most cases important that he should be given leave of absence at regular intervals, and encouraged to visit other scientific centres, whether at home or abroad, and to get into personal contact with the workers in his special field. The value of such 'refresher' intervals is difficult to exaggerate, whether to the individual or the institution which he serves. I am sure that there are few scientific men that would not benefit by such opportunities.

At no time in the history of physics has there been a closer co-operation and sympathy between the two great branches of physics, the experimental and theoretical. With the ever-growing complexity of experimentation and technique, it is rare in these days that a scientific man can claim to be proficient in both of these branches. There has thus arisen the need that these complementary divisions should be adequately represented in a Department of Physics. I am very glad to see that this has been recognised in your University

by the appointment of Prof. Lennard-Jones as professor of theoretical physics. In addition, the institution of research fellowships to attract to the laboratory young men who have shown marked ability for research is a step in the right direction, and I hope that it will be possible in the near future to add to their number. Under such excellent conditions we may confidently anticipate that this laboratory will fulfil the wishes of the donor by developing into one of our most important centres of training and research.

The University owes much to the public spirit shown by the city of Bristol and to the wise generosity of its citizens. I am sure that all scientific men are grateful to the University and to the donor, Mr. Henry Herbert Wills, whose generous benefactions have made possible the erection and endowment of this splendid laboratory dedicated to the pursuit of scientific knowledge.

Marcelin Berthelot.¹

By Prof. HENRY E. ARMSTRONG, F.R.S.

THE highest testimony we can give to the genius of a departed colleague is to study his work and its bearings, as in such exercise we are bound to find food for thought and gather inspiration for the future. As one of the older chemists, I would fain bear such slight witness as I may to the effect and value of Berthelot's achievements, being the more inclined to this task from having noticed, in the younger generation, a strange lack of interest in the pioneers who laid the foundations of the science they would master, now so mighty a structure—even a failure to understand the language these pioneers used. Continuity with the past is desirable, if only in order that we may understand the mental attitude of inquirers at the time they undertook their labours and be in a position to evaluate the mental development of their ideas.

Berthelot himself, who seems to have been extremely well read even at an early age, through his studies of the alchemists endeavoured to shed light upon the beginnings of chemistry and has thereby made clear the extraordinary difficulty of the task. Few to-day can appreciate his own rigid attitude towards atomic weights—how it was that he wrote $C=6$ and $O=8$ almost up to the last. If his intimate story could be written, it would probably be one of a mind ever striving to be scientific, yet held in the thrall of that superhuman force we term conservatism: the force by which our human society is held together and bound—the instinct of the herd—through the exercise of which we alone survive. On the other hand, it also separates us and especially from the past—as each new faith tends to antagonise the mind of its holder against an earlier form.

The rising generation has little if any understanding of the language spoken even so recently as in Berthelot's early days. There are, however, men

still alive who knew him, though not at the beginning of his career. It is his hope that these will give us of their best—the details that will enable us to follow his psychological development as a worker. For Pasteur we apparently have this information; the lessons we gain from it are invaluable. We seem to have the clearest understanding of his temperament and to be able to follow the gradual unfolding of his powers: to appreciate the masterly logic of his disposition: to see one continuous line of thought pervading all his labours, above all, his desire to serve his fellows. Berthelot offers a surprising contrast: we know so little of the man. His seems to have been a more universal genius. We are in sore need of some thread of continuity to guide us through the maze of his mind.

No episode in the history of chemical science is of greater interest than that of the discovery of dephlogisticated air by Priestley and the instant appreciation of its value by Lavoisier—conveyed in the magic word 'oxygen,' a word, however, the magic of which is not heard by young ears to-day, even in France. We can, in a measure, put ourselves into Lavoisier's position—our habit of thought still being largely that which he introduced in raising chemistry from an empiric art to a philosophy. We can no longer enter into the spirit of Priestley's work—we cannot even read his language with understanding. He has been commonly regarded as a pure empiric, but it is impossible to give credit to such estimate of his character. Berthelot, indeed, has questioned the truth of his representation of himself as an empiric. There are numerous passages in his works which may be interpreted as proof that behind all his inquiries there was both method and logical purpose, though maybe the purpose of intuition only. In the preface to his collected works (1790), he himself advocates philosophical studies in the following most remarkable passage:

¹ The English original of an 'appreciation' contributed to *Chimie et Industrie* in connexion with the centenary celebrations of Marcelin Berthelot.

