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THE DEATH OF VITALISM AND THE BIRTH OF ORGANIC CHEMISTRY: WÖHLER'S UREA SYNTHESIS AND THE DISCIPLINARY IDENTITY OF ORGANIC CHEMISTRY

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> ...for I cannot, so to speak, hold my chemical water, and must tell you that I can make urea without need of a kidney or even an animal, be it man or dog.[1]

This excited passage in an 1828 letter from Wöhler to Berzelius remains one of the most often repeated quotations in the history of chemistry. It is rivaled in familiarity only by August Kekulé's account of the dream in which he conceived of the benzene ring, and if chemists know any chemical history, they will know the story of Wöhler's synthesis of urea from ammonium cyanate. Equally famous is the mythical story concerning the impact of Wöhler's synthesis on the science of organic chemistry, a story that has been repeated countless times in textbooks, lectures, and articles. The Myth has a number of features. Before Wöhler announced his synthesis, the story goes, chemistry was deeply divided into organic and inorganic realms. Organic compounds, derived from plant and animal sources, were less stable, more prone to decomposition, and had compositions more difficult to ascertain by elemental analysis. Whereas inorganic compounds followed the laws of chemistry and were easily analyzed and synthesized, organic compounds could be made only in plants or animals by a mysterious vital force that could not be replicated in the laboratory. Wöhler's synthesis of urea from inorganic sources, the mythical story continues, removed this artificial barrier between organic and inorganic chemistry. Chemists then realized that organic and inorganic compounds were governed by the same laws, and Wöhler's synthesis effectively unified chemistry. Furthermore, because Wöhler had succeeded in making an organic compound from its elements, the concept of the vital force was no longer necessary and vitalism could be thrown into the dustbin of failed theories. This mythical story surrounding Wöhler's urea synthesis made it into a classic *Experimentum Crucis* in organic chemistry.

Part of the Myth's appeal can be attributed to its apparently simple argument, which boils down to a syllogism: 1) only vital forces can produce organic compounds; 2) Wöhler synthesized urea, an organic compound, from inorganic sources; 3) therefore the concept of a vital force must be incorrect. This is not a logically valid argument, but there was more to the appeal of the Myth than simple logic. For the urea synthesis came to be seen as a decisive

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moment in the history of organic chemistry, serving as a convenient date to mark the beginning of organic chemistry as a discipline. As we shall see below, it seems likely that both of these attributes of the Myth–the birth of organic chemistry and the death of vitalism–were created shortly after the synthesis itself, by Wöhler and his closest colleagues. Historians now agree that the synthesis, at least by itself, could have had, and indeed had, little real impact on vitalism. Nor could a single experiment have been a founding moment for the discipline of organic chemistry. This is not to say, however, that Wöhler and his colleagues deliberately fabricated a false story; the origin and evolution of the Wöhler Myth reveal both how Wöhler and his contemporaries came to view the significance of the synthesis for chemistry, and how the story served for subsequent generations of chemists as a disciplinary founding myth. While all chemists view Lavoisier in the role of the founding hero of modern chemistry, organic chemists see Wöhler's synthesis as one of the founding episodes for their subdiscipline, and its importance is reflected in the ubiquitous appearance of the Myth, in all its variants, in lectures, textbooks, and the early popular histories of chemistry.

This essay is an exploratory, by no means exhaustive, study of the origins and history of the Wöhler Myth, its incorporation into the popular and textbook literature of organic chemistry, and the important role it has played in establishing and maintaining the disciplinary identity of organic chemistry.[2] The first part will look at the origins of the Wöhler Myth during the first half of the nineteenth century. Understanding what the original participants said and did is crucial for the later interpretation of their words and deeds. Surprisingly, some of the story surrounding the early appearance of the Myth, particularly in textbooks, has not been documented previously by historians. The second part outlines the dissemination of the Myth by means of chemical lectures, textbooks, and popular and scholarly histories from the late nineteenth century to the present. This section will be mainly historiographic in character, using the existing accounts to draw additional conclusions about the meaning and historical importance of the urea synthesis. The final section suggests some ways in which the Wöhler Myth created and still maintains a disciplinary identity for organic chemistry.[3]

This study draws on a limited set of textbooks and popular histories.[4] The sources examined were constrained by availability, resulting in a sample skewed towards Germany in the nineteenth century and the USA in the twentieth century.[5] In any case, it would be extremely difficult to acquire a properly representative sample of textbooks: there is no central index of textbooks, so it is difficult to know how many texts in one or more languages have been published in the last 150 years; libraries generally do not retain many textbooks on their shelves, especially those published before 1960; nor, unfortunately, does the mere preservation of a text indicate its popularity or its influence on subsequent texts. However, because of the homogeneous and repetitious nature of many versions that I *have* found, the sample used here would seem to make it possible to draw significant conclusions.[6]

The Origin and Dissemination of the Wöhler Myth, 1828–1936

For those who are unfamiliar with the story of Wöhler and urea, the historical 'facts' are fairly straightforward.[7] Wöhler entered the University of Heidelberg to study medicine, and became interested in chemistry after attending Leopold Gmelin's lectures. Gmelin encouraged Wöhler to study with Jakob Berzelius in Sweden, which he did during the winter of 1823 and 1824. In the latter year, Wöhler published a short communication describing the synthesis of oxalic acid, a natural constituent of rhubarb and other vegetables, from cyanogen and aqueous ammonia. He also noted the presence of a second product, an unidentified white crystalline substance. In a now well-known paper published in 1828, Wöhler identified this product as urea, and noted that urea was always present as a product when he attempted to produce ammonium cyanate, either by combining cyanic acid with ammonia, or by the double decomposition of silver cyanate and ammonium chloride. The appearance of urea as a product was entirely unexpected, for theory predicted that cyanic acid and ammonia should produce a compound with the properties of a salt. Urea was not a salt, and it did not possess any of the properties expected for cyanates.[8] Furthermore, until that time, urea had been known only as a natural product, that is, as an organic substance isolated from animal sources.

In the famous 1828 letter to Berzelius, Wöhler explained the rationale for his synthesis. It was to be a minor test for the existence of what would soon be called isomers.

> I considered it possible that in the union of cyanic acid with ammonia, the elements could combine in the same proportions, but in another manner and in doing this, for example, a vegetable salt base or something similar could perhaps be formed. ... [The formation of urea] would thus be an indisputable example that two entirely different bodies could contain the same proportions of the same elements, and only a dissimilar kind of union brings out the peculiarities in the properties.[9]

Although he was clearly excited about his unexpected result, Wöhler refrained from speculating on the importance or significance of the synthesis either in the published paper or in his letter to Berzelius. In the paper, he remarked that the formation of urea from ammonium cyanate was a 'curious fact that presents an example of the artificial production of an organic, in fact a so-called animal substance from inorganic substances.'[10] At the end of the discussion, Wöhler cautiously hinted at the importance of the synthesis as another example of isomeric relationships between compounds:

> I reserve all considerations that naturally present themselves in consequence of this fact, especially concerning the compositional properties of organic substances, in relation to the identical elementary and quantitative organic substances in compounds with very different properties.[11]

Nowhere in the paper did Wöhler mention explicity any impact of the synthesis on vitalism, but it seems likely that he had precisely such an impact in mind when he wrote the paper, as in the title and throughout the text he explicitly referred several times to an 'artificial' (*künstlich*) formation or 'artificial' urea. He also obliquely refered to vitalism in his letter to Berzelius, by asking if his urea synthesis truly was an example of producing an organic compound:

> This artificial formation of urea: can one consider it as an example of the formation of an organic substance from inorganic substances? It is peculiar that the production of cyanic acid (and also ammonia), always requires an originally organic substance; a *Naturphilosoph* would say that the organic has not yet disappeared from either the animal coal, or in the cyano compounds formed from them, and therefore an organic body can always be reproduced from them.[12]

Wöhler's meaning here has intrigued commentators for generations, and his reference to a *Naturphilosoph* has been used in support of both sides in the long-standing debate about whether Wöhler made urea from the elements, or whether Wöhler himself considered his synthesis to have been accomplished from the elements. Throughout his correspondence with Berzelius, however, Wöhler can clearly be seen as a strong opponent of *Naturphilosophie*, of which he regarded vitalism to be a manifestation. Wöhler's reference to a *Naturphilosoph* was therefore not a reference to himself, but can be read as a sneering comment about what a vitalist would think about his synthesis.

Given its later fame, there are surprisingly few known contemporary accounts of the immediate reaction to Wöhler's synthesis. The secondary literature on the synthesis usually trots out one of several standard responses – by Berzelius, Justus Liebig, Jean-Baptiste Dumas and the physiologist Johannes Müller –that date to within ten years of the synthesis. Many authors do mention other 'accounts,' such as Hofmann's 1883 obituary of Wöhler, that would appear to substantiate the Wöhler Myth, but these appeared only well after it had become firmly ensconced in chemistry's mythology, and in fact are based on earlier versions of the Myth.

