Hofmann on the grounds that a synthesis is only a synthesis if the compounds are built up from the elements. I would like to point out quite shortly that there are synthetic dyes, synthetic drugs, synthetic perfumes, etc., and we know that these products are produced neither in laboratories nor in factories from the elements. Terminology allows also synthetic precious stones, biosynthesis, etc., and has not followed Dr. McKie's exact definition. I admit that Wöhler's method for the production of urea might be more correctly termed a transformation than a synthesis; for in the year 1828 Wöhler was unable to produce urea either practically or theoretically from its elements. But in 1882, the situation had already changed, and Hofmann had a certain amount of justification in speaking of a synthesis.

The second legend, which Dr. McKie wishes to destroy, is that Wöhler's achievement sounded the death-knell of vitalism. I, for one, have never heard of this legend. Indeed, it is well known that even about twenty years after Wöhler's discovery, both Berzelius and Gerhardt, as well as many others, clung to the idea of the *vis vitalis*.

Had Hofmann's obituary of Wöhler been the only source for Wöhler's work and its consequences, he might have been considered to have originated such a legend; but this was not the case. One must also not forget, while criticizing Hofmann's words, that they occurred in an obituary, and not in a scientific dissertation, that they originated from his deep regard and respect for his great colleague and fellow countryman, who at the same time was one of the greatest chemists of his age.

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Nature, 153, 608 (1944).

It is curious that, in discussing this matter¹, Dr. D. McKie should make no reference to the classical experiments of Scheele. Out of consideration for space, I will do no more than quote the comment of Graebe² on the passage, cited by Dr. McKie, in which Wöhler questioned the significance of his preparation of urea:

"Dieser Einwand war aber schon durch Scheeles Versuche widerlegt, da dieser das Cyankalium nicht nur mittelst Kohle, sondern auch mittelst Graphit erhalten hatte. Auch teilte im Jahre 1828 Desfosses mit, dass beim Überleiten von Stickstoff über ein zur Rotglut erhitztes Gemenge von Ätzkali und Kohle sich Cyankalium bildet. Inbetreff des Ammoniaks waren auch schon die Reaktionen bekannt, nach denen der Stickstoff der Luft sich in Salpetersäure überführen und diese sich zu Ammoniak reduzieren lässt."

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¹ *Nature*, 153, 608 (1944).

² "Geschichte der Organischen Chemie", 55 (Berlin, 1920).

THE legend that I refer to appears in various textbooks and histories of chemistry and in works on general science. I have just read it in more than a dozen of them consulted at random. The sense in

which I used the term 'synthesis' is neither personal nor *ad hoc*, and it is the same as that used by Hofmann when he wrote "der Aufbau des Harnstoffs aus seinen Elementen" in the quotation that I gave. My criticism of Hofmann did not, and could not, derive from any difference in the interpretation of this term, but from his statement that Wöhler built up urea from its elements and that the intellect of the time hailed his unification of chemistry with joy, which is untrue in every detail.

Hofmann made this statement in no hasty ephemeral tribute, but in a long contribution filling 164 pages of the *Berichte*. However, although there are reflexions of his words among later writers, I did not actually ascribe the origin of the legend to him. Still, if there should be any doubt about Hofmann's views after what I have already quoted, let us turn to the Faraday Lecture for 1875, a discourse on Liebig given by Hofmann before the Chemical Society of London:

"... in 1828, Wöhler had demonstrated the possibility of building up from its elements this very urea, the formation of which, up to that period, had been supposed to take place exclusively under the influence of vitality—an experiment ever memorable, since it removed at a single blow the artificial barrier which had been raised between organic and inorganic chemistry."

This is not history, but nonsense.

Scheele's experiments are not classical in the history of the synthesis of organic compounds. In his researches on the colouring principle in Prussian blue (1782–83), the great Swedish chemist incidentally prepared, by the interaction at red heat of either charcoal or plumbago with potash (from vegetable sources) and sal ammoniac (from animal sources), the substance now known as potassium cyanide. Two of his reactants were derived from organic sources; their syntheses had not then been effected; and, moreover, it was not known at that time that the cyanogen group occurred in living matter. Scheele's production of potassium cyanide has therefore no historical significance in this field, and further comment on Graebe's statement is unnecessary.

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The Cedar Tree

THE cedar tree to which Mr. Alexander L. Howard refers¹ on p. 597, and before him the Rev. C. A. Jones, is Bernard de Jussieu's cedar tree in the Jardin des Plantes, Paris; it was planted in October 1734. Jussieu had been given the seed for it by Dr. William Sherrard, the distinguished patron of the Oxford Botanic Garden, and the great cedar tree there near Daubeny's laboratory was most probably planted at about the same time. The story about the transport of the little seedling in Jussieu's hat, or as the French call it *le fameux cèdre-bébé de Syrie*, is very charming; but it is a fairy tale.

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¹ *Nature*, 153, 595 (May 13, 1944)