Meeting held at Burlington House, on Monday, June 6th, 1910.

DR. J. LEWKOWITSCH IN THE CHAIR.

THE FIRST SYNTHESIS OF ETHYL ALCOHOL.

BY R. MELDOLA, F.R.S.

In the course of an inquiry into the history of organic synthesis preparatory to the publication of my book on this subject in 1904* I came to the conclusion that with respect to the first synthesis of a vital product our countryman, Henry Hennell, was in precisely the same position, so far as concerned the artificial production of alcohol, as was Wöhler with respect to the synthesis of urea, which was accomplished in the same year, 1828. This conclusion was stated in my address to section B of the British Association at Ipswich in 1895,† and was repeated in the introductory chapter to the book above referred to. The conclusion at which I had arrived after a quite impartial and independent examination of the original papers by Faraday and Hennell was by no means new; it was simply an independent confirmation of views proclaimed at a much earlier period by such authorities as Chevreul, Wurtz, Fehling, Roscoe and Schorlemmer, Henry Watts, &c. The reopening of the question of Hennell's claim called forth in 1899 a counterclaim by the late illustrious French chemist, Berthelot,‡ from which has arisen a polemical discussion, the latest contribution to which is by M. E. Jungfleisch, the distinguished Professor in the Collège de France, who, in a remarkably lucid paper, argues the case for M. Berthelot both with great skill and with scientific moderation.§

Disputes about priority with respect to scientific discovery are, as a rule, unsatisfactory. I should not have ventured into the arena of polemics on the present occasion were it not that the eminence of M. Jungfleisch and the ability with which he attacks my conclusions, as such as to render his own deliberate verdict worthy of the most serious and respectful consideration. With every desire to give full weight to M. Berthelot's reclamation and to abandon my own position if found untenable, I have been induced by the publication of M. Jungfleisch's article to look once again and more critically into the evidence. As the result of this second inquiry into the claim of Hennell has been to confirm the conclusion formerly arrived at, it seems desirable that the chemical world should now be put in possession of the evidence upon which that claim was based. In submitting this evidence it is perhaps hardly necessary to point out that even if the case in favour of Hennell is considered as proved, the fame of the illustrious French founder of organic chemical synthesis suffers not the slightest diminution. The name of Berthelot will always be revered by the chemists of this country as by those of his own; it is but a small crumb from the rich table of his scientific labours that we claim on behalf of a comparatively obscure English worker, of whom, but for his untimely death, we should probably have heard more as a scientific chemist.

In order to limit the present discussion to those points which are actually at issue it will be necessary to recapitulate briefly the claim as advanced by myself on behalf of Hennell:—

(1) Faraday in 1825 had been investigating the illuminating gas produced by the destructive distillation of oil—the so-called "oil-gas." In the liquid condensed from this gas he had discovered benzene among other hydrocarbons, and among the "vaporous" constituents, olefiant gas. He observed that the latter was absorbed by sulphuric acid:—"Even olefiant gas itself is dissolved by it in

small proportions." "In determining the 'vapours' present in oil and coal gas it is an object to possess means by which their presence and quantity may be ascertained; and this I find may be done with considerable exactness by the use of sulphuric acid, oil, &c., in consequence of their solvent power over them." "Sulphuric acid is in this respect a very excellent agent. It acts upon all these substances instantly, evolving no sulphurous acid, &c." "When olefiant gas is present, additional care is required in analytical experiments in consequence of the gradual combination of the olefiant gas with the sulphuric acid. I found that one volume of sulphuric acid in abundance of olefiant gas absorbed about 7 volumes in 24 hours in the dull light of a room; sunshine seemed to increase the action a little, &c." "I find also that sulphuric acid will condense and combine with olefiant gas, no carbon being separated, or sulphurous or carbonic acid being formed, and this absorption has in the course of 18 days amounted to 84.7 volumes of olefiant gas to one volume of sulphuric acid. The acid produced combines with bases, &c., forming peculiar salts, which I have not yet had time, but which it is my intention to examine." From a paper "On New Compounds of Carbon and Hydrogen and on certain other Products obtained during the Decomposition of Oil by Heat," Philosoph. Trans. Roy. Soc., 1825, p. 340 *et seq.* (Read June 16th, 1825.)