Berzelius' immediate reaction is recorded in an enthusiastic response to Wöhler's letter:

> After one has begun his immortality in urine, no doubt every reason is present to complete his ascension in the same thing–and truly, Herr Doctor has actually devised a trick that leads down the true path to an immortal name. Aluminum and artificial urea, certainly two very different things that follow one other so closely, will be added as gems to your laurel wreath, dear Sir! Should the quantity of artificial urea not suffice, one can easily supply a little from a bedpan. ... It is a very odd circumstance, that the nature of the salt so completely disappears when

> the acid and ammonia unite. This will certainly be very enlightening for future theories.[13]

Berzelius warned Wöhler not to be distracted by the discovery into neglecting his other research projects. While Berzelius clearly seemed excited about the discovery, he did not raise the issues of any implications for vitalism or the relationship between organic and inorganic chemistry. Nor did he provide an answer to Wöhler's question about synthesis 'from the elements.' What he considered odd or remarkable was the transformation of a salt-like compound into a compound that had none of the properties of a salt.

Indeed, some nine years later, for the third edition of his *Lehrbuch der Chemie* (1837), Berzelius wrote a twenty-six page introduction to the section on organic chemistry in which he carefully laid out the difference between the organic and inorganic worlds. He described how the elements obeyed different laws in organic compounds from those that they did in inorganic compounds, and referred to the living body as a factory in which chemical processes take place. He carefully described what he meant by a *Lebenskraft* that lay 'entirely outside of the inorganic elements, and is not one of its original properties, such as gravity, impenetrability, electrical polarity, etc.'[14] In 1837, the vital force was still very much a reality for Berzelius. Most intriguing, however, is that in the entire introduction, there is not a single reference whatsoever to Wöhler or the urea synthesis, much less its impact on vitalism.[15]

Müller and Dumas also offered early reactions to the synthesis, in public. Müller acknowledged the synthesis of urea from inorganic materials, but doubted that urea was a true organic compound in the first place.[16] In 1830, Dumas remarked that 'All chemists have applauded the brilliant discovery ... of the artificial formation of urea. More than anyone, I have felt the sincere desire to see the same principle applied to analogous cases for which it seemed to give the key.'[17] The 'key' to which Dumas referred was how the synthesis of urea from ammonium cyanate allowed inferences about the different arrangement of atoms in ammonium cyanate and urea.

It was in an 1837 address to the British Association for the Advancement of Science that Justus Liebig gave the strongest public indication that the importance of the synthesis lay in Wöhler's *artificial* production of an organic body from inorganic elements:

> The extraordinary and to some extent inexplicable production of urea without the assistance of the vital functions, for which we are indebted to Wöhler, must be considered one of the discoveries with which a new era in science has commenced. ... There are many bodies similar to urea, all of which will probably at a future period be produced by artificial means; ... I am certain that this object will be attained.[18]

While Liebig was optimistic about the future of organic synthesis, and was clear that urea could be made 'without the assistance of the vital functions', he said very little about the fate of the vital force at the hands of Wöhler.[19]

While these contemporary and near contemporary remarks on Wöhler's synthesis clearly indicate that Wöhler had done something important, none of them hints at what would later become the Wöhler Myth. There were suggestions about the possible impact of an artificial synthesis on vitalism, but at least publicly, the great immediate curiosity of the synthesis was the transformation of a salt into a non-salt, and the recognition that urea and ammonium cyanate must have identical chemical compositions. If Wöhler and his contemporaries saw the bridging of two different branches of chemistry, the imminent demise of vitalism, or the emergence of a new form of organic chemistry from the synthesis, they showed remarkable restraint in their public interpretations of it.

The earliest known published formulation of the Wöhler Myth appeared fifteen years after the event, in the first (1843) and fourth (1847) volumes of Hermann Kopp's *Geschichte der Chemie.*[20] According to Kopp, Wöhler's

> ... discovery destroyed the formerly accepted distinction between organic and inorganic bodies, namely that the former first develop under the influence of a vegetable or animal life force, while the latter may only be prepared artificially. ... The then supremely heeded distinction that the inorganic compounds may be prepared from their elements, but not the organic, was shown invalid by Wöhler's discovery of the preparation of urea from cyanic acid and ammonia.[21]

On a strict reading, Kopp's claim is slightly less exaggerated than the later version of the Myth which I summarized in the introduction. According to Kopp, Wöhler destroyed the previous distinction between inorganic and organic chemistry by showing that organic compounds could be synthesized in the laboratory. Kopp did not say, however, that Wöhler's synthesis destroyed grounds for belief in the vital force itself, an aspect of the Myth that would emerge more forcefully in later versions. Kopp's version was somewhat more restrained than many of its successors.

If Kopp was indeed the author of the first published version of the Wöhler Myth, Hermann Kolbe deserves credit for one of the earliest versions in a textbook. In 1847, Kolbe was chosen by the Vieweg Verlag to assume the editorship of its multi-volume *Concise Dictionary of Pure and Applied Chemistry* that was to appear over several years. Volume three, published in 1847, contained an entry for urea (*Harnstoff*) written by Georg Staedler, at the time an *Extraordinarius* at Göttingen.[22] Echoing Liebig, Staedler wrote that Wöhler's synthesis was the first example of the artificial production of an organic compound from inorganic materials. His comment was confined to four lines of a ten page article. Volume five of the dictionary, published in 1851, contained an article on organic chemistry penned by Kolbe himself, in which he offered a story similar to Kopp's, and emphasized how organic and inorganic chemistry had been united as a result of the urea synthesis.[23]

Kolbe developed the views expressed in this three-page dictionary article in the introduction to his 1854 *Lehrbuch der organischen Chemie.* In his "General Considerations," Kolbe laid out what was to become the prototype for nearly all textbook accounts of the Wöhler Myth. It is worth quoting at length:

> The distinction between inorganic and organic chemical compounds has its origin in the until recently widely held conception that the compounds composing the organs of animal and plant bodies, ... owe their formation to a quite mysterious inherent force exclusive to living nature, the so-called life force. The failure of earlier attempts to assemble artificially those chemical compounds produced in the organism, and the curious facility of almost all substances of organic origin to decompose readily, especially apparent in fermentation and putrefaction phenomena, and in the application of higher temperatures that ends in carbonization, could only serve to reinforce the view that these compounds must be subject to other quite special laws of combination [*Verbindungsgesetze*]. The conviction had generally increased that the synthesis of organic compounds could not be accomplished artificially, until we are capable of producing the organs themselves, ... a problem whose solution is even now considered unattainable.
>
> At the same time, this view subsequently offered a natural boundary for dividing substances of inanimate nature from those of organic origin, in which the latter still includes chemical compounds derived by artificial metamorphoses preceding the products of total decomposition.
>
> Epochal and momentous for our views on the nature of organic compounds was therefore the discovery made by Wöhler in 1828 that urea, known earlier as a product of the animal organism, could be assembled in a so-called artificial manner almost directly from its elements. With this important discovery, followed afterwards by many others, the natural dividing wall [*Scheidewand*] that separated organic from inorganic compounds came down; according to the previous criteria, urea could no longer be considered to belong exclusively to organic chemistry, and a classification of chemical compounds into organic and inorganic – using the former meaning – therefore lacked its natural basis.[24]

The similarity of Kolbe's story to Kopp's, and to subsequent textbook versions, is remarkable. The inclusion of an extended discussion of the impact of Wöhler's synthesis in the dictionary and textbook, shortly after the appearance of Kopp's *Geschichte*, strongly suggests that Kolbe adopted and elaborated Kopp's story. Because the 'natural' dividing wall between organic and inorganic had fallen, and because organic compounds still were subject to different laws of combination than inorganic compounds, Kolbe proposed an alternate definition of organic chemistry as the study of compounds containing carbon. Kolbe's was one of the first textbooks to define organic chemistry in this modern sense, and he was careful to distinguish between organic and physiological chemistry. Organic chemistry would be to physiology what inorganic chemistry was to geology. Organic chemists would study the composition and properties of those compounds deriving from nature, but not the natural processes themselves.

Although it is not entirely clear why Kopp initially wrote his version of the Wöhler Myth, it seems probable that he wrote up the story because he believed it to be a true account of the meaning of Wöhler's synthesis, most likely derived from private verbal claims made by Wöhler himself, or by Wöhler's close friend Liebig. Liebig and Kopp were well acquainted by the 1840s. As for Kolbe, Wöhler was one of Kolbe's principal mentors, so Kolbe is also likely to have heard the story directly. Already familiar with the story, and comfortable that Wöhler himself believed it to be true, the initial publication of the story in Kopp's *Geschichte* prompted Kolbe to adopt and elaborate it for his own text.[25] It also seems significant that at the time he was writing the dictionary article during the late 1840s, Kolbe was occupied with his own synthesis of acetic acid from inorganic compounds, and the comparisons to Wöhler's urea synthesis may have weighed heavily in his mind as he was writing.

Why the Wöhler Myth first appeared in print only in 1847, nearly twenty years after the event, is a more difficult question to answer. As we have seen, contemporary responses to the synthesis, neither published nor in letters, contained much hint of it. The mythical significance of the synthesis seems to have occurred to chemists only later, as they realized that a new subdiscipline of chemistry had been created in the 1830s. The story might well have have circulated among the small but growing community of organic chemists during the 1830s and 1840s, until Kopp decided to include it in his *Geschichte.*

Versions of the Myth began to appear in lectures, probably also from the 1850s. Certainly, at about the same time as Kolbe's textbook appeared, his close friend Edward Frankland incorporated the story into his lectures on organic chemistry at Owens College in Manchester. His lecture notes from early 1855 contain a condensed version of the Myth:

> *Vital Force* thought to form alone organic bodies. It was even thought that organic compounds could not be artificially made until organ of plants and animals were artificially produced. Hence this afforded convenient line of distinction between *inorganic* and *organic* compounds. *The artificial formation* of *Urea* by Wöhler in 1828 therefore *revolutionised* our ideas and *destroyed* this *natural boundary* between inorganic and organic bodies.[26]

Because of his close association and collaboration with Kolbe, Frankland has always been most strongly associated with the German tradition in chemistry, and as Russell noted in his biography of Frankland, the appearance of the Myth in Frankland's notes, so close to Kolbe's original, squarely places Frankland within that tradition.