I am sorry to have occasion to observe that natural science is very little, if at all, the object of *education* in this country, in which many individuals have distinguished themselves so much by their application to it. And I would observe that, if we wish to lay a good foundation for a philosophical taste and philosophical pursuits, persons should be accustomed to the sight of experiments and processes in *early life*. They should more especially be initiated in the theory and practice of *investigation* by which many of the old discoveries may be made to be really their own—on which account they will be much more valued by them. And, in a great variety of articles, very young persons may be made so far acquainted with everything necessary to be previously known as to engage (which they will do with particular alacrity) in pursuits truly original.

Nowhere, in our schools, have we yet acted up to these recommendations: we pay faint heed to them even in our colleges.

It is my good fortune to be able to overlook the period from 1865 to the present time—an interval of more than sixty years. I still remember how greatly, at the beginning of my career, I was fascinated by Berthelot's achievements, especially by reading his book on organic synthesis (1860). English chemists, I may say, have always been accustomed to pay close attention to the work of their French colleagues, especially that published in the *Comptes rendus de l'Académie des Sciences*. Not only is the concise form in which work is there presented eminently attractive but also, almost invariably, the force of genius is apparent in each communication, some new idea being stated, some advance recorded. Moreover, Berthelot compelled attention by his persistent appearance on the boards week after week. "What has he to say this week?" was the unconscious question we asked ourselves,

as each new number came to hand. Some point he always made. His method had the advantage that he made his points singly, so they went home.

Myself a student under Hofmann, Frankland and Kolbe, three of the great pioneers of our science on the organic side, I was specially prepared to appreciate Berthelot's early successes in synthetic chemistry. Few to-day will realise how great was the impression he made by his production of alcohol from ethylene

and water, of formic acid from carbonic oxide and soda, of acetylene from carbon and hydrogen—this last a particularly striking discovery at the time when we were beginning to specify organic chemistry as the chemistry of hydrocarbons and their derivatives. The barrier between the living world and the laboratory had indeed been broken down, in 1828, by Wöhler's achievement in preparing urea from ammonia and cyanic acid, but this was a case of Berzelian metameric change, not synthesis. Berthelot went much further and fired our imagination by

MARCELIN BERTHELOT.

showing how two and two might be put together to make four and in ways which were extraordinary in their directness and simplicity. Not only was our vision greatly extended but our ambitions also became boundless. We saw that the pass over the heights was won and that the conquest of the great kingdom beyond was a mere matter of time. We now seem able to make what we will, when once we know what we are called upon to do. No plant is safe from our imitation. The world is arrayed in laboratory products. Where our forefathers used woad as a source of indigotin, we use the pure pigment, made by thousands of tons in our factories. The

purple used by emperors in the past, laboriously extracted from multitudinous shell-fish, is now a relatively inexpensive laboratory product. We all but laugh at Nature—in fact, we can do more than she elects to do and arrogantly wear two gloves where she is content with but one; this, her prime secret, however, is her security, it seems, one for which Berthelot did not allow when he suggested that, in the future, perhaps we should live on synthetic food—an unpardonable suggestion for a man belonging to a gastronomic nation to make, a clear blot upon his shield. Chemists, it is true, are cooks but the French cook is by heredity an artist and his art one to be held in reverence, not interfered with by science and spoilt—as is brewing.

Still, the example which we perforce have followed, in our mad career of wresting so much of her prerogative from Nature, is that set by Berthelot. His methods of producing alcohol and formic acid long remained in abeyance—there was no call for their application; in recent years, however, they have been developed on the large scale. That of acetylene may be ranked as the most fruitful of his discoveries. Produced by the calcium carbide process, which in its essence is Berthelot's, this gas is now used not only as an illuminant but also in the form of the acetylene blowpipe, in welding and cutting steel. Acetylene is also used as the primary material in the production of acetic acid on the large scale. If, to-day, though we do not despise Nature, we are tending to put her aside and make coal the universal raw material, the initial fault was Berthelot's; if to do so be a virtue, the credit lies equally with him.

Regarding Berthelot, as we must, as the parent of synthetic organic chemistry, we need to be clear in our definition of synthesis, in view of the too general tendency to attach loose meanings to the terms we use and of the danger we run, especially at legal hands, of our terms being misinterpreted by those who have no feeling for their use.