(2) The following year Hennell, through Brande, communicated a paper to the Royal Society, in the course of which he says:—"I have also ascertained that hydrocarbon (referring to olefiant gas) with an additional proportion of sulphuric acid, affords a compound which is capable of uniting with salifiable bases and of forming a distinct series of products." Referring to Faraday he continues:—"He gave me some sulphuric acid which had been exposed to olefiant gas during some of his experiments on the products of the decomposition of oil by heat. It had absorbed about 80 times its volume of the gas, acquired a deep brown colour and a smell resembling oil of wine. It was saturated by carbonate of potash, carefully evaporated to dryness, and the dry mass digested in alcohol. A small quantity of a salt was obtained from the alcoholic solution having the crystalline form and general character of the salts I have been describing.

"Thus it would appear that hydrocarbon constituted of single proportionals, or 6 carbon and 1 hydrogen by weight, has the power of combining with sulphuric acid; and that whether it be evolved and then combined, as in the case of olefiant gas, or its elements separated from other compounds, as from alcohol, it forms precisely the same combination, sulphovinic acid." It may be added that the original object of Hennell's work was the investigation of the so-called "oil of wine," and that the paper was entitled "On the Mutual Action of Sulphuric Acid and Alcohol, with Observations on the Composition and Properties of the Resulting Compound," Philosoph. Trans. Roy. Soc., 1826, p. 240. (Read March, 9th, 1826.)

(3) Two years later Hennell sent a second instalment of his work to the Royal Society, in the course of which paper he states:—

"When alcohol and sulphuric acid in equal weights are put together without the application of any heat beyond that generated during the mixture, the most abundant and important product is sulphovinic acid, above one half of the sulphuric acid being converted into that peculiar acid by union with hydrocarbon."

He adds further that "the sulphuric acid loses half its saturating power by the union, and all the salts formed by the new acid are soluble." He finds that ether is produced by the distillation of sulphovinic acid and he proves conclusively that sulphovinic acid on distillation with dilute sulphuric acid is resolved quantitatively into its components, sulphuric acid and alcohol. The paper is entitled "On the Mutual Action of Sulphuric Acid and Alcohol, and on the Nature of the Process by which Ether is formed," Philosoph. Trans. Roy. Soc. 1828, p. 365 *et seq.* (Read June 19th, 1828.)

* "The Chemical Synthesis of Vital Products," Edward Arnold, 1904.

† Rep. Brit. Assoc. Ipswich, 1895, p. 649.

‡ Comp. rend., 1899, Vol. 128, p. 802. Prominent among the later supporters of Hennell's claim is P. Fritzsche; see Journ. Agr. Chem., Vol. 65, p. 597.

§ Revue Scientifique, Jan. 20th, 1910, pp. 129—140.

The above summary of the main conclusions must make it clear :—

(1) That Faraday was aware that ethylene was among the products of the destructive distillation of oil and coal.

(2) That Faraday was familiar with the use of sulphuric acid as an absorbent for ethylene and other hydrocarbons.

(3) That Hennell identified the acid produced by the combination of ethylene and sulphuric acid as sulphovinic acid.

(4) That Hennell discovered that sulphovinic acid was decomposed by heating with dilute sulphuric with the production of alcohol.

The chain of evidence seemed sufficiently complete to warrant the deduction that this was the first synthesis of ethyl alcohol and therefore of a vital product. It is urged by way of objection to this claim that neither Faraday nor Hennell nor their contemporaries made any specific reference to their having produced alcohol by a chemical process. So far as Hennell is concerned this criticism is, I venture to think, disposed of by a passage which makes it clear that he fully realised, as far as existing knowledge admitted, the results of his own experiments :—

"We may . . . form either alcohol or ether, using olefiant gas as the hydrocarbon base : for I have shown in my last paper (1826) that olefiant gas by combining with sulphuric acid forms sulphovinic acid, and the acid so produced forms either ether or alcohol, according to circumstances which are under perfect command." (Philosoph. Trans. Roy. Soc. 1828, p. 371. Italics are mine.)

Nothing could be clearer than this statement. If the discovery failed to rouse the same interest in the scientific world as the contemporaneous synthesis of urea from ammonium cyanate by Wöhler, this is explicable by the circumstance that alcohol was not in 1828 regarded as an organic compound in the old sense of being the product of a living organism.* Whether Hennell's synthesis was "complete" is in reality a matter of definition. All that was claimed by me in 1895 was that it was as complete as Wöhler's synthesis of urea, for the latter set out from a cyanogen compound which was prepared from nitrogenous "organic" matter. To this point, however, it may be necessary to return when the arguments against Hennell's claim† are being considered.