Another example of surviving lecture notes, taken in 1871 during Johannes Wislicenus' course in organic chemistry at the Zürich Polytechnical Institute, also includes an abbreviated version of the Myth. Taken by Robert Gnehm, they begin with the note that the name 'organic' chemistry 'derived from the time when it was thought that organic bodies could only be considered under the effect of an organic species,' and that 'they could not be artificially prepared' until Wöhler made urea in 1828.[27]

Whether Kolbe's version directly influenced the content of subsequent versions is not clear, but German textbooks soon included versions of the Myth that were remarkably similar to Kolbe's while downplaying the singular importance of the urea synthesis in favor of a more gradual erosion of the boundary between organic and inorganic compounds. In his 1871 textbook, Wislicenus noted that the distinction between organic and inorganic diminished as more organic compounds were made in the laboratory, but unlike in his lectures, he did not mention Wöhler by name, and did not attribute the need for a new definition exclusively to Wöhler's synthesis.[28] Other syntheses, unnamed, but undoubtedly including Kolbe's synthesis of acetic acid, also contributed to the redefinition of organic chemistry. Already in his 1861 *Lehrbuch,* August Kekulé had defined organic chemistry using Kolbe's criteria, and presented an analysis of the urea synthesis similar to Wislicenus. [29] Emil Erlenmeyer (1883) also included a modification of Kolbe's story, and Edvard Hjelt (1887) recounted Kolbe's story, but suggested that Wöhler's synthesis was only the 'first blow to the belief in the necessity of a vital force.' Victor Meyer and Paul Jacobsen (1922) traced to Wöhler's synthesis the origins of organic synthesis from the elements as a standard method in organic chemistry.[30]

After Wöhler's death in 1882, his biographers continued to spread the story of the urea synthesis. Perhaps the most famous of the Wöhler obituaries, for a long time taken as the first appearance of the Myth, was by A.W. Hofmann, who labelled the synthesis as an 'epochal discovery,' that was greeted with 'jubilation' as it unified chemistry.[31] It is unknown whether Hofmann explicitly drew his version of the Myth from Kopp or Kolbe's versions, or if by 1882 it was so common among chemists that its original published version was already forgotten. When at Giessen, Hofmann had been, however, a contemporary of Kopp; he also certainly knew Kolbe and was familiar with his textbook, making it likely that he was among the early generation of chemists familiar with the story. Other biographers would also emphasize the importance of the synthesis. Ira Remsen wrote that the importance of Wöhler's synthesis 'would be difficult to exaggerate,' and that the formation of urea 'in the laboratory out of inorganic compounds appeared to chemists then to be little less than a miracle.'[32] T.E. Thorpe, who based most of his treatment of Wöhler on Hofmann's obituary, stated that 'No single chemical discovery of this century has exercised so great an influence on the development of scientific thought.'[33]

The Wöhler Myth also became standard in general histories of chemistry. For example, Ernst von Meyer (1889), wrote that the urea synthesis was a 'notable observation,'[34] and Karl Graebe (1920), said that Wöhler's synthesis 'opened the beginning of the glorious development of synthetic organic substances,' and that the 1828 *Annalen* article 'immediately excited great admiration.'[35] Schorlemmer (1894) labelled the synthesis as 'important,' but perceptively he called it 'incomplete' and argued that it had not 'shake[n] the belief in a vital force.'[36]

In 1928, the centennial of the urea synthesis, two articles appeared in major scientific journals. W. H. Warren of Clark University wrote up a lecture that he had given at the 1928 meeting of the American Chemical Society for the recently launched *Journal of Chemical Education.* Warren elaborated an expanded version of the Myth based on passages from Wöhler's article, Wöhler's letter to Berzelius and Berzelius' response, Liebig's and Dumas' remarks, and many other essays published well after the event, most on Wöhler's death. He did not mention the original published versions of the Myth by Kopp and Kolbe, both of which had been essentially forgotten by 1928.[37] According to Warren, Wöhler's synthesis 'overthrew what most chemists had hitherto regarded as a well-established dogma, namely, that compounds produced by the plant or animal organism could not be formed by artificial means in the laboratory.'[38] Warren used the metaphor of an 'impassable gulf' between organic and inorganic chemistry that was to be used many times in subsequent American textbooks. Like many other authors, Warren also claimed that the synthesis 'inaugurated that wonderful era of synthetic organic chemistry which is still in progress.'[39]

Somewhat less influential was a centennial article written for *Die Naturwissenschaften* by the chemist-historian Paul Walden on "The Meaning of the Wöhler Urea Synthesis."[40] Walden continued to recognize the fundamental importance of the urea synthesis to the development of organic chemistry, and made several unique observations. The synthesis, Walden wrote, was 'a lovely textbook example [*Schulbeispiel*] for the biology of any discovery, an illustration of the interplay between theory, practice, and chance.'[41] Walden was one of the few commentators to emphasize that the synthesis was the result '*not* of *theory*, not of the scientific *search for this synthesis*, but a mischievous *accident*, ..: a *failed experiment*.[42] He argued that Wöhler had believed that his synthesis was genuinely from the elements, but had declined to pursue its implications for vitalism because he 'was aware that his discovery and its meaning was philosophically foolish and, from a chemical standpoint, heretical.'[43] Walden even suggested that Wöhler might have thought that he would have suffered Galileo's fate for speaking out against the vital force.

A far more influential account of the synthesis was by Bernard Jaffe in his popular history of chemistry, *Crucibles: The Lives and Achievements of the Great Chemists,* first published in 1931 and still in print today. In a chapter entitled "Wöhler: Urea Without a Kidney," Jaffe took the Myth to the limits of credibility, making the refutation of the vital force the central feature of Wöhler's synthesis. Wöhler was a young scientist in the 'sacred temple of his laboratory' making every effort to discredit the 'mysterious vital force.' Jaffe's conception of the vital force is best put in his own words:

> Inside the living body of plants and animals, it was thought, burned a

> steady invisible flame, and through this flame a mysterious vital force built up the sugars, starches, the proteins and hundreds of other very complex compounds. This vague creative force existed in the animal and vegetable kingdoms but not in the mineral world. ... Man could never imitate the power of this vital force. It was one of those mystic causations of which man was to remain in ignorance all the days of his life. Man's mental machinery and his chemical engines were too puny and simple to reproduce this force of nature.[44]

Ignoring all pretense of historical accuracy, Jaffe turned Wöhler into a crusader who made attempt after attempt to synthesize a natural product that would refute vitalism and lift the veil of ignorance, until 'one afternoon the miracle happened.'

> He was standing upon the threshold of a new era in chemistry, witnessing 'the great tragedy of science, the slaying of a beautiful hypothesis by an ugly fact.' ... The pregnant mind of young Wöhler almost reeled at the thought of the virgin fields rich in mighty harvests which now awaited the creatures of the crucible. He kept his head. He carefully analyzed his product to verify its identity. He must assure himself that this historic crystal was the same as that formed under the influence of the so-called vital force.[45]

Jaffe's version seems to be largely drawn from Warren's article, although we can see phrases from past chemists' reactions to Wöhler's synthesis woven into a seamless narrative. Liebig's 'new era' and Hofmann's (from Kolbe) 'epoch making discovery,' Warren's 'impassable gulf,' Remsen's 'miracle,' and others all find their way into Jaffe's narrative. Needless to say, Jaffe represented Wöhler's synthesis as the complete and utter destruction of vitalistic thought in all its forms.

Warren's article was and still is available in a readily accessible journal read widely by chemical educators. Jaffe's popular history has been taken as an authoritative collection of biographies of great chemists, and is still readily available on bookstore shelves. The intriguing suggestions in Walden's essay have largely been ignored, at least in English language versions of the Myth, likely because of its publication in German. The Warren and Jaffe versions have therefore remained more visible, particularly in America, and have had the most influence on recent American textbook versions of the Wöhler Myth.

