

WÖHLER'S UREA, AND ITS VITAL FORCE ?—A VERDICT  
FROM THE CHEMISTS

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WÖHLER'S synthesis of urea—an organic compound—from inorganic material, in 1828, has often been exhibited as the thin end of the reductionists' wedge. As one nineteenth century historian proclaimed: Wöhler's experiment was "an epoch-making discovery in the real sense of that word", and "one can conceive the feeling of joy with which the gospel of a new unified chemistry was hailed."<sup>1</sup> Vital forces were supposedly swept away as the chasm between the organic and inorganic worlds was bridged. With the ever-mounting disrespect for whiggish historiography, Wöhler's achievement has more recently suffered the fate of many "crucial experiments", and the most influential exposé has been effected by McKie, who has gone so far as to say that Wöhler did not even perform a complete synthesis.<sup>2</sup> It may be true that historians familiar with the many commentaries on Wöhler's experiment would now be suspicious of any claim reporting a devastating impact, and yet, hitherto, there has appeared no thoroughly satisfactory explanation as to why the urea synthesis should have been relevant or irrelevant to the expulsion of vital forces from physiology. That it is still possible to allude to an element of "paradox"<sup>3</sup> in the unobtrusiveness of artificial urea, invites a more thorough investigation of the chemical and physiological background to the 1828 discovery.

What follows is an essentially exploratory essay: by untying a cluster of knots which have usually concealed the complexity of the issues involved, I hope to demonstrate that with respect to the significance of Wöhler's preparation, the last word has not yet been spoken. In order to inject some coherence into the account, the three major sections of the paper will serve to substantiate an *insignificance* claim for the urea synthesis vis-à-vis current vitalist beliefs. It will be argued in the first section that a notable failure to explore five ambiguities within the literature of the period has vitiated many reconstructions of

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<sup>1</sup> A. W. Hofmann; *Berichte d. deutsch. chem. Gesell.*, **15** (1882), pp. 3152-53. Reproduced in Thorpe, *Essays in Historical Chemistry* (2nd ed., London, 1902), pp. 302-3.

<sup>2</sup> D. McKie, "Wöhler's Synthetic Urea and the Rejection of Vitalism", *Nature*, **153** (1944), p. 608f.

<sup>3</sup> T. O. Lipman, "Wöhler's Preparation of Urea and the Fate of Vitalism", *J. Chem. Educ.*, **41** (1964), p. 454: "There developed the paradox that the artificial formation of an organic compound did not affect the theory that it was impossible to construct an organic compound in the laboratory".

Wöhler's achievement, and can change the most hearty reductionist into a zealous vitalist. Each of these ambiguities will be resolved in such a way as to corroborate the insignificance claim. In the second section it will be maintained that a persistent conflation of two distinct questions, namely "Is there something peculiar about an *organism*?" and "Is there something peculiar about an *organic compound*?", is largely responsible for perpetuating the popular appraisal of Wöhler's achievement. Finally, it will be proposed that contemporary references to the "singularity" of Wöhler's result do not testify to its portentous implications for vitalist beliefs; there were other, excellent, reasons why it was considered remarkable.

At the end of his life, as he surveyed the past development of organic chemistry and contemplated its future, Berzelius was still campaigning for a fuller recognition of a fundamental guiding principle: "We repeat again", he insisted, "that the application of principles relating to the way the elements are combined in the inorganic domain must lead us to an appreciation of organic compounds; it is by setting out from there that we may hope to arrive at exact and harmonious ideas about the composition of organic bodies."<sup>4</sup> The whole pronouncement was italicized, and this emphasis of the thesis that organic chemical theory would only progress if analogies were culled from the inorganic concepts of the day, has consequences of paramount importance for a study of the chemical backcloth to the "drama" of 1828.<sup>5</sup> For as organic chemistry had

<sup>4</sup> J. J. Berzelius, *Traité de Chimie*, 5th ed. (2nd French ed.), transl. by Hoefer and Esslinger, Paris, 1845-50, vol. V, p. 28.

<sup>5</sup> It is rarely recognized that this regulative principle *itself* was implicitly averse to a vitalism erected on the differences between inorganic and organic compounds. That it was so is clear from an account such as that of Bussy in his "Compte rendu des travaux de la Société de Pharmacie", for he proceeds to ascribe the disappearance of vital forces during the 1830s, *not* to the artificial production of organic compounds, but to the evidence which has permitted the composition of organic compounds to be assimilated to the dualistic composition of the inorganic realm. (*J. de Pharmacie*, 22 (1836), pp. 680-4.)

Consequently any suggestion that Berzelius, the author of this regulative principle, was a "vitalist" must be handled with care. It is true that the Swedish chemist began vol. V of the last French edition of his textbook with the admission, "Dans la nature vivante, les éléments paraissent obéir à des lois tout autres que dans la nature inorganique . . .", but this must be construed as a *description* of the current state of organic theory. As Berzelius himself continued to say, the key concepts for organic comprehensibility had not yet been found. To quote *this passage*, as does McKie (*Nature*, 153, p. 609), as an example of a residual vitalism—with the implication that Wöhler's synthesis was therefore innocuous—is unwise, and especially so since Berzelius did clarify his position. "Nous sommes certainement bien loin de comprendre comment les phénomènes de la vie s'accomplissent", he was ready to acknowledge, "*mais le peu que nous en savons, montre clairement que si, dans les phénomènes de la chimie organique, on entend par force vitale autre chose qu'un concours particulier de circonstances différentes mettant en jeu les forces naturelles de la nature organique . . . on commet une erreur.*" [Italics mine]

advanced it had been peculiarly conscious of its relationship with its inorganic senior, and statements of analogies established between inorganic and organic compounds are to be found in profusion in the literature of the period. And if such statements (and questions) of analogy commanded a widespread interest, so too did statements of analogy breakdown.<sup>6</sup> Thus a significant source of confusion in discussions of Wöhler's work has been the failure to recognize that such statements of analogy breakdown were of immense interest in their own right—quite independently of any vitalistic connotation.<sup>7</sup> The following proposition must therefore be emphasized: because inorganic/organic analogies of all kinds excited universal curiosity, statements recording the collapse of analogies between inorganic and organic compounds—statements such as, “unlike inorganic compounds, organic substances cannot be artificially prepared” —might have been refuse, rather than refuge, for the more scrupulous vitalist.<sup>8</sup> They might have been statements of intrinsic interest.