The historical record of the work done in connection with the synthesis of alcohol is so fully, and, on the whole, so accurately set forth by Prof. Jungfleisch that those who are interested in the question under discussion cannot do better than read his paper. On one point only may it be necessary to question the rendering of a particular paragraph in Hennell's original (1828) paper because important conclusions respecting Hennell's position are drawn from an expression which the author makes use of. In acknowledging Faraday's assistance he says, as will be seen from the full text quoted above :—"He gave me some sulphuric acid," &c. This is translated by Prof. Jungfleisch :—"Il me donna un peu d'acide sulfurique, &c." A point against Hennell is made out of this statement, because it is concluded therefrom that he was working with such a small quantity of material that any conclusions arrived at by him were thereby rendered doubtful. But this, as will be seen from Hennell's statements, is a gratuitous deduction. There is no evidence that he had in hand only a small quantity of Faraday's acid ; the passage indicates simply that out of the whole of the acid given to him he obtained a small quantity of the potassium salt after neutralisation by potassium carbonate and the extraction of the whole of the dried residue by alcohol. His identification of this salt as potassium sulphovinate seems conclusive, so far as covered by the terms "crystalline form and general characters." It may even be concluded that he was so far satisfied with

his diagnosis that he did not take the trouble to exhaust the residue of dried potassium salts by alcoholic extraction ; he had dissolved out enough for his purpose, and, as any modern chemist would do under similar circumstances, when he wanted more of the material he adopted the more direct method of preparing the compound directly from alcohol and sulphuric acid.

Passing now to the specific criticisms of the work of Faraday and Hennell, the first and perhaps the most important point urged by Prof. Jungfleisch is that the ethylene used by Faraday in saturating the sulphuric acid to the extent of about 80 volumes could not have been pure, because pure ethylene is not absorbed to anything approaching this extent at ordinary temperatures and pressures. In other words the presence of sulphovinic acid in Faraday's sulphuric acid is attributed primarily to the presence of ether and alcohol vapours in the olefiant gas used. Faraday, it is true, does not give any particulars respecting the state of purity of his gas in his 1825 paper ; neither does he specify the precise conditions under which the absorption by sulphuric acid was effected. There is certainly no warrant for the belief that the particular specimen of acid given to Hennell was saturated by the olefiant gas from oil gas. I quite agree with Prof. Jungfleisch that there is no justification for this statement. But Hennell himself has never made such an assertion ; all that he says in his paper is that the acid given to him "had been exposed to olefiant gas during some of his (Faraday's) experiments, &c. (italics are mine). The ethylene may have been, and no doubt was made by Faraday by the ordinary method after he had satisfied himself of the presence of this gas in oil gas. The assertion that "les mémoires de Hennell . . . prêtent parfois à la confusion ; par exemple, le gaz oléfiant, soumis par Faraday au traitement sulfurique, y semble indiqué comme provenant des produits de la décomposition de l'huile par le chaleur, &c.," is certainly an unwarrantable interpretation of Hennell's statement. Prof. Jungfleisch's translation of this passage in Hennell's paper is quite correct, so that this criticism appears to be all the more unjustifiable.

The contention that the whole of the sulphovinic acid contained in the sulphuric acid worked up by Hennell could not have been due to the ethylene absorbed must certainly be conceded, in the light of existing knowledge. Prof. Jungfleisch has done well to insist upon this point. The later discovery that pure ethylene is absorbed only to the extent of 1.4 volumes by 1 volume of sulphuric acid was made by Liebig in 1834* and has since become common knowledge. But in making this concession I venture to think that the validity of Hennell's claim is in no way affected ; his position may be said to have undergone quantitative but not qualitative modification as the result of later research. The deduction from Liebig's work and from the later experiments of Coriainoff and Butleroff (1870) is drawn by Prof. Jungfleisch in terms which will appear to impartial critics of the present controversy as extreme :—

"De telles observations ne laissent pas entrevoir la possibilité que l'acide sulfovinique trouvé par Hennell dans le produit qu'il tenait de Faraday ait eu l'éthylène pour origine ; cet acide sulfovinique, comme l'a montré Liebig, avait eu l'alcool et l'éther comme générateurs."