Unraveling the Importance of the Urea Synthesis

Before we turn to recent textbooks, however, it is useful to examine how historians and historically-minded chemists have interpreted the significance of Wöhler's synthesis, so as to be able to compare the current historiographical understanding of the episode with that presented in recent textbooks. Up to and including Jaffe's account, there seemed to be an implicit agreement (with the exception of Schorlemmer) that the synthesis was indeed a synthesis from the elements, and therefore a refutation of vitalism. In 1944, perhaps in response to Jaffe, the chemist and biographer of Lavoisier, Douglas McKie, queried how the synthesis could have 'acquired an altogether inexplicable and widespread significance, both in text-books and in popular scientific literature.[46] Wöhler did not sound the 'death-knell' for vitalism, McKie argued, because his starting materials were derived from organic materials. He had not therefore performed a true total synthesis of urea directly from the elements. According to McKie, organic chemistry and organic synthesis began with Kolbe's synthesis of acetic acid from coal in 1845. He concluded rather forcefully 'those who believe Wöhler drove vitalism out of organic chemistry will believe anything.' There was, however, an argument against McKie – that Wöhler *had* done a total synthesis, because as historians such as Graebe had noted previously, Scheele had long before prepared cyanate and ammonia from truly inorganic materials.[47]

For twenty years after McKie's article, historians and chemists debated whether Wöhler had completed a 'total' synthesis, as can be seen in subsequent general histories of chemistry. Leicester (1956) mentioned that Chevreul's work on the fatty acids in 1820 had already 'weakened' the belief in the vital force, and Wöhler presented it its first major challenge.[48] Partington's *Short History of Chemistry* (1957) described the vital force as 'mysterious,' receiving a 'severe blow' from Wöhler, but persisting for long afterwards.[49] Szabadvary (1966), on the other hand, subscribed to the strong version of the legend: 'the foundations of the theory of the *Vitalisos* were demolished in 1828.'[50] The most historically sensitive treatments in general histories of chemistry were by Toulmin and Goodfield (1962), and Ihde (1964). Ihde called the synthesis one of Wöhler's 'greatest achievements ... that started the decline of the idea of vitalism,' but noted also that it had 'by no means a dramatic impact,' because neither Wöhler nor his contemporaries claimed that the synthesis ended vitalism.[51] Toulmin and Goodfield mentioned that Wöhler 'acquired a posthumous but irrelevant glory,' that derived primarily from Hofmann's obituary. The synthesis 'at most, ... suggested that the constituent materials of living things might differ from familiar mineral substances in their degree of complexity, rather than in kind.'[52]

Until the 1960s, because of these conflicting interpretations of the 'true' historical meaning of the urea synthesis, it was difficult to tell *why* it would be Wöhler's 'greatest achievement,' or what the 'true' impact of the synthesis was. The issue of total synthesis (meaning directly from the elements) dominated any attempts to prove or disprove the truth of the Wöhler Myth. It was precisely this obsession with Wöhler's act of synthesis that obscured its other, equally valid effects on organic chemistry. Three analyses of the impact of the synthesis that appeared in the 1960s finally moved discussions of the synthesis away from what had become a rather stale, unproductive argument that could not be proven either way. Two of these studies were by professional historians of science, Timothy Lipman and John Brooke, while the third was by a chemist-historian, Theodor Benfey. Each acknowledged the importance of the synthesis, but in different ways.

In *From Vital Force to Structural Formulas* (1964), on the development of organic chemistry up to Kekulé's time, Benfey devoted a chapter to "The Laboratory Preparation of Urea – A Breakthrough in Organic Chemistry." As the book and chapter title suggest, Benfey still subscribed to much of the Wöhler Myth. He wrote of the urea synthesis as building a 'bridge' between the 'mysterious world' of organic and inorganic chemistry, but his training as a chemist as well as historical sensitivity did make him recognize the importance of Wöhler's synthesis as a chemical experiment. In a careful discussion, Benfey explained the theory behind the experiment, and why urea proved to be a puzzling compound for current theories – it was a unitary compound that did not fit the theoretical confines of electrochemical dualism. But more importantly, Benfey explicitly acknowledged the experimental skill that went into the previously neglected, but always mentioned fact that ammonium cyanate and urea had identical elemental compositions. Determining an accurate and replicable composition from combustion analysis, Benfey noted, required high purity in the sample, accurate measurements of the combustion products, and accurate values for the elemental composition of carbon dioxide and water. 'Accurate analyses of organic compounds,' said Benfey, 'were the key to the unraveling of the relations between them.'[53]

In an article on "Wöhler's preparation of Urea and the Fate of Vitalism," Lipman cast serious doubt on the notion that Wöhler's synthesis had any effect on vitalism. As we have seen above, nearly all contemporary accounts that label Wöhler's synthesis as 'remarkable' mention next to nothing about the fate of the vital force. Later chemists and some historians, already familiar with the Myth, overinterpreted those same passages as proof that the significance of the synthesis lay in the destruction of vitalism. Lipman, however, pointed out a previously unrecognized distinction – at least to the chemists and historians after Wöhler – between 'organic' and 'organized' bodies. While the former referred to single substances, the latter referred to complex organisms, i.e. a plant or an animal. And while the former certainly could be formed in the laboratory, the latter most certainly could not.[54] Significantly, Lipman recognized that 'vitalism' was not a simple hypothesis refutable by a single experiment, but a cluster of related beliefs that may or may not be consistent, and that varied from scientist to scientist. Writing in 1968, Brooke provided much the same analysis of vitalism as Lipman, and came to roughly the same conclusions, though he also suggested that the oft-quoted passages from Wöhler's contemporaries that called the synthesis 'singular' or 'remarkable' in the 1830s referred to the transformation of a salt to a non-salt, and the identical elemental composition, and not mainly to the absence of vital functions.[55]

After considering Benfey's, Lipman's and Brooke's arguments, we are left with three principal historically significant aspects of the Wöhler synthesis. First, Wöhler displayed considerable experimental ability in providing an accurate elemental analysis of synthetic and natural urea that demonstrated unequivocally that they had the same elemental composition. Second, his accurate elemental analyses of synthetic urea showed that it had the same composition as ammonium cyanate, and therefore provided a concrete new example of what would later be called isomerism. In fact, as Wöhler mentioned to Berzelius in the 1828 letter, this was his original purpose in the experiment, and Walden was quite right to characterize the synthesis of urea as the outcome of a failed experiment. Third, the synthesis could not destroy the foundations of vitalism, for the concept of 'vitalism' had as many definitions as adherents and served widely different functions, such that it could never be refuted by any one experiment.[56] By the late 1960s, the historical literature had reversed the predominant view that the urea synthesis had given a 'crushing' or at least an 'initial blow' to vitalism, and replaced it with a view that it had an 'insignificant' (or nearly so) impact on vitalism. However, while Lipman and Brooke found other historically significant consequences of Wöhler's preparation, they assumed that, because vitalism did *not* disappear with the urea synthesis, the Myth itself can have no truth value. Neither considered why the Myth appeared at all. In their correspondence, Wöhler and Berzelius were clearly excited from the beginning about the implications of the artificial synthesis of urea, even if the potential impact on vitalism was not mentioned directly. It is precisely the oblique character of their discussion concerning vitalism that later allowed both the 'insignificance' and the 'crushing blow' interpretations of the synthesis.[57]

Two further observations emerge from this brief recounting of the historiography of the Wöhler Myth. First, it has long been known that Wöhler synthesized oxalic acid, isolated from various plants, in 1824. Why, then, was it the 1828 urea synthesis that chemists have considered as the 'death blow' to vitalism? There are two possible answers. According to Benfey, the behavior of oxalic acid made its double decomposition reaction easy to model within the dualistic scheme of inorganic chemistry, and therefore it could be considered as an analog to an inorganic acid. Urea's behavior as a *unitary* (non-salt) compound, synthesized from a salt, proved more difficult to fit into the dualistic scheme. Brooke has suggested that, because chemists had not yet arrived at a consistent definition of 'organic substance,' it seems likely that oxalic acid was not considered organic. These two suggestions complement each other nicely. Because oxalic acid could be modelled easily in the dualistic scheme, it could be considered an inorganic acid.

Another intriguing aspect of the Wöhler Myth is its paradoxical dual role as both founding moment of organic chemistry and as the agent that unified organic and inorganic chemistry under the same principles. That these effects might in fact be contradictory has gone largely unnoticed, but the contradiciton can be resolved partially by noting that to a great extent the 'chasm' dividing organic from inorganic was a creation of Wöhler and those who initially believed his story. Chemists such as Lavoisier and Berzelius had always had in mind, however, a unified chemistry. Berzelius in particular presumed that the difficulties in organic chemistry could be overcome by considering organic compounds as analogous to inorganic compounds.[58] As we have seen, the trouble with urea was the difficulty in applying to it models developed for inorganic compounds. We are still left, however, with two questions whose answers are beyond the scope of this essay. First, if chemists assumed that organic and inorganic compounds should be subsumed under the same set of laws, from where did the mythical idea of a 'chasm' separating the organic from the inorganic come? Second, if Wohler's synthesis was a founding moment for organic chemistry, but organic and inorganic chemistry followed the same principles, by what criteria should we define 'organic' chemistry? Why should we, as chemists clearly did during the 1830s and 1840s, divide chemistry into distinct subdisciplines of organic and inorganic chemistry?

The Urea Synthesis in Recent Textbooks of Organic Chemistry

Having attempted to uncover the various aspects of why Wöhler's synthesis is historically significant, we can move to the portrayal of the Myth in recent American textbooks. The tradition begun in Germany of including the Wöhler Myth at the beginning of textbooks soon appeared in the United States, both in translations of standard German texts and in Ira Remsen's first text published in 1885. In 1928, Warren could write that 'there is hardly a textbook of organic chemistry' that does not mention the Myth.[59] McKie lamented the 'altogether inexplicable and widespread significance' given to Wöhler, 'both in textbooks and in popular scientific works, especially among those whose statements show that they cannot possibly have referred to [Wöhler's] own account of them.'[60] Modern textbooks continue to present variants on the Wöhler Myth. Of the thirty-three twentieth-century American textbooks texts published between 1922 and 1996 that I surveyed, twelve had historical introductions of at least one paragraph. The inclusion of an historical introduction usually entails a recounting of Wöhler's synthesis, but the inclusion of the Wöhler Myth in a textbook does not mean the text has an historical introduction. Most of these historical introductions are one to two pages, and the longest, at sixteen pages, was in Gutsche and Pasto's *Fundamentals of Organic Chemistry* (1975).[61] Thirty of these texts (91%) mention Wöhler and the urea synthesis by name. The versions of the Myth range from the 'no impact' to 'little impact,' to the 'single blow' interpretations. Vital force is mentioned either by name, or implicitly by referring to a 'gulf' between organic and inorganic chemistry, in twenty-one of the texts. Reflecting McKie's conclusion, sixteen authors mention that other syntheses, usually Kolbe's acetic acid synthesis, and less frequently Marcelin Berthelot's multiple syntheses, led to the demise of the vital force. In 1944, Fieser and Fieser included for the first time the modern chemical equation for Wöhler's synthesis in a textbook , and thirteen texts have since included it.