Cf. Bent Søren Jørgensen, “More on Berzelius and the Vital Force” in *J. Chem. Educ.*, **42** (1965), p. 394f.

I am grateful to Dr. Colin Russell for having drawn my attention to what is the best available discussion of “Berzelius und die Lebenskraft”, namely the exposition given by Jørgensen in *Centaurus*, vol. 10, no. 4 (1965), pp. 258–82.

<sup>6</sup> It is not the object of this paper to examine the answers that were given to these questions; but their *prevalence* ensured that irrespective of whether the answer was in the affirmative or the negative, that answer would be of great interest in its own right—quite independently of vitalist reference.

<sup>7</sup> It is in fact possible to distinguish five very different senses in which a statement of analogy breakdown may be construed:

- (i) as a characterization or definition of a species of vitalist belief.
- (ii) as evidence for a prior vitalist belief.
- (iii) as an assertion of intrinsic interest.
- (iv) as an informal, and perhaps superficial, contrast between the states of inorganic and organic theory. (Cf. Berzelius in note 5.)
- (v) as an analytic proposition, defining a demarcation criterion for organic status. (Cf. Liebig and Dumas, “Dans la chimie minérale, les radicaux sont simples; en chimie organique les radicaux sont composés. Voilà toute la différence.”) *Comptes Rendus Ac. des Scs.*, **5** (1837), p. 567f.

In the light of the fact that it is difficult to see how certain statements of analogy breakdown could possibly support a vitalist position, it must not be assumed too readily that senses “i” and “ii” generally have the monopoly. For some years Berzelius advocated the omnipresence of oxygen among organic compounds, but to suggest that because he could not say the same for inorganic compounds, he was entitled to call himself a vitalist, would be absurd.

<sup>8</sup> Having presupposed that statements of analogy breakdown were construed in the first sense defined in note 7, Jean Jacques [*Revue D'Histoire des Scs.*, **3** (1950), pp. 32–66] has imposed an interpretation, on the literature of the period, which is perhaps as misleading as it is methodical. He is able to characterize the first half of the nineteenth century as a period during which a “chemical vitalism” defined in terms of a basic difference between

## I. AN EXPLORATION OF FIVE AMBIGUITIES

Consider the pessimistic assertion: "WE CANNOT, FROM ITS ELEMENTS, PRODUCE AN ORGANIC BODY AS NATURE DOES". For convenience, the more articulate versions of this statement which are prominent in the literature, and which incorporate the same ingredients, will hereafter be designated as "X-statements". Taken at face value, an X-statement akin to the above would seem to be denying the possibility of the artificial production of organic compounds; and it is this interpretation, imposed upon pre-1828 X-statements, which is responsible for the popular reverence for Wöhler's urea: X-statements were falsified when urea was synthesized. Unfortunately for the popular view, the model X-statement is susceptible of at least 32 different interpretations, corresponding to ambiguities lurking in the ingredients: "we cannot"; "from its elements"; "organic"; "as Nature does"; and "body".<sup>9</sup> By resolving these ambiguities in turn, the insignificance of the urea synthesis, *qua* synthesis, may be demonstrated.

(A) *The proscriptive and descriptive senses of "we cannot"*

To assert that "we cannot land a man on the moon" might be to deny the possibility of the feat—or, more probably, it might be to lament the fact that as yet we have not succeeded in depositing anyone there. "We cannot" may carry a descriptive, rather than a proscriptive denotation. Correspondingly, a

inert matter and that of biological origin, successively gave way to similar vitalisms constructed upon the inapplicability of Proust's law of fixed proportions to organic compounds, and upon the apparent impossibility of organic synthesis. To reinterpret all the passages which Jacques adduces to confirm his thesis would require more than a footnote, but it is not only because he neglects the distinctions of note 7 that his account is suspect. For, with explicit reference to Berzelius, Jacques insists that "c'est surtout la question de savoir si la loi des proportions de Proust est valable pour la chimie organique qui offrira la refuge le plus durable au vitalisme en analyse organique". (p. 49.) But when Berzelius wrote in 1819 that "Les lois qui *limitent* les combinaisons des atomes élémentaires dans la nature organique diffèrent beaucoup de celles qui régissent la nature inorganique, et permettent une telle multiplicité dans les combinaisons *qu'on peut dire qu'il n'y existe aucune proportion déterminée*", he was *not* saying that Proust's law of definite proportions was inappropriate for organic compounds. He was using the word "determined" *not* in the sense of "fixed" or "characteristic" but rather in the sense of "regulated by general patterns of combination", analogous to the *multiple* proportions of the inorganic world. Berzelius had stated quite categorically in 1814 (*Annals of Philosophy*, IV (1814), p. 326) that "determinate" proportions in Proust's sense of "invariable" and "characteristic" were operative in both kingdoms of nature. Consequently a "disparity" which Jacques locates as a cave of refuge for timid vitalists, was already refuse—for Berzelius at least.

<sup>9</sup> Bearing in mind that five senses have already been distinguished (note 7) in which a statement of analogy breakdown might be construed—supposing its *content* to be unambiguous—it follows that at least 160 different interpretations are available for the explication of a given X-statement. If this conclusion should appear ostentatious, it is nonetheless an indication of the complexity of the problem.

chemist of the 1820s or '30s who assents to an X-statement may only be lamenting current practical failure, and not at all pronouncing as impossible the artificial production of organic compounds. If this was commonly the case then the most that Wöhler's preparation could have achieved was to provide a practical stimulus to other chemists by confirming that previous failure was to be ascribed only to lack of practical or theoretical finesse, and not to the sheer impossibility of the enterprise.

