popular than the existing instruments for precise fuel calorimetry. The Bryan Donkin calorimeter gave maximum and minimum results nearer to the average than the Roland Wild calorimeter according to the experiments in Tables 4 and 5, but these differences were not important. Had any experiments been made with liquid fuels? He suggested pure ceresin as a standard substance. Could barium peroxide be substituted in the case of liquid fuels, since it was more stable.

Mr B. G. McCLELLAN referred to the incompleteness of the combustion of naphthalene which had been noted by several investigators when this substance is used for the determination of the water equivalent of bomb calorimeters, and said that phthalic anhydride and sugar were used with excellent results. His experience with the Cook modification of the Mahler bomb had shown that it gave very satisfactory results with solid fuels, but he found that considerable damage to the glaze lining resulted when this instrument was employed for liquid fuels such as petrol.

Mr. J. EVANS asked if the higher values for the Roland Wild figures were due to the use of sodium peroxide.

PROF. A. SMITHELLS said that in fuel calorimetry, results often averaged out correctly, although in many

instruments corrections for variation and composition were ignored. The Lewis Thompson calorimeter, for instance, gave wonderfully accurate results for such a crude method.

Dr. L. L. LLOYD, in reply, said he had tried tartaric and other acids in addition to salicylic acid, and was now trying other substances containing higher percentages of hydrogen. Naphthalene on combustion did not burn completely. The time taken for the rise in temperature to the maximum was two minutes. He had not come across any account of the Roland Wild calorimeter before

these results were published, and thought they were all within the limit of experimental error. The calorimeter could not be used for liquid fuel, since it was impossible to mix such substances with sodium peroxide if traces of sulphur were present. An allowance was made for heat of decomposition of sodium peroxide by taking only 0.73 grm. of the fuel. Unless perfectly dry sodium peroxide was used the results were not reliable. Barium peroxide decomposed on heating, and was therefore not suitable.

Journal and Patent Literature.

PATENT SPECIFICATIONS may be obtained by post by remitting as follows:—

English.—8d. each, to the Comptroller of the Patent Office, W. Temple Franks, Esq., Southampton Buildings, Chancery Lane, London, W.C.

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I.—GENERAL PLANT; MACHINERY.

Boiler-incrustations; Influence of —, on the economy and safety of heating installations. E. Reutlinger. Z. des Ver. deutscher Ingenieure, 1910; through Stahl u. Eisen, 1910, 30, 929–930.

THE effect of incrustations in retarding the transmission of heat has been greatly overestimated; on the other hand they may cause dangerous overheating of the plates. Calculation shows that an incrustation 5.5 mm. ($\frac{1}{4}$ inch) thick may cause losses of heat amounting to 20–30 per cent. according to the temperature of the firing gases. Oil or fat is much more objectionable in this respect; a coating of tar 0.3 mm. thick caused a loss of 30–45 per cent. If the transference of heat is effected entirely by contact and not by radiation, the losses caused by incrustations may not, where hot gases form the heating medium, be more than one-third of the above amounts; but they are much greater where the heating is done by hot water or saturated steam—in cases as much as 70–80 per cent. In such cases (steam-heated evaporators, brewing coppers, etc.) incrustations should be most carefully avoided. Where the temperatures of the heating medium and of the material heated do not remain constant, the author comes to the following conclusions:—1. *Heating medium constant, heated fluid variable*.—(Steam-heated hot-water apparatus, surface condensers, etc.) For apparatus not too heavily worked and of moderately large size, the loss may amount to 15–30 per cent.; for smaller heating surface, or rapid circulation, as much as 50 per cent. 2. *Heating medium variable, heated fluid constant*.—(Boilers, and all arrangements in which steam experiences changes of pressure and temperature at different parts of the heating surface). The loss may amount to about 7 per cent. if contact-heating alone be considered; 5 per cent., if the heating be both by contact and radiation. The loss increases somewhat with the demands made upon the boiler; but even in a locomotive boiler in full work, an incrustation of 2–3 mm. thick will not cause a loss of more than 5–9 per cent. 3. *Both heating medium and heated fluid variable*.—(Feed heaters, economisers, spray condensers). For installations heated by saturated steam, losses of 20–35 per cent.; for those heated by gases or superheated steam, losses of 5–15 per cent. For very narrow tubes, or for heat-transference between moving liquids, the loss may amount to 50 per cent.—J. T. D.

PATENTS.

Crucibles. The Morgan Crucible Co., Ltd., and C. W. Speirs, London. Eng. Pat. 10,771, May 6, 1909.

A LIMIT is placed on the size of crucibles of the ordinary type by considerations of strength and rate of heating. The crucible claimed is provided with a central flue of such

shape that the transverse section of the trough of the annular crucible so formed is similar to that of an ordinary crucible.—J. W. H.

Pumping or propelling liquids and gases; Rotary apparatus for —. J. G. Calvert, Gothenburg, Sweden. Eng. Pat. 14,233, June 17, 1909. Addition to Eng. Pat. 214, Jan. 4, 1909.

THE screw pump or propelling apparatus claimed is a modification of that described in the main patent (this J., 1910, 141), by which the axial flow of the fluid through the pump is facilitated. At the outlet and inlet ends, the conical casing and the core which carries the helical vanes are made cylindrical. In the case of the pump with a stationary receiving chamber, vanes are secured to the stationary conical core with a view to destroying the circumferential motion of the fluid.—J. W. H.

Filtering liquids; Apparatus for —. J. Wilson, London. Eng. Pat. 433, Jan. 7, 1910.

THE apparatus comprises a filtering tank, having troughs of U- or V-section formed in the bottom, and containing a bed of filtering material such as sand and shingle. Perforated outlet pipes for the filtered liquid are arranged in each of the troughs. In order to permit of a thorough cleansing of the filtering medium at intervals, a separate set of perforated pipes is arranged in the troughs, these pipes being connected to a water supply. The filter-bed is cleansed by first blowing air through the outlet pipes, and then permitting an upward flow of clean water from the second set of perforated pipes.—A. T. L.

Filtering apparatus. D. J. Kelly. Assignor to Kelly Filter Press Co., Salt Lake City, Utah. U.S. Pat. 957,467, May 10, 1910.

MEANS are provided for exhausting the air from a filter tank during the time it is being filled and for supplying compressed air during the process of filtration, when the "hydraulic pressure" due to the liquid being filtered has fallen below a definite predetermined value. The supply of compressed air is automatically regulated by the pressure existing in the tank.—W. H. C.

Vacuum-filter. M. E. Hiltner, Deadwood, S. D. U.S. Pat. 957,658, May 10, 1910.

THE filtering medium is arranged upon an endless belt which passes over a pair of pulleys. The filtering surface is alternately put into connection with a vacuum- or a pressure-chamber. During the time the filter is connected with the pressure-chamber the surface is sprinkled by a suitable device.—W. H. C.