Although Kopp's and Kolbe's original versions have long since been forgotten, the basic story presented in all cases adheres generally to Kolbe's 1854 version. In their modern incarnations, however, presentations of the Myth can be characterized by historical or philosophical naivete that does not seem to be present in the original version, and could be the result of Jaffe's popular but wildly exaggerated account. The descriptions of the impact of the synthesis are unequivocal and dramatic. The assumed boundary between organic and inorganic was a 'restricted view.' It was 'chiefly' Wöhler who dispelled the need for the vital force, or the synthesis 'shattered' or 'demolished' the belief in a vital force or at least gave it a 'heavy blow.' The vital force was 'deeply entrenched,' or an 'obstacle' to the correct understanding of organic chemistry. One 1971 text described the speculations of the early chemists as 'haywire' rather than 'incorrect.'[62] Nearly all texts mention how Wöhler crossed the 'boundary' between organic and inorganic, performing the contradictory acts of uniting two branches of chemistry and founding organic chemistry as a separate discipline. Curiously, one textbook made a unique, rather convoluted claim that the belief in a vital force itself was responsible for establishing organic chemistry as a discipline.[63]

Also readily apparent in most texts is the almost total absence of any influence of the historical studies of the synthesis described in the last section. There are occasional trivial historical inaccuracies. 'Historical' descriptions also include vague words and phrases (e.g. 'some' chemists, 'many more' examples or experiments were needed), heavy use of the passive voice, and vague and unsubstantiated claims such as 'evidence accumulated' against the vital force, or 'more and more carefully executed experiments were providing an increasingly scientific basis for understanding chemistry.'[64] In addition to inaccuracies and imprecise prose, many texts are historically or philosophically simplistic. Even if the author has acknowledged that Wöhler's synthesis did not destroy vitalism, the persistence of a belief in vital force after the synthesis is a mystery, as if it were obvious that such an assumption was unnecessary after the synthesis.

The presentation of Wöhler's story also often illustrates a naive conception of the 'scientific method,' in which it is assumed that any theory can be unequivocally destroyed by a single experiment, and that vitalism therefore should have been rejected solely on the basis of Wöhler's experiment. In part, this simple view of science derives from the basic nature of textbooks that are unlikely to be sophisticated in presenting methodology. But there is also a misunderstanding in these texts of the concept of 'vitalism' – the authors commonly assume that it was a simple, testable hypothesis that could be easily refuted. However, such philosophical simplifications are but a symptom of the generally poor presentation of the *activity* of organic chemistry in general. In recent textbooks there is little or no mention of experiment so that mentioning Wöhler's synthesis at all, even in a simplified form, is an exception to this rule. Textbooks in general tend to be 'epistemologically bankrupt,' with barely a glimmer of the complex relationship between theory and experiment, the meaning of chemical formulas, or even pictures of laboratory apparatus and instruments.[65] Nearly all recent texts surveyed begin with the brief Wöhler story and then launch immediately into the theoretical details of orbital hybridization, Lewis structures and molecular geometry, rather than a discussion, often seen in nineteenth and early twentieth century texts, about the elemental composition of organic molecules and how to determine it.

There are exceptions, however. Cram and Hammond (1964), for example, acknowledge that the disappearance of vital force 'has an interesting and somewhat obscure history,' that the destruction of vitalism by Wöhler's synthesis is more obvious to us now than it was to Wöhler's contemporaries, and (perhaps resulting from McKie's article) that 'historians disagree' on the impact of the synthesis. Most important, Cram and Hammond also acknowledge a more realistic picture of how science works:

> Probably the theory of vitalism, like many other scientific theories, disappeared slowly under the weight of accumulated evidence, rather than as a consequence of any single brilliant and illuminating experiment. Similar slow change is characteristic of most chemical theories including, no doubt, those which are found most useful today be organic chemists.[66]

Such statements are rare in organic textbooks, whose authors seldom mention that theories develop and change over time.

As textbooks have multiplied, small variations in the story have appeared. For example, Fieser and Fieser mention that Wöhler repeated his synthesis many times before he felt confident enough to publish the results. This derives either from Warren or Jaffe; neither Wöhler nor the readily available historical records say anything about how many times he did the synthesis.[67] Loudon (1995) gives a description of Wöhler and the vital force that is an awkward combination of Jaffe and McKie:

> Somehow, the fact that these chemical substances were organic in nature was thought to put them beyond the scope of the experimentalist. The logic of the time seems to have been that life is not understandable; organic compounds spring from life; therefore, organic compounds are not understandable. ... Although vitalism was not so much a textbook theory as an intuitive idea that something might be special and beyond human grasp about the chemistry of living things, Wöhler did not identify his urea synthesis with the demise of the vitalistic idea; rather, his work signaled the start of a period in which the synthesis of so-called organic compounds was no longer regarded as something outside the province of the laboratory.[68]

A larger sample size would no doubt multiply examples and reveal more idiosyncratic versions of the Myth.

THE WÖHLER MYTH AND DISCIPLINARY IDENTITY

Why do authors continue to use the Wöhler Myth in textbooks of organic chemistry? It is often but a one paragraph, or even one sentence, 'sound bite' that has almost no relation to the remainder of the text. What is the pedagogical purpose behind continuing to include such descriptions of Wöhler's synthesis in undergraduate organic chemistry texts? Wöhler's accidental synthesis could serve as a good example for illustrating the fine observational skills required by all skilled chemists, as Wöhler must have had a broad knowledge of the properties of many different compounds, organic and inorganic, for him to suspect that his unknown product was similar to urea. Yet textbook authors do not use his synthesis as a lesson in proper scientific practice. Why, then do authors continue to include it?

One possible reason, at least for more recent texts, has little to do with the discipline of organic chemistry itself, but with the economics of modern textbook production. The repetition of specific examples in different textbooks occurs in nearly all introductory texts across a range of subjects. The majority of biology texts, for example, mention when discussing the evolution of the horse that the *eohippus*, an evolutionary ancestor of the horse, was the size of a fox terrier. Not content with the explanation that all textbook authors are dog breeders, Stephen Jay Gould traced the use of the fox terrier to a 1904 article on "The Evolution of the Horse in America" in *Century Magazine.* The fox terrier analogy was incorporated into biology texts and soon became the animal of choice, preferable to cats and foxes, and the fox terrier continues despite the fact that most readers (and authors) have only a vague impression of exactly how big a fox terrier actually is.[69]

Another example of textbook repetition involves the discussion of the inheritance of intelligence in genetics texts. Diane Paul traced the use of Cyril Burt's study, initially published between 1955 and 1966 on the genetic inheritance of IQ in introductory genetics textbooks. Even though Burt's results were shown to be fraudulent in 1976, genetics texts have continued to cite his results, but not Burt himself as the author. Authors should know explicitly that Burt's study was fraudulent, but they include it implicitly in their own texts by incorporating information from older textbooks that do not explicitly reference Burt.[70]

The only credible reason for the continued use of fox terriers and Burt's data is that textbook authors borrow widely from previous textbooks. There are many reasons for this practice. By their very nature, introductory texts cover the same material. There is enormous pressure for authors to produce texts that emulate the current market leader. Rarely is one author so expert in all subdisciplines as to be able to create an original discussion of a topic that is not within their expertise, nor are chemists, in the case of the urea synthesis, familiar with the history of chemistry. Professors often do not like to change drastically their lecture notes, whether from pressure of time, inertia or conservatism, so a new textbook is likely to be more attractive if it does not deviate too greatly from established tradition. The result is widespread wholesale repetition of texts, right down to historical anecdotes and specific examples. Enough different pictures, graphs, practice problems, and supplementary materials are included to avoid copyright infringement, but introductory textbooks, including those in organic chemistry, are often nearly identical to one another and rarely reflect the author's own preferences and idiosyncrasies. The vast majority of American organic texts follow the same overall format, originated by Ira Remsen in 1883, in which each functional group is paraded before the student.[71]

Yet these economic considerations do not explain fully the initial appearance of the Wöhler Myth or its propagation during the nineteenth century. Even with historical inaccuracies and philosophical naiveté, the urea synthesis does serve useful purposes, one of which is as a disciplinary founding myth. Some textbooks explicitly say that organic chemistry began with Wöhler's synthesis, while others that mention the synthesis do not. Yet in placing the synthesis prominently at the beginning of the text, where authors define the subject for the student, they are implicitly acknowledging at least a partial role for the event as the beginning of organic chemistry in its modern form. The prevalence of the Myth itself indicates its appeal. By 'appeal,' I mean here that chemists would like it to be true story, even if it is not. The appeal of the Wöhler story, like many myths about origins, lies perhaps in its ability to pinpoint the beginning of organic chemistry to a single datable event. Accurately or not, Wöhler's synthesis has arbitrarily (although not without reason) been taken as the initial discovery that founded organic chemistry.