While it might be dangerous to generalize, several of the most eminent chemists were explicitly optimistic. At the turn of the century Berthollet set a precedent when he argued that the formation of vegetable products did not presuppose anything other than the normal interplay of chemical affinities,<sup>10</sup> and Chevreul, in 1824, could remind his readers: "I have shown that among the materials which constitute organized beings it is admissible to distinguish between organic and inorganic compounds, because there exist numerous compounds that are found only in animals and plants, and which cannot be produced by any chemical process now known". "However", he advised, "to regard the distinction as absolute and invariable would be contrary to the spirit of science".<sup>11</sup> In the light of a recent verdict that "few, if any chemists deliberately attempted the synthesis of an organic compound",<sup>12</sup> what Chevreul proceeds to say is of particular interest: "To regard the distinction [between inorganic and organic compounds] as absolute . . . would be to admit the futility of *all the attempts* that have for their objective to produce compounds identical with or analogous to those that are now considered as peculiar to organized beings."<sup>13</sup> Similarly in England, Thomas Thomson, one-time editor

<sup>10</sup> C. L. Berthollet: the formation of vegetable products "ne supposent ni d'autres affinités, ni d'autres principes d'action, que ceux qui produisent les effets chimiques. . .". (Appendix to vol. II of "Essai de Statique Chimique", Paris, 1803, pp. 482 and 483.)

<sup>11</sup> Cited by P. Lemay and R. E. Oesper, "M. E. Chevreul", *J. Chem. Educ.*, **25** (1948), p. 68f.

The same ambiguity surrounding the phrase "we cannot" is to be found in Thenard and Fourcroy, and in both cases the resolution must favour an optimism:—Of the vegetable substances, Thenard wrote, "nous n'en pouvons former aucune de toutes pièces", but he hastened to add that only those who were ignorant of science would read anything into this impotence, and he attributed current failure primarily to the recalcitrant states of the elements themselves. (*Traité de Chimie*, Paris, 1815, III, pp. 3 and 4.)

Similarly, Fourcroy conceded that "aucun instrument de l'art ne peut imiter les compositions qui se font dans les machines organisées des plantes" (*Philosophie Chimique*, 3rd ed., 8vo, 1806, p. 303), but his mechanical metaphor would seem to preclude a vitalist interpretation.

<sup>12</sup> O. T. Benfey, *From Vital Force to Structural Formulae* (Boston, 1964), p. 12.

<sup>13</sup> It would of course be misleading to suggest that a dearth of attempts to prepare organic compounds *could* be correlated with an excess of vitalists. So primeval was the forest of organic compounds that the major priority was felling rather than planting.

of *Annals of Philosophy*, and who was thereby conversant with ideas circulating on the Continent and on the home front, far from expressing doubts about the artificial fabrication of organic products, was quietly contemplating the likely economy of such processes.<sup>14</sup>

To emphasize a disjunction between statements of analogy breakdown and statements of vitalist belief, is not to misrepresent the chemists of the '20s and '30s, for Berzelius could maintain that any proof of the existence of unique forces in Nature, based on the current inability of chemists to imitate organic products, was no proof at all.<sup>15</sup> Consequently it would be a serious oversimplification to characterize the period 1800–35 as one during which "chemists were sceptical about the possibility of organic synthesis because they respected a unique and inimitable vital force active in organisms"; and the very fact that distinguished chemists did not doubt the possibility of artificial productions must detract from any profound conceptual significance which Wöhler's urea might have had.

(B) *The direct and indirect senses of "from its elements"*

From what has been said so far, it would be natural to expect that although discussions of "vital force" may have survived, X-statements should *not* be found long after 1828. Wöhler's preparation, although perhaps not as dramatic as has often been implied, was surely a genuine preparation of an organic product from inorganic material? It is, however, the case that X-statements abound in the literature for many years after Wöhler's experiment had won general acclaim. And it is this fact which now requires explication, and which will lead to an analysis of the second ambiguity—namely that concealed within the phrase, "from its elements".

A popular English textbook, Turner's *Elements of Chemistry*, allows a "controlled" assessment of the impact of Wöhler's discovery, since there are 1827 and 1831 editions. In the earlier volume Turner stated that a "circumstance characteristic of organic products is the impracticability of forming them artificially by direct union of their elements".<sup>16</sup>

In the light of the above analysis of the proscriptive/descriptive ambiguity, it is significant that Turner should select the word "impracticability" in preference to "impossibility", but the remarkable point is that in 1831 the same statement is repeated. Although he was now well acquainted with Wöhler's

<sup>14</sup> T. Thomson: "Should chemists, in consequence of the knowledge acquired by future investigations, ever arrive at the knowledge of the mode of forming [animal and vegetable] principles from their elements *at a cheap rate*, the prodigious change which such a discovery would make upon the state of society must be at once evident". (*History of Chemistry*, London, 1830, vol. II, Ch. VII, p. 317) [Italics mine].

<sup>15</sup> J. Berzelius, Letter to Agardh, 1831. (*Bref.* (ed. Söderbaum), 4, p. 71.) (Translated by Jørgensen in *J. Chem. Educ.*, **42**, (1965), p. 395f.)

<sup>16</sup> E. Turner, *Elements of Chemistry*, Edinburgh, 1827, p. 513.

work,<sup>17</sup> he could still affirm that a "circumstance characteristic of organic products is the impracticability of forming them artificially by direct union of their elements".<sup>18</sup>

It has been argued above that to some chemists Wöhler's experiment would not be astonishing because they had already entertained the possibility of such reactions "in vitro". It follows from Turner's account that to other chemists, Wöhler's result was irrelevant, but for a different reason. Whatever it was that was "impracticable" in 1827 was still impracticable, despite Wöhler's urea. Of three possible resolutions of the apparent dilemma posed by this persistent affirmation of X-statements, the first two worthy of consideration have appeared in earlier discussions of Wöhler. A third—and possibly more satisfactory—solution will then be offered, derived from a closer scrutiny of the phrase "from its elements".