He thus even disallows the 1.4 volumes which Liebig allowed! And again—in claiming for Berthelot the discovery that purified ethylene and sulphuric acid combine to form sulphovinic acid he states :—

"Faraday a cru, il est vrai, l'avoir effectuée en 1825, mais, dès 1834, Liebig a montré que l'expérience de Faraday était fautive, l'éthylène n'étant pas absorbé par l'acide dans les conditions adoptées ; l'acide sulfovinique, sommairement caractérisé par Hennell en 1826 dans le produit de Faraday, avait été formé aux dépens des vapeurs d'alcool et d'éther contenues dans l'éthylène employé. Faraday n'a, d'ailleurs, jamais tiré de cette expérience une conclusion relative à la synthèse de l'alcool."

This last statement is perfectly true, but it has never been asserted that Faraday did make such a claim. All

* See the Address to Sect. B, Brit. Assoc. Rep. Ipswich, 1895. Also "Chemical Synthesis of Vital Products," introductory chap. p. 1.

† In speaking of Hennell's "claim" it must be understood that he himself never put forward any claim beyond the statement of the results of his experiments. Prof. Jungfleisch is perfectly correct on this point. It is we who claim that the first synthesis of alcohol must be credited to Hennell.

that he said was that, after having absorbed the ethylene, the sulphuric acid contained an acid capable of forming "peculiar salts," which he proposed investigating later. Had he done so he might have been the discoverer of the synthesis of alcohol from ethylene. As it was he handed over this work to Hennell, and the correct conclusion, that this was a synthesis of alcohol, although not expressed in modern phraseology, is quite clearly drawn by Hennell in the paragraph previously quoted. It cannot be conceded that:—

"De cette synthèse, d'ailleurs, Hennell, pas plus que Faraday, n'a jamais parlé." (Sec, *per contra*, Philosoph. Trans. Roy. Soc. 1828, p. 371.)

The position to which the discussion has brought us up to this stage may be summarised by the statement that while it is admitted by both parties in the controversy that Hennell effected the second step (the decomposition of sulphovinic acid into alcohol) he had not realised the first step because the sulphovinic acid used by him had, according to Liebig's experiments, originated from alcohol and ether vapours, and not from ethylene. How far this is true will appear from the argument already submitted—that some of the sulphovinic acid, if only a small proportion, resulted from the ethylene absorbed. Is not that sufficient to establish the principle that alcohol can be and was synthesised from olefiant gas? Is not the opposite position—that none of Faraday's sulphovinic acid originated from ethylene—an unwarranted straining of the evidence against Hennell? It is not the question of the establishment of a practical, working method that is at issue, since Berthelot's method, although an enormous advance over Hennell's, still cannot claim to have brought the synthesis to more than an academic stage. The observations of Faraday, followed by the experiments of Hennell, seem to me to have established the principle quite satisfactorily and to justify the claim which has been made with respect to Hennell's position.

It may perhaps be pertinent in the next place to consider somewhat more fully the nature of the evidence adduced by Prof. Jungfleisch in support of the contention that Faraday's 80 volumes of absorbed "gas" consisted chiefly of ether and alcohol vapours. Neither Faraday in 1825 nor Liebig in 1834 made any definite statement with respect to the strength of the sulphuric acid. Faraday says nothing about the state of purity of his olefiant gas, but Liebig is very precise in his description of the means employed to purify his gas by caustic potash and sulphuric acid. Faraday gives no description of his apparatus, but if the acid given by him to Hennell in 1826 was the same as that which he refers to in his paper of 1825—and the evidence points to this being the case—then the gas and acid in Faraday's experiment may have been in contact for more than a year. Liebig's gas, after purification, was passed into graduated tubes over mercury, and the sulphuric acid then introduced. The absorption to the extent recorded was complete in two days, and on further absorption was observed after three weeks. It does not appear to me that the arrest of the experiment after three weeks warrants the complete obliteration of Faraday's result. At least it may be pleaded that this is again a straining of the evidence by the Counsel for the Opposition against Hennell. Liebig, it is true, infers from his results that sulphuric acid has no specific affinity for ethylene—in other words, that there is no chemical combination, but simply a physical solution. I do not think this view is generally admitted; certainly all the works of reference which I have consulted are unanimous in attributing the absorption to the formation of sulphovinic acid.*