By the mid to late nineteenth century, recounting the Wöhler story became a pedagogical and symbolic ritual for instilling the characteristics of organic chemistry in its new practitioners.[72] Its appearance coincides nicely with the late nineteenth century 'mass production' of traditions described by the social historian Eric Hobsbawm.[73] According to Hobsbawm, the late nineteenth century saw the creation of 'invented traditions' such as the formation of national holidays, monuments, and rituals in the furtherance of nationalism, and growing numbers of rituals and events in sports such as tennis, soccer, cycling, and, in the United States, baseball, which itself possesses a specific founding mythology. We can see a similar process of tradition formation in organic chemistry during the 1870s and 1880s, following the deaths of Liebig and Wöhler, both of whom received extensive obituaries and memorials, enshrining them as the founding heroes of organic chemistry. Hofmann, in particular, was interested in preserving the memory of Wöhler and the urea synthesis. In addititon to the extensive obituary mentioned above that included one of the most famous versions of the Myth, he edited the two-volume edition of the Liebig-Wöhler correspondence that appeared in 1888.[74] The Wöhler Myth therefore provides a hero who accomplished a specific datable task that to the later community has great significance. The historical truth value of the synthesis – whether it destroyed vitalism, or inaugurated organic synthesis – has ceased to matter.

Like the other nineteenth century myths of tradition, the Wöhler Myth may also have been propagated as part of German nationalism, by German chemists who wished to place the origins of modern organic chemistry squarely in Germany. Who better than Wöhler, and what better act than the unexpected synthesis of a natural product, to establish the origin of the powerful German chemical community, within which synthesis played a central role during the nineteenth century? In his centennial article, Walden made precisely this point: was not Wöhler's synthesis the 'historical founder of the *leading position* of the German chemical genius?'[75] Kopp and Kolbe may not themselves have started the Wöhler Myth in order to further German nationalism, but nationalism can go a long way towards explaining its durability. It helps to explain, for example, why Chevreul's study of animal and vegetable fats, of comparable significance for vitalism, or Marcelin Berthelot's syntheses, often more directly from the elements than Wöhler's, have been considered as only 'paving the way' for, or substantiating, respectively, the more significant urea synthesis.

As Russell has suggested in the case of Frankland, the Myth may also have served as a way of emphasizing to students that the problems of chemistry could be solved in a uniquely chemical way. According to Frankland and his contemporaries, the fundamental problems that were unique to chemistry– among others, understanding isomerism, constitution, and reactivity – required a materialist or reductionist theory that did not rely on biological or physiological concepts such as vital force.[76] In other words, organic chemistry should be regarded as an autonomous discipline with its own aims and explanatory methods. The general conviction of the autonomy of chemistry can also explain the initial hold the urea synthesis had on the minds of those that initiated and perpetuated the Myth: Wöhler, Berzelius, Liebig, Kolbe, and Kopp. All were devoted materialists and strongly opposed to the mysticism of *Naturphilosophie.*

Another reason for later generations of nineteenth century chemists to focus on the urea synthesis as a founding moment is its role as a *synthesis.* By the mid- to late nineteenth century, the synthesis of organic compounds had become central to the science of organic chemistry. The constitution and structure of increasingly complex organic molecules were elucidated by synthesis from simpler substances, that ultimately could be made from the elements themselves. As Brooke has noted, nineteenth century chemists may even have overemphasized the activity of synthesis to the point of considering it the definitive activity of organic chemists.[77] The total synthesis of natural products as a method of structure determination remained a major area of research for organic chemistry well into the twentieth century. In his textbook, Weininger (1973) remarked that this long obsession with total synthesis may be due to a 'psychological legacy of the concern with vitalism.'[78] Because so many present day histories of chemistry have been derived from nineteenth-century (primarily German) sources in which synthesis is given a prominent role, synthesis has also played a major, but certainly not unjustified, role in the historiography of chemistry to the present day. Given this historiographic bias and the past preoccupation of organic chemists with synthesis, it should not be surprising that Wöhler's synthesis has retained its position as an important founding moment for organic chemistry.

Without the single defining event, the exact origin of discoveries, disciplines, or sports like baseball get lost in the details of history. Organic chemistry did not begin in one single instant. It evolved from and separated slowly from inorganic chemistry as chemists moved gradually towards an exclusive study of the reactivity and composition of compounds derived from plants and animals. No one event can unequivocally be said to be the first for the discipline of organic chemistry.

As it first did for Kolbe, the Wöhler Myth also still provides a background for the modern definition of organic chemistry. To non-chemists, the word 'organic' has continued to be largely associated with 'life,' and with 'natural' or 'pure,' even though those meanings have long since been discarded by chemists. An historical introduction that makes use of the original meaning of organic chemistry as the 'study of those compounds that originate in living organisms' and mentions the division of organic and inorganic, gives students reassurance that the non-chemist's conception of 'organic' is partially true, but in the context of their formal study of organic chemistry, they must redefine the subject as the chemistry of compounds containing carbon. Laying out this distinction has become somewhat ironic in recent years, because modern organic chemistry has moved more and more towards biochemistry and physiology, blurring any true distinction between 'organic' (meaning carbon based compounds) and 'organized.'

There is one final reason for the continued existence of the Wöhler Myth. The transformation of ammonium cyanate into urea does express a basic truth of organic chemistry recounted by Warren in the opening lines of his centennial paper:

> The story of Friedrich Wöhler and his synthesis of urea is familiar to every student of organic chemistry, for in their opening lectures, teachers of this subject usually call particular attention to this discovery as the first instance of the artificial formation of a compound produced by the animal organism. Then, if the student in turn becomes a teacher, he yearly repeats the tale to each new class as it begins the study of perplexing formulas, mystifying transformations, and reactions without end.[79]

Organic chemistry is a perennially difficult course in any curriculum. Students encounter for the first time real chemical reactions (at least on paper) and a formalistic way of thinking that is quite foreign. Completing general chemistry requires students to memorize mathematical formulas and solve problems by knowing which number to place in the correct formulas to get an exact answer. Questions in organic chemistry generally have no one 'correct' answer, and students often find themselves learning a foreign language, in which they must learn to 'spell' and construct 'sentences' by making informed guesses using a set of formal rules. The number of reactions and transformations the students must master is indeed daunting, and they can be mystifying. As the early organic chemists knew well, organic compounds *are* different from inorganic compounds. They *do* have wildly varying combining ratios, they *do* decompose more easily, they undergo a bewildering variety of transformations, and many of these compounds *are* produced by living organisms in processes that are today not as mysterious, but nevertheless quite complex. Organic compounds offered significant challenges to chemists, and for a time were mysterious. Perhaps nature itself should get its own share of credit for perpetuating the Wöhler Myth.

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APPENDIX: TEXTBOOKS CONSULTED BUT NOT CITED IN NOTES

Ronald F. Brown, *Organic Chemistry* (Belmont, CA: Wadsworth, 1975).
Paula Bruice, *Organic Chemistry* (Englewood Cliffs, NJ: Prentice-Hall, 1995).
Donald Burton, Joseph Routh, *Essentials of Organic and Biochemistry* (Philadelphia, Saunders, 1974).
James Cason, *Essential Principles of Organic Chemistry* (Englewood Cliffs, N. J., Prentice-Hall, 1956).
James Bryant Conant, *Organic Chemistry; a Brief Introductory Course* (New York: The Macmillian Company, 1936).
Seyhan Ege, *Organic Chemistry,* 2d ed. (Lexington, MA: DC Heath, 1989).
Ralph Fessenden, Joan Fessenden, *Fundamentals of Organic Chemistry* (New York, Harper and Row, 1990).
I. L. Finar, *Organic Chemistry,* (London: Longmans, 1956).
Reynold C. Fuson, *Organic Chemistry* (New York: J. Wiley & Sons, 1942).
Paul Karrer, *Organic Chemistry,* translated by A. J. Mee (Amsterdam: Elsevier, 1938).
Harold Lowther, *Organic chemistry; an Introductory Course* (Oxford: Pergamon Press; New York, Macmillan, 1964).
John McMurry, *Organic Chemistry,* 3d ed. (Pacific Grove, CA: Brooks-Cole, 1992).
Robert Thornton Morrison, Robert Neilson Boyd, *Organic chemistry* , 2d ed. (Boston: Allyn and Bacon, 1966).
Douglas C. Neckers, Michael P. Doyle, *Organic Chemistry* (New York: Wiley, 1977).
Carl. R. Noller, *Textbook of Organic Chemistry,* 3d ed. (Philadephia: Saunders, 1966).
R. O. C. Norman, D. J. Waddington, Modern Organic Chemistry (London: Bell and Hyman, 1983).
James F. Norris, *The Principles of Organic Chemistry,* 2nd ed. (New York: McGraw-Hill, 1922).
William Henry Perkin, F. Stanley Kipping, *Organic Chemistry,* New and revised edition (London: Edinburgh, W. & R. Chambers, 1902–1903).
Stanley Pine, *Organic Chemistry,* 5th ed. (New York: McGraw-Hill, 1987).
Ira Remsen, *An Introduction to the Study of the Compounds of Carbon,* (Boston: D.C. Heath & Company, 1887).
Victor von Richter, *Organic Chemistry, or: Chemistry of the Carbon Compounds* (London: K. Paul, Trench, Trubner, 1919–1929).
James D. Roberts, Ross Stewart, Marjorie C. Caserio, *Organic Chemistry: Methane to Macromolecules* (Menlo Park: Benjamin, 1971).
David A. Shirley, *Organic chemistry* (New York: Holt, Rinehart and Winston, 1964).
T. W. Graham Solomons, *Organic Chemistry,* 5th ed. (New York: Wiley, 1992).
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K. P. C. Vollhardt, Niel Schore, *Organic Chemistry,* 2d ed. (New York: Freeman, 1994).