(i) The first solution relies on the refusal to grant urea organic status. Wöhler had certainly synthesized urea, but if it were not generally recognized as an organic compound, X-statements would remain intact. Now some chemists and physiologists *did* react to the urea preparation by denying that organic status. To Berzelius it seemed that urea occupied the "milieu"<sup>19</sup> between inorganic and organic substances; and Müller declared that "urea . . . can scarcely be considered as organic matter, being rather an excretion than a component of the animal body".<sup>20</sup> Although such a relegation of urea is consonant with the thesis that Wöhler's discovery was not critical, it is so in a disturbing way, because if one were arguing *for* the revolutionary nature of Wöhler's result, one would undoubtedly exhibit Berzelius and Müller as two vehement vitalists, seeking to evade the emergency.

There are, nevertheless, three good reasons why this resolution of the dilemma, raised by the persistence of X-statements, is unsatisfactory as a general account of the reception of the urea synthesis:

(a) Many chemists certainly did not consign urea to a chemical "no man's land", and these included Liebig, Turner, and others who still pounded X-statements.<sup>21</sup>

<sup>17</sup> E. Turner, *Elements of Chemistry*, 3rd ed., London, 1831, p. 801.

<sup>18</sup> *Ibid.*, p. 67of.

<sup>19</sup> J. Berzelius, *Traité de Chimie*, transl. Esslinger (Paris, 1831), vol. V, p. 12.

<sup>20</sup> J. Müller, *Elements of Physiology*, transl. W. Baly, 2 vols., London (1838, 42), vol. I, p. 3 (footnote).

<sup>21</sup> Liebig, for example, would not entertain an exiled urea: "It may be affirmed with the utmost certainty, that urea and uric acid are products of the organization . . ." (Notices and Abstracts of communications to the British Assoc. for Adv. of Sc. Liverpool meeting, Sept. 1837 p. 38f.). And yet in his *Chemical Letters* he continued to make X-statements (1st ed., London (1843, '44), p. 125; 3rd ed., London, 1851, p. 175).

(b) Berzelius and Müller had peculiar motives for their conventionalist manoeuvre. Urea was transported to a no-man's land not so much because it had actually been prepared artificially, as because its mode of preparation held embarrassing implications for a "structural" theory of organic compounds to which they were committed. The structural ramifications of the urea synthesis will be discussed in the final section of the paper, so it will suffice to say that the electrochemical theory of Berzelius, which was winning its adherents, envisaged all inorganic combinations as the addition of an electropositive to an electronegative component. Thus, a neutral inorganic salt would be represented as if it contained an acid and a base as preformed constituents.<sup>22</sup> Now, in 1828, Berzelius and Müller, with their respective disciples, were not convinced that organic compounds could be made to comply with this electrochemical scheme.<sup>23</sup> But Wöhler had just prepared urea from ammonia and cyanic acid, the former a base and the latter an acid, and so one could conclude that urea actually contained ammonia and cyanic acid as preformed parts. And it was primarily this conclusion—that an organic compound could be envisaged as a salt—which Berzelius and Müller were evading when they abandoned urea.<sup>24</sup> Accordingly, when the Swedish master gave his reasons for removing urea to "no man's land", first and foremost he referred to its composition, secondly to its crystalline properties, and last of all to its having been synthesized.<sup>25</sup>

<sup>22</sup> Perhaps the most accessible exposition of his theory is in Berzelius, *Théorie des Proportions Chimiques*, 2nd ed., Paris, 1835, p. 34f.

It should also be added that Berzelius' attitude to his "dualism" was far more conventionalist than is usually supposed. See, for example, the letter of Berzelius to Robert Hare, 23rd Sept., 1834. (*Bref.*, ed. Söderbaum, sect. VII, p. 141-2.)

<sup>23</sup> Müller borrowed, from Berzelius and Gmelin, the view that whereas in *minerals* the elements are never observed to combine 3 or 4 together, so as to form a compound in which each element is equally united with all the others, in *organic* products the proximate principles result from a unitary fusion of all the elements with each other. (Müller, *op. cit.*, I, p. 3.) Nevertheless, Berzelius was less committed to a dichotomy between inorganic and organic domains than Müller's account implies, for in vol. V of his *Traité* (Paris, 1831), Berzelius could say of the attempts to introduce a dualism into organic compounds, via their elements, "nos connaissances dans ce champ ne sont pas encore assez mûres, pour que nous puissions dire avec certitude jusqu'à quel point cette manière de voir est admissible . . ." (pp. 6 and 7).

<sup>24</sup> Cf. Müller, *op. cit.*, I, p. 38: urea is analogous to a binary compound, "and is perhaps really one".

<sup>25</sup> Berzelius: Urea "tient, en quelque sorte, le milieu entre les substances organiques et inorganiques, relativement à sa composition et à sa propriété de cristalliser, et elle peut être fabriquée de toutes pièces". (*Traité*, vol. V, 1831, p. 12.)



- (c) It is not even clear that a majority opinion favoured Berzelius' unitary (and therefore potentially holistic) conception of an organic compound. After he had expounded his own views in the *Traité*, Berzelius had to confess that "many chemists"<sup>26</sup> did not share them.

(ii) The second resolution of the dilemma raised by the viability of X-statements is that popularized by McKie. In an influential article<sup>2</sup>, he has ascribed the persistence of X-statements to the fact, not that there was something 'dubious' about urea, but that there was something 'dubious' about the synthesis. Because Wöhler's cyanates, McKie argues, were ultimately obtained from hooves, horns and desiccated blood; i.e. from organic material, Wöhler only achieved a "transformation" from one set of organic material to another, and the term "synthesis" is an utter misnomer.