It now becomes necessary, in order that there may be no misunderstanding as to the real point at issue, to consider in turn the evidence upon which Berthelot's claim is based. What steps were taken by the illustrious French chemist which in the opinion of his supporters are considered sufficient to override all the earlier work of Faraday and Hennell? These steps, apart from side issues, are really

only two:—(1) The synthesis of ethylene from its elements, *via* acetylene; (2) The more complete conversion of ethylene into sulphovinic acid. Dealing with the second step first, it may be asked what improvement or modification was introduced by Berthelot, and was that modification so fundamental as to warrant the conclusion that success only was achieved thereby? A reference to the original publications will make it clear that the whole point of difference which, it is virtually declared, converted Faraday's failure into a success by Berthelot, is that the latter discovered that agitation was essential in order to insure complete absorption of the ethylene by the acid. This method was made known by Berthelot in 1854. Unquestionably it was an improvement; it is described moreover with a scientific precision in every way worthy of the reputation of its distinguished discoverer. The only doubtful point that arises is whether Faraday, whose skill as an experimenter none will challenge, was completely ignorant of this fact—whether, indeed, he had not shaken up his acid from time to time, and so contributed somewhat more than Liebig supposed to the amount of genuine "synthetical" sulphovinic acid in the acid which he gave to Hennell. However, as nothing specifically is said on this point in his paper of 1825, "judgment will be given by default" by those who are anxious to minimise Hennell's claim.* Returning to Berthelot's process, the following description is quoted from his well-known work, "Chimie Organique fondée sur la Synthèse," vol. i., 1860, chap. iii., p. 104:—†

A globe of 31–32 litres capacity was filled with purified ethylene, and into this 900 grammes of pure (boiled) sulphuric acid were introduced by instalments together with "some kilogrammes" of mercury. After continuous and violent agitation:—"Le gaz oléfiant s'est absorbé graduellement. Après 5300 secousses, l'absorption devenant trop lente, on arrêta l'opération. Elle avait duré quatre jours. 30 litres de gaz oléfiant se trouvaient absorbés; l'acide avait pris une odeur et une teinte analogues à celles d'un mélange d'acide sulfurique et d'alcool; il se troublait de même très légèrement par l'eau." The volume of gas absorbed was determined by measuring the volume of air admitted on opening the globe. The residual olefiant gas although diluted with air, "conservait la propriété d'être absorbable, soit par le brome, soit par l'acide sulfurique concentré avec le concours de l'agitation." From the acid thus saturated he obtained (by Hennell's method) a quantity of alcohol corresponding to 45 grammes of absolute alcohol, and to three-quarters of the ethylene absorbed. He characterises the alcohol and the ethylene obtained from it with all scientific accuracy. He adds in a footnote that 100 grammes sulphuric acid absorb 6.7 grammes of ethylene, corresponding to 120 volumes.

All chemists must pay homage to the memory of the man who carried out that piece of work. It surpassed the experiment of Faraday in scale (so far as we know), in the purity of the ethylene, and in the completeness of the conversion of the latter into sulphovinic acid. Combined with his proof of the convertibility of acetylene into ethylene by reduction, it will stand for all time as a classical achievement in chemical synthesis. But still it must not be forgotten that at the time of that work the science of organic chemistry had developed by some 29 years since Faraday's investigation of oil-gas. And after allowing for such development, both in theory and in practical methods—and Berthelot was himself among the most active in promoting such development—there yet remains in the mind of the impartial critic the conviction that Faraday had realised in principle that chemical combination which Berthelot afterwards brought about with much

* I attribute no weight to the argument that Faraday made no possible rejoinder to Liebig's criticism of his work. It is well known that he was most averse to enter into scientific controversy. It is possible that correspondence passed between him and Liebig with reference to this work, but if so we have, unfortunately, no record of it.

† The original paper from which the above description is reprinted, appeared under the title "Sur la reproduction de l'alcool par le bicarbonate d'hydrogène," in 1855 in the *Comptes rendus* Vol. XL., p. 102.

* Beilstein, 3rd. ed., Vol. I., p. 112, credits Faraday & Hennell with this discovery.

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.