We do well to remember the caution given by Gerhardt in the preface to his "Introduction à l'étude de la chimie" (Montpellier, Fév. 1848):

J'ai cherché, autant que possible, à joindre, dans l'explication des phénomènes, la précision à la clarté, prenant en cela pour modèle les mathématiciens qui ne se servent jamais d'un terme sans en avoir préalablement établi le sens. Il faudra bien un jour arriver à écrire les livres de la chimie comme on écrit une géométrie ou une algèbre; c'est alors seulement que la chimie se répandra davantage dans les masses.

In the following passage Gerhardt uses synthesis as the antithesis of analysis:

Les opérations de la chimie sont de deux espèces: elle détermine les métamorphoses, soit par *analyse*, soit par *synthèse*; elle sépare de la matière toutes les parties dissemblables, ou, par un procédé inverse, elle unit ces parties entre elles; elle décompose ou elle récompose. L'activité inhérente à la matière est son moyen; le chimiste la provoque par le contact immédiat des corps hétérogènes; il la renforce ou l'affaiblit par l'intermédiaire des agents physiques, chaleur, électricité, lumière.

The word synthesis can only be given its proper

verbal meaning: that of putting together, in the fashion so perfectly set by Berthelot, by working upwards from simple to complex. To give an example, oxalic acid may be made analytically by oxidation of sugar; it may be synthesised through cyanogen, NC.CN. We have reason, however, to believe that in no case are compounds formed directly merely from their atomic constituents. The production of acetylene, in fact, is a highly complex process, involving as it does not only the resolution of the diatomic hydrogen molecule but also that of the carbon complex C_x , where x is an unknown but probably very high value. The equation may be written

In result only is the synthesis simple. If we accept Faraday's teaching, the process is still more complex, involving other factors which make the occurrence of change possible by the inclusion of the interacting materials in an electrolytic circuit. The same may be said of calcium carbide. There is good reason to believe that, in preparing this substance, by heating carbon with lime, in an electric arc, we are but producing calcium *in situ* and that the carbide is formed by the interaction of carbon and the metal—again under the influence of factors which together with these constitute an electrolytic circuit. The argument is equally applicable to the synthesis of alcohol from ethylene and of a formate from carbonic oxide.

The germ of Berthelot's synthetic achievements is already forecast in his first chemical essay—"Action de la chaleur rouge sur l'alcool et sur l'acide acétique" (*Annal. de Chimie*, 33, 295; 1851). This inquiry was undertaken to ascertain what would happen if *simple* substances were submitted to a red-heat, as nearly all organic substances of high atomic weight gave similar products, coal-tar being an example. His experiments proved that the hydrocarbons benzene and naphthalene, phenol and various other complex compounds were equally obtainable from the simple substances and so laid the foundation of pyrogenic synthesis. In his essay he insists, in the most definite manner possible, that the *synthesis* of benzene, naphthalene, etc., from their elements could henceforth be regarded as a *fait accompli*, inasmuch as acetic acid, from which he had prepared them, had already been produced synthetically, by Kolbe, commencing with carbon bisulphide.

Berthelot, therefore, from the outset, was prepared to give a wide definition of the term synthesis and obviously interpreted it in the sense of the passage quoted above from Gerhardt, whose writings will have been known to him. Moreover, he clearly recognises that Kolbe was his forerunner. The essay is striking testimony to the early development of his genius.

Milestones remain from which we may infer the state of knowledge at the time when his synthetic work was begun. The volume of Liebig's *Annalen* (81; 1852) in which Berthelot's essay is reprinted from the *Annales de Chimie* also contains Alexander Williamson's celebrated memoir on the formation of ether. In this, as is well known, the long

standing dispute over the relationship of alcohol and ether was finally settled and the constitution of the two compounds made clear. The student of to-day has difficulty in picturing the crudity of belief and the slight body of knowledge of his forerunner of those by no means far away times. Williamson's interpretation was a master stroke of genius but an absolute 'bolt from the blue,' taking into account the uncertainty, if not confusion, which prevailed. We know that when he undertook the inquiry, he had not in mind the problem of which, as he ultimately saw, it gave the solution. I have vainly sought for a clue to his mental development up to the stage when he suddenly took so great a step forward and defined for all time the alcoholic and etheric functions. The nearest approach to an explanation which may be offered is, that he was under the spell of Gerhardt's logical genius, as may be inferred from the consistent manner in which he (also his friend Kekulé) made use of 'typical' formulae from this time onwards. The alcohol-ether problem was ripe for solution, in fact, owing to the publication of Gerhardt's advanced theoretical views.