In its favour it must be said that McKie's thesis does illuminate the well-known query of Wöhler himself, who asked Berzelius, "Can the artificial formation of urea be regarded as an example of the formation of an organic, from an inorganic, substance? It is striking," Wöhler explained, "that for the production of cyanate (and also of ammonia) an organic substance is still ultimately required, and a 'Naturphilosophe' might say that the organic part had not disappeared from the animal charcoal or from the cyanogen compounds made from it."<sup>27</sup> Against McKie's thesis it must be said that this query can be construed not as an expression of doubt, from Wöhler, as to whether he had performed a proper synthesis, but rather as a speculative comment on the behaviour of a particularly obdurate "Naturphilosophe".<sup>28</sup> In favour of this interpretation, it must be stressed that even among chemists who reserved a place for vital forces within an organism, the notion of a vital force inhering within a specific element or compound was vigorously repudiated.<sup>29</sup> Further more, it was common practice to label as inorganic matter the ultimate

<sup>26</sup> "Beaucoup de chimistes envisagent la composition organique d'une manière différente de celle qui vient d'être exposée." *Ibid.*, p. 6.

<sup>27</sup> Letter of Wöhler to Berzelius, Feb. 22nd, 1828. (*Correspondence*, ed. O. Wallach, Leipzig, 1901, vol. I, p. 206f.)

<sup>28</sup> This is the interpretation offered by the most recent commentaries, e.g. E. Campaigne, "Wöhler and the Overthrow of Vitalism", *J. Chem. Educ.*, **32** (1955), p. 403. T. O. Lipman, *op. cit.*, p. 453. B. S. Jørgensen, *J. Chem. Educ.*, **42** (1965), p. 394f.

<sup>29</sup> Cf. Berzelius, *Traité de Chimie*, 5th ed., Paris (1845-50), vol. V, p. 2. Cf. W. Prout, "There is nothing peculiar in the elements of which organized beings are composed. . .". (*Bridgewater Treatise: Chemistry, Meteorology and the Function of Digestion considered with reference to Natural Theology*, 2nd edit., London, 1834, p. 444.)

decomposition products of animal substances.<sup>30</sup> That Wöhler had recourse to hooves and horns did *not* render the synthesis defective, and indeed there were chemists who quite explicitly acknowledged that an organic compound had been procured from inorganic material.<sup>31</sup> As Berzelius reminded Liebig, "we usually regard cyanic and hydrocyanic acids as inorganic compounds".<sup>32</sup>

If McKie's thesis were correct it would endorse the insignificance claim for Wöhler's urea, but there are two other reasons which strongly suggest that McKie has the right answer for the wrong reason:

- (a) The term "synthesis" is not a misnomer, for Scheele had produced potassium cyanide, resorting to charcoal and graphite as sources of carbon, and ammonia had been produced from its elements in the 1780s. As Partington has so succinctly stated, "Wöhler's synthesis of urea in 1828 was, therefore, complete."<sup>33</sup>
- (b) If McKie were correct, one would surely anticipate at least some controversy during the 1830s as to whether Wöhler had effected a complete synthesis. As it is, some chemists appear to have regarded it as complete, and others as a transformation,<sup>34</sup> but this does not seem to have been a

<sup>30</sup> Cf. Lipman, *op. cit.*, p. 456.

It was, in fact, a major tenet of the species of vitalism fostered by Bichat and his colleagues, that after death, where no vital force could resist the corruptive action of chemical affinity, decomposition took place in such a way as to produce *inorganic* compounds.

<sup>31</sup> Berzelius referred to a fabrication "de toutes pièces" (note 25) as did Gay-Lussac (*Cours de Chimie*, 2 vols., Paris, 1828, vol. II, Lesson 33, p. 12).

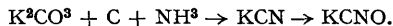
Similarly, Mitscherlich reported that urea had been prepared, "sans l'emploi d'une combinaison due au règne organique" (*Elémens de Chimie*, transl. M. B. Valérius, 3 vols., Brussels, 1835-36; vol. I, p. 160).

Some years later, Hofmann specifically distinguished between transformations and preparations from the elements, and he assigned the urea to this latter class. (*Medical Times and Gazette*, vol. 6, Feb., 1853, p. 132.)

<sup>32</sup> Letter of Berzelius to Liebig, *Ann. Pharmacie*, 6 (1833), p. 173f. Also reproduced in *J. de Pharmacie*, 19 (1833), p. 618.

<sup>33</sup> J. R. Partington, *A History of Chemistry*, vol. IV, London, 1964, p. 260.

Although Wöhler obtained his cyanate by oxidation of a cyanide derived from hooves and horns, the point is that Scheele's method could also have been used:



<sup>34</sup> To Gerhardt's assertion that, "Tous les produits artificiels de nos laboratoires . . . sont tous moins complexes que les substances d'où ils résultent", Laurent felt obliged to reply, "Avec du charbon et de l'ammoniaque ne fait-on pas de l'acide hydrocyanique, et avec celui-ci des cyanates . . . de l'urée etc?" (*Revue Scientifique et Industrielle, de Quesneville*, 10 (1842), pp. 150 and 369f.) That Laurent's correction should be taken at face value, and not interpreted as a word of admonition to an obdurate vitalist, is clear from Laurent's response to Gerhardt's statement that the 'vital force' achieves the complexification of organic compounds. Far from decrying such a view, Laurent implicitly condoned a

point of dissension among them. That it is the historians, and not the chemists, who have gone to battle on this question might be a further indication that, even as a complete synthesis, Wöhler's reactions were not of momentous import.<sup>35</sup>

"descriptive" reading of it, when he quoted a passage from his own doctorate thesis of six years previous: "Il résulte . . . qu'en général les moyens employés par les chimistes pour transformer les corps les uns dans les autres, tendent à les faire converger vers des combinaisons de plus en plus simples, tandis que la nature opère en sens inverse dans les végétaux. . .". This simply was the case.