Leroy G. Wade, *Organic Chemistry*, 3d ed. (Englewood Cliffs, NJ: Prentice Hall, 1995).
Edgar Wertheim, *Introductory Organic Chemistry, with Certain Chapters of Biochemistry* (Philadelphia: Blakiston, 1942).
G. W. Wheland, *Advanced Organic Chemistry* 3d edition (New York, Wiley, 1960).

Notes and References

1. '... denn ich kann, so zu sagen, mein chemisches Wasser nicht halten und muß Ihnen sagen ich Harnstoff machen kann, ohne dazu Nieren oder überhaupt ein Thier, sey es Mensch oder Hund, nöthig zu haben,' Friedrich Wöhler to Jakob Berzelius, February 22, 1828, in Otto Wallach, editor, *Briefwechsel zwischen J. Berzelius und F. Wöhler*, 2 vols. (Leipzig: Engelmann, 1901) vol. 1, p. 206. Unless noted, this and all subsequent translations are my own.
2. Historians of science have recently developed a number of approaches for understanding the formation of such identities on intellectual, institutional and geographic levels. For example, in her study of the disciplinary identity of chemistry, Nye has carefully outlined six distinct 'elements of identity' for disciplines, the first of which includes the 'historical mythology of heroic origins and heroic episodes.' Mary Jo Nye, *From Chemical Philosophy to Theoretical Chemistry: Dynamics of Matter and Dynamics of Disciplines, 1800–1950* (Berkeley, CA: University of California Press, 1993), p. 19.
3. Cohen and Cohen have published recently a similar study of the portrayal of Wöhler's Urea Synthesis in textbooks that in large part reinforces the claims made here. They do not, however, offer any reasons for its initial appearance, nor for the endurance of the Myth, and in fact, reach the same conclusion given in many, but not all, textbooks that the synthesis only created the 'first blow' against vitalism. Paul S. Cohen and Stephen M. Cohen, "Wöhler's Synthesis of Urea: How do the Textbooks Report It?," *J. Chem. Ed.*, **73** (1996), 883–6.
4. Not included in this sample are many shorter, less rigorous texts in organic chemistry for one-semester courses such as allied health science (e.g. nursing) students. And, as I am interested here in the role of the Wöhler Myth in organic chemistry, I have ignored accounts of physiology or biochemistry that may mention Wöhler's synthesis.
5. In addition to the textbooks that will be cited below, I consulted the textbooks listed in the Appendix.
6. Although the inclusion of examples of the Myth from additional textbooks would add little to the basic conclusions given here, much opportunity remains for further scholarly study of the dissemination of the Wöhler Myth in other countries, especially in France, where the influence of the German tradition in structural chemistry was not as great. A much larger sample of textbooks would also allow the further exploration of the genealogy surrounding the variants of the Wöhler Myth appearing in textbooks.
7. This account is drawn from Otto T. Benfey, *From Vital Force to Structural Formulas* (Washington, DC: American Chemical Society, 1975, reprint of 1964 original), pp. 14–16, and the documents cited below.
8. Friedrich Wöhler, "Über künstliche Bildung des Harnstoffs," *Annalen der Chemie und Pharmacie*, **12** (1828), 253–6. Ammonium compounds release ammonia when treated with base, cyanates produce cyanic acid when treated with acid. Wöhler's compound showed neither of these properties.
9. Friedrich Wöhler to Jakob Berzelius, February 22, 1828, *op. cit.* (1).
10. Wöhler, *op. cit.* (8), p. 253.
11. *Ibid*, p. 256
12. Wöhler to Berzelius, February 22, 1828, in Wallach, *op. cit.* (1), p. 208.
13. Berzelius to Wöhler, March 7, 1828, *ibid.*, p. 208.
14. Jakob Berzelius, *Lehrbuch der Chemie*, 3d ed., tr. Friedrich Wöhler (Dresden, 1837), vol 6, pp. 3–28.
15. Although Berzelius used the term *Lebenskraft*, his exact stance on vitalism can be interpreted differently. Alan Rocke, for example, has argued that in his physiology, Berzelius was not a vitalist at all, in the way the term is traditionally defined. Alan J. Rocke, "Berzelius' Animal Chemistry: From Physiology to Organic Chemistry (1805–1814)," in Evan Melhado and Tore Frängsmyer, eds., *Enlightenment Science in the Romantic Era: The Chemistry of Berzelius and Its Cultural Setting* (Cambridge: Cambridge University Press, 1992), pp. 107–31.
16. Timothy O. Lipman, "Wöhler's Preparation of Urea and the Fate of Vitalism," *J. Chem. Ed.*, **41** (1964), 452–8, on p. 455.

17. J.-B. Dumas, "Note sur las composition de l'urée," *Ann. Chim.*, **44** (1830), p. 273, translated by and quoted in J. H. Brooke, "Wöhler's Urea, and its Vital Force: A Verdict from the Chemists," *Ambix*, **15** (1968), 84–114, on p. 111.
18. Justus Liebig, *Report of the Seventh Meeting of theBritish Association for the Advancement of Science: Notes and Abstracts*, **7** (1837), p. 38, translated by and quoted in Lipman, *op.cit.* (16), p. 455.
19. In the forty-three known letters exchanged between Wöhler and Liebig between 1829 and 1833 that mention urea, there are no references to the original 1828 synthesis, nor to its subsequent impact on vitalism. I thank Christoph Meinel (University of Regensburg) for making transcriptions of these letters available to me.
20. Alan J. Rocke, *The Quiet Revolution: Hermann Kolbe and the Science of Organic Chemistry* (Berkeley: University of California Press, 1993), pp. 239–41.
21. Hermann Kopp, *Geschichte der Chemie*, 4 vols. (Braunschweig: Vieweg, 1843–1847), vol. 1, on p. 442, vol. 4, on p. 244.
22. G. Staedler, "Harnstoff" in Hermann Kolbe ed., *Handwörterbuch der reinen und angewandten Chemie* (Braunschweig: Vieweg, 1848), vol. 3, pp. 805–16. Staedler would later be Professor of Chemistry at the Zürich Polytechnical Insitute from 1855 to 1870, and Professor of Chemistry at the University of Zürich.
23. Hermann Kolbe, "Organische Chemie" in Hermann Kolbe ed., *Handwörterbuch der reinen und angewandten Chemie* (Braunschweig: Vieweg, 1851), vol. 5, pp. 735–8.
24. Hermann Kolbe, *Ausführliches Lehrbuch der organischen Chemie*, 2 vols. (Braunschweig: Vieweg, 1854), vol. 1, pp. 3–4.
25. Curiously, although Kopp and Kolbe were well acquainted, and of roughly the same age, Kolbe, at least later in his life when he was becoming increasingly isolated from the chemical community, did not admire greatly Kopp's approach to history. Alan J. Rocke, personal communication, and "Between Two Stools: Kopp, Kolbe, and the History of Chemistry, *Bulletin for the History of Chemistry* **7** (1990), 19–24.
26. Quoted in Colin R. Russell, *Edward Frankland: Chemistry, Controversy and Conspiracy in Victorian England* (Cambridge: Cambridge University Press, 1996), p. 156, Frankland's emphasis.
27. Lecture notes on organic chemistry taken by Robert Gnehm, Sommersemester 1871, Hs 633:5, Wissenschaftshistorische Sammlung, ETH-Bibliothek, Zürich.
28. Johannes Wislicenus, *Adolph Strecker's kurzes Lehrbuch der organischen Chemie*, 6th ed. (Braunschweig: Vieweg, 1874), pp. 1–2.
29. August Kekulé, *Lehrbuch der organischen Chemie*, 4 vols. (Erlangen: Enke, 1861), vol. 1, p. 9.
30. Emil Erlenmeyer, *Lehrbuch der organischen Chemie*, 2 vols. (Leipzig: Winter, 1883); Edvard Hjelt, *Grundzüge der allgemeinen organischen Chemie* (Berlin: Oppenheim, 1887); Victor Meyer and Paul Jacobsen, *Lehrbuch der organischen Chemie* 4 vols. (Berlin, Springer, 1922), vol. 1, part 1, p. 1. The first volume of Meyer and Jacobsen's text was published in 1893.
31. A.W. Hofmann, "Zur Erinnerungen an Friedrich Wöhler," *Berichte der deutschen chemischen Gesellschaft*, **15** (1882), 3127–275.
32. Ira Remsen, "Friedrich Wöhler," *American Chemical Journal*, **4** (1887), 289–92, on p. 291.
33. T.E. Thorpe, *Essays in Historical Chemistry* (London: Macmillan, 1902), p. 302.
34. Ernst von Meyer, *Geschichte der Chemie* (Leipzig: von Veit, 1889), p. 289.
35. Karl Graebe, *Geschichte der organische Chemie* (Berlin: Springer, 1920), p. 54.
36. Carl Schorlemmer, *The Rise and Development of Organic Chemistry* (London: Macmillan, 1894), pp. 21–2.
37. The complete disappearance of Kopp and Kolbe as the first authors of the Myth is intriguing, and an explanation is elusive. It is possible that by the late nineteenth century the Myth had become so commonplace in the thought of organic chemists that citing its source was unnecessary, as they were only restating a common belief amongst those in the profession. The same may also be said of Kopp and Kolbe themselves. Only recently has Rocke [*op. cit.* (20)] noted that Kopp's version was the earliest, and Kolbe's was the earliest version I have found in a textbook.
38. W. H. Warren, "Contemporary Reception of Wöhler's Discovery of the Synthesis of Urea," *J. of Chem. Ed.*, **5** (1928), 1539–52, on p. 1539.
39. *Ibid.*, p. 1540. In another centennial article, Warren recounted Wöhler's 1824 synthesis of oxalic acid, and observed that Wöhler had been the first to synthesize compounds from both plant and animal sources. W. H. Warren, "Die vollständige Geschichte von Friedrich Wöhler's erster organischer Synthese," *Berichte der deutschen chemischen Gesellschaft*, **61A** (1928), 3–7.
40. Paul Walden, "Die Bedeutung der Wöhler'schen Harnstoff Synthese," *Naturwissenschaften*, **16** (1928), 835–49, p. 836.