Another article of special historical interest, in connexion with Berthelot's work, in the volume of Liebig's *Annalen* referred to, is a letter from Hofmann to Liebig on "The Application of Organic Chemistry to Perfumery." Hofmann prefaces his account by directing attention to Cahour's work on the oil of *Gaultheria procumbens*, showing it to be a compound ether. He sees proof of the influence this had upon perfumers in the appearance in our first English Exhibition (1851) of numerous fruit ethers and describes the results of his examination of various samples which he had collected as a jurymen—he had found the 'pear oil' to be amylic acetate, the 'apple oil' amylic valerate, the 'pine-apple oil' ethylic butyrate, etc. To-day, such synthetic materials are used not only in perfumery but also, on a huge scale, as solvents in the cellulose and varnish industries. Berthelot soon afterwards became interested in the more complex ethereal salts and, in fact, began his synthetic campaign by building up fats artificially from glycerol and fatty acids, thus crowning the work of his great countryman Chevreul.

The inquiry that has received more attention perhaps than any other published under his name and probably is the most esteemed is that on etherification, in which he was associated with Pean de St. Gilles. Carried out with exceptional care and finish, dealing with a fundamental problem of the highest interest, akin to that which Williamson had dealt with in his interpretation of the conditions of metallic salts in aqueous solution, the inquiry excited the widest interest and was the prototype of many similar inquiries—none the less, even now, we are not fully informed nor agreed as to the precise nature of the process.

A branch of Berthelot's work which appealed to me in my early days was that upon oil of turpentine, in which again he appeared as a pioneer. He was the first to distinguish between the oils from various sources and especially to note the distinctive differences in optical properties.

Acetylene, if not his discovery, was first characterised by him and eventually synthesised from its elements. As this is the most fundamental of all syntheses and the gas is of great technical importance, his name will always be indissolubly linked by chemists with this hydrocarbon and its direct conversion into benzene.

Persulphonic anhydride and persulphonic acid are probably to be ranked as his most important primary chemical discoveries.

The most difficult chapter in Berthelot's work to appraise is the thermochemical, including that of explosive combustion. Ever a fundamentalist at heart, in this, as in all his other work, he sought for primary values. Commencing in 1865, it covers a wide range. The earlier data are probably uncertain in not a few cases. In this, as in most other sections of his work, he was opening up a new field of inquiry and methods had to be devised and developed. Such work, moreover, was scarcely suited to his imaginative temperament and impetuous mode of attack. It needs not only a highly developed sense of accuracy but also a persistence and patience and a self-denial which few possess. He came into conflict with Julius Thomsen, who was making thermochemical measurement his life work; usually, in the end, they were in agreement. Berthelot probably derived his interest in such work from the example set by his great countrymen Lavoisier and Laplace. He sought to generalise and so was led to formulate his well-known three principles. Ultimately he rendered a service which it is difficult to over-estimate by his invention of the calorimetric bomb, one of the most powerful engines of research yet devised. Maybe the principle embodied in its use will some day be utilised in the internal combustion engine, when oil is no longer available as a cheap fuel. Hitherto it has been used chiefly in estimating the calorific value of fuels but it has also been of service in analysis.

In fact, thermochemistry seems almost to be a subject of the past. Few to-day will realise how great was the interest we took in Berthelot's and Thomsen's results as they were published. Testimony to my own interest in the subject is to be found in the article "Chemistry," which I wrote for the "Encyclopædia Britannica" (vol. 5, ed. 9), published in 1876. I then quoted characteristic thermal data for most of the elements.

It is unfortunate that chemists now so rarely take thermochemical values into consideration. When this is done, interpretations which on the surface may seem plausible are often precluded. An example may be found in the enigmatic behaviour of carbonic oxide. As H. B. Dixon was the first to show, this gas in admixture with oxygen is incombustible when dried, although if the mixture be moistened it explodes, the rate of combustion being more rapid the more moisture is present up to a certain point. This behaviour has been interpreted as proof that combustion takes place through the agency of 'water' in accordance with the equation

This interpretation is precluded, however, by the

fact that the heat of combustion of hydrogen to liquid water is greater than that of carbonic oxide.