To contrast Gerhardt's "vitalism" with Laurent's "matérialisme rigoureux", as does Jacques ("Essai bibliographique sur l'oeuvre et la correspondance d'Auguste Laurent", Institut Grand-Ducal de Luxembourg, Extrait des Archives, vol. XXII, New Series, 1955, p. 16) is to magnify a correction into dissension, and to miss the point that by "vital force", Gerhardt usually meant a "*reducing-force*" operative in organisms: Le chimiste "remonte l'échelle, en appliquant aux substances organiques des procédés de réduction, et en cela il imite en quelque sorte la force *réductrice* que possèdent les parties vertes des plantes sous l'influence de la lumière". (*Revue Scientifique et Industrielle de Quesneville*, XIV (1843), p. 585.) [Italics mine.]

I am very grateful to Dr. W. V. Farrar for having lent me a copy of Jacques' "Essai bibliographique".

<sup>35</sup> A further clue which suggests that McKie has relied too heavily on the distinction between a complete synthesis and a mere transformation, is provided by the behaviour of the words "artificial" and "synthesis". For they are *both* used to cover both synthesis and transformation; i.e. the chemical language of the 1830s, and subsequently, did not presuppose a *significant* distinction. Thus Pelouze and Frémy noted that "L'acide butyrique que l'on extrait difficilement des produits de la saponification du beurre, se produit *artificiellement* dans la fermentation du glucose". (*Cours de Chimie Générale*, Paris, 1848-50, III, p. 40.) [Italics mine.]

And in Berzelius' *Traité*, after Liebig's empirical formula for quinine is noted (C<sup>20</sup>H<sup>34</sup>N<sup>2</sup>), it is written: "En cherchant à confirmer le résultat par la *synthèse* de l'hydrochlorate, Liebig trouve que 100 parties de quinine absorbent 24.1 de gaz hydrochlorique . . ." (*Traité*, 1831, V, p. 157). [Italics mine.]

It may be a failure to examine the behaviour of these words which led McKie to promote a serious misrepresentation of Gerhardt's position. In his desire to display eminent vitalists, untouched by Wöhler's urea, McKie quoted a discussion from Gerhardt, and summarized it with the words: "Gerhardt . . . held that the chemist could no more succeed in . . . rebuilding (organic compounds) than the alchemist could attain an aim that seems less difficult." (McKie, *op. cit.*, p. 610.) Closer scrutiny of the relevant passage will show that this summary cannot be sanctioned:—

Gerhardt wrote: "En présence de ces belles découvertes et de tout d'autres dont la chimie organique s'est enrichie depuis ces dernières années, celui qui ne considérerait que le fait de la reproduction artificielle, sans avoir connaissance des procédés, serait presque tenté d'attribuer au chimiste un pouvoir surnaturel, un pouvoir magique. A le voir ainsi enfanter dans ses cornues et ses creusets, les créations de la nature vivante, pourquoi en effet, ne lui accorderait-on pas la puissance de transmuter les métaux et de faire de l'or, ce qui semble cependant moins difficile? Un observateur superficiel pourrait seul se bercer de ce fol espoir." (*Revue Scientifique et Industrielle, de Quesneville*, 10 (1842), p. 147.)

(iii) McKie's thesis of the partial nature of Wöhler's preparation does draw attention to the importance of the phrase "from its elements" which occurs frequently in post-1828 X-statements. There is however an ambiguity in this phrase itself which has not been explored, and one sense of which allows the assertion that Wöhler's production of urea was effectively the complete synthesis of a genuine organic product, and also allows X-statements to be in general currency afterwards. The phrase "from its elements", as it appears in post-1828 X-statements is usually accompanied by the word "directly". It will be remembered that in the 1827 and 1831 editions of his textbook, Turner had said that a "circumstance characteristic of organic products is the impracticability of forming them artificially by *direct* union of their elements".<sup>36</sup> When in 1848, Gerhardt was reflecting on the past twenty years of progress in organic chemistry, he was happy to report that not only urea, but several new alkaloids had been obtained artificially. Moreover, Gerhardt cherished the hope that quinine, morphine and narcotine would shortly be generated in the laboratory. But—and this is the crucial point—he emphasized that "it is not by *directly* combining nitrogen with carbon, hydrogen and oxygen that these artificial reproductions have been successfully carried out".<sup>37</sup> The point is that there is a very important distinction to be made—a distinction which permits us to say that Wöhler *had* produced urea *from its elements*, where "from its elements" designates a complete synthesis achieved with the co-operation of intermediate compounds containing elements, such as potassium, other than carbon, hydrogen, oxygen and nitrogen; and yet which also permits us to say that Wöhler had *not* produced urea directly "from its elements", where "from its elements" designates synthesis from carbon, hydrogen, oxygen and nitrogen *alone*.<sup>38</sup> Consequently, the persistence of X-statements may perhaps best be ascribed to the fact that *irrespective* of whether Wöhler's preparation was complete or a

The precise denotation of this paragraph is not easy to define: was Gerhardt's "observateur superficiel" supposed to be a stupid savant or a shrewd dilettante? Nevertheless it would have to be misconstrued to yield McKie's conclusion that Gerhardt is here pointing to the folly of even attempting to simulate nature's operations. In fact what Gerhardt says is that in the light of the fine discoveries enriching organic chemistry during recent years, he who cast a cursory glance at the "*fact* of artificial production", without taking cognisance of the methods employed, would be "*tempted*" to attribute a magic power to the chemists. The fact of "artificial production"—in the sense of transformation—is assumed, not denied.

<sup>36</sup> Italics mine. (Cf. 16 and 18.)

<sup>37</sup> C. Gerhardt, *Introduction à l'étude de la chimie par le système unitaire*, Paris, 1848, p. 187. [Italics mine.]