41. *Ibid.*, p. 838.
42. Ibid., p. 836, emphasis in the original.
43. *Ibid.*, p. 837.
44. Bernard Jaffe, *Crucibles: the Lives and Achievements of the Great Chemists* (New York, NY: Tudor, 1934), p. 175.
45. *Ibid.*, p. 177.
46. Douglas McKie, "Wöhler's Synthetic Urea and the Rejection of Vitalism: A Chemical Legend," *Nature*, **153** (1944), 608–9, on p. 608.
47. Graebe, *op. cit.* (35), p. 54, Ernest Campaigne, "Wöhler and the Overthrow of Vitalism," *J. Chem. Ed.*, **32** (1955), 403.
48. Henry M. Leicester, *The Historical Background of Chemistry* (New York, NY: Wiley, 1956), p. 173.
49. J. R. Partington, *A Short History of Chemistry* (London: MacMillan, 1957), p. 221.
50. F. Szabavary, *History of Analytical Chemistry*, tr. Gyula Svela (London: Pergamon, 1966), p. 284.
51. Aaron J. Ihde, *The Development of Modern Chemistry* (New York, NY: Harper and Row, 1964), p. 164.
52. Stephen Toulmin and June Goodfield, *The Architecture of Matter* (New York, NY: Harper, 1962), p. 326.
53. Benfey, *op. cit.* (7), p. 21.
54. Lipman, *op. cit.* (16).
55. Brooke, *op. cit.* (17).
56. The variations and subtleties in the concept of a vital force are given in Kenneth L. Caneva, *Robert Mayer and the Conservation of Energy* (Princeton, NJ: Princeton University Press, 1993); and Timothy Lenoir, *The Strategy of Life: Teleology and Mechanics in Nineteenth Century German Biology* (Chicago, IL: University of Chicago Press, 1982, reprinted 1989).
57. There is another, albeit hypothetical, scenario under which the synthesis was insignificant, but for different reasons. Given the strong materialist and mechanist leanings of Wöhler, Berzelius, Liebig, and their contemporaries, it may be possible that by 1828, the issue of vitalism in organic chemistry was *already* dead, and for organic chemists, the synthesis was therefore a nail in vitalism's coffin, rather than an initial blow. This would help to explain why there were so few public statements about the impact of the synthesis on vitalism, and why there were no explicit pronouncements about the imminent demise of the vital force. If chemists had already abandoned vitalism, there would be no need to argue the point further. Because of the difficulties in proving that Wöhler had made urea from the elements, it would also be pointless for a vitalist to argue that vitalistic theories were still relevant. Under this interpretation, the synthesis would indeed be insignificant to vitalism, but only because vitalism in chemistry was already dead by 1828, and the artificial synthesis of an organic compound would then be considered an inevitable event. I am grateful to Alan Rocke for bringing this possible interpretation to my attention.
58. Recently Ursula Klein has elaborated extensively on the overwhelming desire of chemists to force organic compounds into the dualistic schemes that had been so successful for inorganic compounds. It was in the 1830s that the original version of organic chemistry as plant and animal chemistry disappeared, to be replaced by modern organic chemistry as the chemistry of carbon compounds. This transformation occurred during the attempt to construct constitutional models of compounds using Berzelian formulas under a dualistic framework. It was the phenomena of substitution that eventually undermined the entire enterprise. Ursula Klein, *Experimente, Modelle, Paper-Tools: Kulturen der organischen Chemie im 19. Jahrhundert*, (Habilitation, University of Konstanz, 1999).
59. Warren, *op. cit.* (38), p. 4.
60. McKie *op. cit.* (46), p. 608.
61. C. David Gutsche, and Daniel J. Pasto, *Fundamentals of Organic Chemistry* (Englewood Cliffs, NJ: Prentice Hall, 1975).
62. James D. Roberts, Marjorie C. Caserio, *Basic Principles of Organic Chemistry* (New York, NY: Benjamin, 1964).
63. George B. Butler, K. Darrel Berlin, *Fundamentals of Organic Chemistry* (New York, NY: Ronald Press, 1972), p. 3.
64. Stephen J. Weininger, *Contemporary Organic Chemistry* (New York, NY: Holt, Reinhart, and Winston, 1972), and Stanley Pine, *Organic Chemistry*, 5th ed. (New York, NY: McGraw-Hill, 1987).

65. I have borrowed the phrase 'epistemologically bankrupt' from Lawrence Principe (Johns Hopkins University) by personal communication.
66. Donald J. Cram, George S. Hammond, *Organic Chemistry*, 2d ed. (New York: McGraw-Hill, 1964), p. 3.
67. Louis Frederick Fieser, Mary Fieser, *Organic Chemistry* (Boston, MA: D. C. Heath and Company, 1944). The 'many trials' variant has also appeared recently in Marten J. ten Hoor, "The Formation of Urea," *J. Chem. Ed.*, **73** (1996), 42–5. Hoor also mentions (echoing Jaffe) that Berzelius did not understand Wöhler's synthesis as an 'attack on vitalism.'
68. G. Marc Loudon, *Organic Chemistry*, 3d ed. (Menlo Park, CA: Benjamin Cummings, 1995), on pp. 1–2.
69. Stephen J. Gould, "The Case of the Creeping Fox Terrier Clone," in *Bully for Brontosaurus* (New York: W.W. Norton, 1991), pp. 155–67.
70. Diane Paul, "The Nine Lives of Discredited Data," *The Sciences* (May 1987), pp. 26–30.
71. Of course, modern texts do include reaction mechanisms, electronic conceptions of bonding and spectroscopy that were not in Remsen's text. The fundamental importance of reaction mechanisms and electronic theory to twentieth century organic chemistry, however, has not compelled chemists to restructure textbooks around them rather than around the traditional framework of functional groups. Rather, concepts of mechanism and bonding are laid over the traditional sequence of functional groups, sometimes resulting in a disjointed presentation. Only spectroscopy tends to warrant one or two dedicated chapters.
72. The subdiscipline of physical chemistry also possesses a founding myth, created in the 1880s by Wilhelm Ostwald, Svante Arrhenius, and J. H. van 't Hoff. The Myth consists of two parts: that Arrhenius, Van 't Hoff, and Ostwald created physical chemistry *de novo*, and that they faced tremendous hostility towards the theory of ionic dissociation. Elisabeth Crawford, *Arrhenius: From the Ionic Theory to the Greenhouse Effect*, (Canton, MA: Science History Publications, 1995), chapter 7; Robert Scott Root-Bernstein, *The Ionists: Founding Physical Chemistry, 1872–1890* (PhD: Princeton University, 1980); and Diana Barkan, *Walther Nernst and the Transition to Modern Physical Science* (Cambridge: Cambridge University Press, 1999).
73. Eric Hobsbawm and Terence Ranger, eds. *The Invention of Tradition* (Cambridge: Cambridge University Press, 1983).
74. The form of Hofmann's edition of the Liebig-Wöhler correspondence also supports the claim that, during the 1880s, Wöhler was elevated to the status of a founding hero of organic chemistry. The edition was not meant to be an accurate reproduction of the correspondence, but a memorial to the two great men and an inspiration to their chemical descendants. The letters were heavily edited, chosen largely by Wöhler himself at the end of his life from the voluminous correspondence with Liebig. The two volumes contain only approximately one-third of the approximately 1670 total letters exchanged between 1829 and 1873, and the letters included were often shortened, abstracted, or even combined into single non-existent letters. Christoph Meinel, "Communication and Knowledge Production: The Liebig-Wöhler Correspondence," paper given at the meeting of the Society for the History of Alchemy and Chemistry, London, 25 May 2000. August Wilhelm von Hofmann, *Aus Justus Liebig's und Friedrich Wöhler's Briefwechsel in den Jahren 1829–1873*,2 vols. (Braunschweig: Vieweg, 1888).
75. Walden, *op. cit.* (40), p. 836, emphasis in the original.
76. Russell, *op. cit.* (26), p. 157.
77. J.H. Brooke, "Organic Synthesis and the Unification of Chemistry: A Reappraisal," *Brit. J. Hist Sci.*, **5** (1971), 363–392. See also C. A. Russell, "The Changing Role of Synthesis in Organic Chemistry," *Ambix*, **34** (1987), 168–80.
78. Weininger, *op. cit.* (64).
79. Warren, *op. cit.* (38) p. 1539.