Wieland has shown that such an interaction apparently does take place under the influence of palladium black but he goes further: having observed that traces of formic acid are present in the liquid, he argues that formic acid is the first product:

and that this is deprived of hydrogen by the action of the palladium

As formic acid has a higher heat of combustion than carbonic oxide, the formation of the acid from the gas would be an endothermic process. On this ground, Wieland's modified interpretation of the combustion process must equally be ruled out of order.

On thermo- and electro-chemical grounds we may go even further and premise that carbonic oxide cannot be burnt by ordinary oxygen, even when all other necessary determinative conditions are realised. On the assumption that every chemical interchange is an electrolytic process, whenever 'conducting water' is present as a necessary element in the circuit in which interchange is effected, no action should take place at any potential below that developed in the formation of water from hydrogen and oxygen. To burn carbonic oxide, it is necessary therefore to use an oxidising agent of higher potential than ordinary oxygen, the heat of combustion of carbonic oxide being below that of hydrogen. Hydrogen peroxide is such an agent. This oxide is a product of the primary interaction when hydrogen is burnt

To explain the fact that a moist mixture of carbonic oxide and ordinary oxygen is explosive, we therefore need only to assume that on firing the mixture a minute quantity of water is decomposed: that the liberated hydrogen is then at once *peroxidised* and that the hydrogen peroxide thus produced serves to oxidise the equivalent amount of carbonic oxide. All the peculiarities in the behaviour of carbonic oxide on combustion are accounted for by these assumptions.

The halting use hitherto made of thermochemical data is due, in no slight measure, to a suspicion that the values, in not a few cases, are of doubtful accuracy. The difficulty of interpreting the observed values is a still greater bar. These are all based upon the study of molecular interactions, often complex in character, the direct determination of atomic values being impossible. The difficulty has been in part met, in recent years, by resort to thermo-dynamical reasoning and the introduction of 'electrolytic' methods.

We are in face of this difficulty especially in the case of carbon. Julius Thomsen's numerous calculated values for carbon compounds were all based upon Favre and Silbermann's determination (1842-43) of the heat of combustion of charcoal

carbon, made at a time when the methods were but imperfectly developed.

Berthelot, with Petit, made use of his calorimetric bomb in redetermining the value of carbon in its reputed three forms. They give as the means of closely accordant determinations the 'atomic values'

Diamond	94,310	gram-degree	Centigrade	units
Graphite	94,810	"	"	"
Charcoal	96,960	"	"	"

Apart from the question as to which of the forms the theoretical heats of formation of carbon compounds may properly be referred, we have yet to show that there are forms of carbon other than the diamond. Now that there is reason to suppose, in view of Sir Charles Parsons's inquiries, that diamond has never been produced artificially, the problem is raised to an acute stage. The probability is that the reported allotropes of diamond (graphite and charcoal) are hydrocarbons containing the minute proportion of hydrogen needed to prevent the carbon atoms from lapsing into the highly stable condition they present in diamond, owing to the symmetrical, uniformly tetrahedral arrangement of the atomic units. The very small difference between diamond and graphite, the relatively large difference between graphite and charcoal are both surprising. We cannot accept even Berthelot and Petit's determinations as final.

A worthy monument to Berthelot's memory and genius would be an institute charged with the duty of placing beyond question all the fundamental thermal values and of extending thermochemical study in general. A vast field of fruitful inquiry lies open in the hydrocarbons especially.

It is impossible to contemplate the long list of titles—about 1500—to which his name is attached without feeling that justice remains to be done to his phenomenal activity. We need a studied, critical analysis of his work, in which the logic of his progress is fitly displayed. The work of poets is considered line by line: their critics seek to reconcile the miracles they have wrought in words with the peculiarities, if not deficiencies, of their moral character. The characters of statesmen, of soldiers, sailors and explorers are often discussed in considering their deeds. Little has been done to analyse the psychology of the scientific mind, to define the special features of the scientific spirit, yet even poets have foreseen that it is the spirit of the future. With most wonderful prescience Shelley has said of man:

"All things confess his strength. . . .

The lightning is his slave; heaven's utmost deep
Gives up her stars and like a flock of sheep
They pass before his eye, are numbered and roll on!
The tempest is his steed, he strides the air
And the abyss shouts from her depth laid bare,
'Heaven, hast thou secrets? Man unveils me; I
have none!'"

Of such order was Berthelot. We need to have the strength of the scientific mind fully unveiled: its weaknesses too, so that we may seek to combat them in the future service of the world.