<sup>38</sup> This same distinction is endorsed by Prout's admission of 1834: "we cannot . . . produce artificially either sugar or any other organic compound, by *directly* combining their elements; because we cannot bring the elements together, precisely in the requisite states and proportions". (Prout, *op. cit.*, p. 421.) [Italics mine.]

mere transformation, it did not alleviate either a descriptive or proscriptive depression engendered by the failure to produce organic compounds from their elements alone.

Although the distinction between a direct and indirect synthesis may seem gratuitous, it was in fact basic, granted the conceptual framework of the '20s and '30s. There are at least four good reasons why chemists should have been pre-occupied with reference to *direct* synthesis:

- (a) If one subscribed, as Berzelius and Müller had done, to a unitary—and potentially holistic—conception of an organic compound, if one could agree with Chevreul that “the majority of neutral, ternary and quaternary organic compounds appear to be formed . . . directly from oxygen, carbon and hydrogen”,<sup>39</sup> then the only conceivable way in which to execute an artificial preparation was by fusing together the elements alone.<sup>40</sup>
- (b) It has been emphasized that a major issue during the whole period under discussion was the question of resemblances and discrepancies between inorganic and organic compounds, and that statements of analogy breakdown can therefore boast an *intrinsic* interest. Now, inorganic compounds, even the double salts like potassium aluminium sulphate<sup>41</sup> were thought<sup>42</sup> to have been synthesized directly from their elements *alone*.
- Consequently our viable X-statements could simply be understood as statements of analogy breakdown, and not as propositions parasitic on vitalist belief.
- (c) Permeating the literature were rather primitive views about the means whereby a plant performed its synthetic functions. As late as 1844, Dumas could stipulate that for the plant to form neutral non-nitrogenous matter, “it suffices . . . to unite carbon with water or its elements”,<sup>43</sup> and similarly to produce neutral nitrogenous matter, “it suffices to unite

<sup>39</sup> Cited by Pelouze and Frémy, *Cours de Chimie Générale*, Paris, 1848–50, III, p. 884.

<sup>40</sup> Cf. Hofmann, “Chemists were long of opinion, that most organic bodies were simply formed by the juxtaposition of their carbon, hydrogen, nitrogen and oxygen, without any further subdivision . . .” (*Medical Times and Gazette*, vol. 6, 1853, Lecture VI, p. 418.)

<sup>41</sup> In the early 19th century such salts, formed from two bases and one acid, were called triple salts.

<sup>42</sup> For the significance of the “were thought . . .” see Laurent’s discussion in his *Chemical Method*, transl. W. Odling, London, 1855, p. 18.

<sup>43</sup> Dumas and Boussingault, *Essai de Statique Chimique des êtres organisés*, 3rd ed., Paris, 1844, p. 30.

carbon and ammonium with the elements of water."<sup>44</sup> This persuasion that plants fabricated their material from the four elements directly would naturally induce comparison with what the chemist could do with these elements alone, and because the chemist entertained an exalted opinion of what a plant could do, his own efforts would seem pallid by comparison.<sup>45</sup>

- (d) Again quite independently of any vitalist notions, the recalcitrant nature of the elements themselves would induce a pessimism about direct synthesis. Three of them condescend to appear as colourless, odourless gases; carbon in its various sooty guises could evoke only despondency; and the immediate compounds of these elements were no less frustrating. Thus Gerhardt complained of the perversity of nitrogen: in the free state it did not appear to react with anything.<sup>46</sup> As for carbon: "amidst the wonders of creation," wrote William Prout in fine English style, "it is perhaps difficult to say what is most wonderful; but we have often thought, that the Deity has displayed a greater stretch of power, in accommodating to such an extraordinary variety of changes, a material so unpromising and so refractory as charcoal, and in finally uniting it with the human mind; than was requisite for the creation of the human mind itself."<sup>47</sup> This is not a trivial point since Thenard could affirm that "if carbon, hydrogen, and oxygen were liquids, *nothing* would stop them from combining . . . at ordinary temperatures, and it is probable that we could then form a large number of vegetable substances".<sup>48</sup>

By way of clarification: two senses in which Wöhler's synthesis was insignificant have so far been distinguished:

- (i) For several chemists the synthesis of organic compounds was not a live issue, and their pre-1828 X-statements are to be construed descriptively and not proscriptively.

<sup>44</sup> *Ibid.* p. 30.

Similar prescriptions will be found in Dumas (*J. de Pharmacie*, 20 (1834), p. 269) and Liebig (*Traité de Chimie Organique*, transl. Gerhardt, Paris, 1840-44, vol. I, p. xcvi).

<sup>45</sup> If one believed with Prout (*op. cit.*, p. 421) that sugars were literally compounds of carbon and water, one would naturally attempt to prepare them by co-ercing carbon and water into union—and one would naturally be disappointed.

Nor could the chemist emulate the plant's feat of so calmly decomposing the carbon dioxide of the atmosphere into *nascent* carbon, ready for synthesis. (Cf. Dumas, *Essai* . . . (1844), p. 26.)

<sup>46</sup> Gerhardt, *Introduction à l'étude de la chimie par le système unitaire*, Paris, 1848, p. 187.

<sup>47</sup> Prout (*op. cit.*) 1834, p. 446.

<sup>48</sup> L. J. Thenard, *Traité de Chimie*, Paris, 1815, vol. III, pp. 3 and 4. [Italics mine.] Cf. H. Rose, "On the combinations of ammonia with carbonic acid", *Pogg. Ann.*, vol. xlvi, part 3; translated in Taylor's *Scientific Memoirs*, vol. II (1841), p. 98.

- (ii) Not only was Wöhler's discovery irrelevant to the question of a direct synthesis from carbon, hydrogen, oxygen and nitrogen,<sup>49</sup> but scepticism about this possibility was often derived from sources not directly associated with vitalist belief.<sup>50</sup>

<sup>49</sup> The distinction between a direct and an indirect synthesis has the additional advantage of clarifying the significance of Berthelot's syntheses. In November, 1855, he was responsible for perhaps the first compelling example of a *direct* synthesis when he obtained formic acid from carbon monoxide and steam. Subsequently he achieved the *direct* synthesis of acetylene—with all its synthetic potential—from carbon and hydrogen. But to claim, as Berthelot did, that it was *his* syntheses which terminated the reign of vital forces is to be very myopic. When Liebig nominated Berthelot for foreign membership of the Bavarian Academy, he did not congratulate him for any banishment of vital forces, but he did say that it was Berthelot "who first succeeded in producing formic acid from carbon monoxide and alcohol from ethylene, by *direct* synthesis" (cf. E. Jungfleisch: "Notice sur la vie et les travaux de M. Berthelot", *Extrait du Bulletin de la Société Chimique de France*, 1913, p. 61). [Italics mine.]

<sup>50</sup> In order to illustrate how the distinctions defined so far do demand a reinterpretation of oft-quoted discussions, it will be convenient to indulge in a demythologized exegesis of an account which appears in the several editions of Liebig's 'Chemical Letters'.

"We can make an alum crystal from its constituent elements", Liebig declared "because we can up to a certain point dispose the chemical affinity of these elements according to our will, and so determine the arrangement of the atoms. . . . We cannot reproduce [the molecule of sugar] because the force responsible for arranging its elements in the form appropriate to the sugar molecule is not subservient to our will." (Translated from the French ed., cited by Jacques in *Revue d'Histoire des scs.*, 3, 1950, p. 57. See also London, 1844 ed., p. 125.)

Now Jacques infers from this that "Liebig croit donc, lui aussi, la synthèse impossible . . ." (*supra* p. 57). There are, however, four convincing reasons why this is an erroneous generalization:

- (i) Liebig's explicit comparison with the alum crystal characterizes his declaration as a statement of analogy breakdown, and this in turn sanctions the interpretation of the phrase "from its constituent elements" in the sense of "from its elements *directly*". In which case Liebig is not denying the possibility of organic synthesis: he is simply saying that it is not yet possible to obtain *sugar* by *directly* combining its elements.
- (ii) In a note to the British Association, in 1837, Liebig had referred to the artificial production of organic compounds as his "final object of investigation", an object he was "certain" would be attained. (Notices and abstracts of communications to the British Assoc., Liverpool meeting, Sept., 1837, p. 41f.)
- (iii) The following year (1838) saw Liebig specifically forecasting the artificial synthesis of sugar. With Wöhler he announced that "sugar, salicine, morphine will be artificially prepared. As yet we are ignorant of the road by which this end is to be reached, since the proximate constituents required for building up these substances are not yet known to us; but these the progress of science cannot fail to reveal. . .". (Transl. by Hofmann in his Faraday Lecture for 1875, *The Life Work of Liebig.*, London, 1876, p. 89)

(C) *The problem of "organic" status*

All treatments of this period which assume that a prevalent attitude among chemists and physiologists was inimical to the synthesis of organic compounds tend to presuppose that the concept of an "organic" compound was non-problematic. A demonstration that there was no universally accepted criterion for organic status would jeopardise yet again the popular impression. For many years the presence of carbon within a compound most certainly was not a sufficient condition for organic status.<sup>51</sup> Thus in Dumas' *Traité*<sup>52</sup>, marsh gas, ethylene, naphthalene and other carbon compounds were subsumed under inorganic headlines.<sup>53</sup> Indeed Berzelius suffered from an obsession with oxygen as the centre around which the whole of chemistry turned, and at times it was oxygen which he seemed to regard as a necessary condition for organic status.<sup>54</sup>

It has generally been accepted that the origin of a compound was taken as the determining criterion. If it were located in a plant or animal, then a compound would be deemed "organic". This natural assumption is, however, a serious oversimplification. As early as 1817 Thomas Thomson had classified a whole string of acids as "combustible acids", and had reported that they *used* to be called "vegetable" and "animal" acids.<sup>55</sup> Furthermore, apart from the fact that in animals several acids were identified, such as sulphuric and phosphoric, which could belong equally well to the mineral kingdom,<sup>56</sup> there were other familiar components of vegetable matter the organic credentials of which were suspect. Indeed, the inadequacy of *origin* as a criterion was so obvious to Dumas, that when he could replace it with no other, he was on the brink of

- (iv) Precisely the same controversial passage is repeated in the 1851 London edit. of Liebig's letters (p. 175), by which time Liebig most certainly was not denying organic syntheses, and by which time he had given a reductionist account of what he meant by "vital force": "The term 'vital force' in the present state of [the] science, does not denote a force *per se* . . . but it is a collective term, embracing all those causes on which the vital properties depend." (*Ibid.*, p. 166.)

<sup>51</sup> It is true that Döbereiner dealt with organic compounds in a carbon context (*Grundriss der allgemeinen Chemie*, Jena, 1816 and 1826), but not until the 1840s was organic chemistry unequivocally defined as the chemistry of carbon compounds. (Gerhardt, *Précis de Chimie Organique*, Paris, 1844-45, vol. I, p. 1.) For other relevant references see J. R. Partington, *op. cit.*, pp. 239, 240.

<sup>52</sup> *Traité de chimie appliquée aux Arts*, 8 vols., 1828-46, vol. I.

<sup>53</sup> M. Delacré: *Histoire de la Chimie*, Paris, 1920, p. 370.

<sup>54</sup> See, for example, his *Essai sur la théorie des proportions chimiques*, Paris (1819), p. 42.

<sup>55</sup> T. Thomson, *System of Chemistry*, London, 1817, vol. II, p. 123.

<sup>56</sup> Hofmann was later to say that the presence of mineral compounds in plants and animals made it *obvious* "that the definition representing organic chemistry as that branch . . . which treats of the substances composing the structure of plants and animals cannot be admitted without limitation". (*Medical Times and Gazette*, vol. 6, 1853, p. 132.)



dismissing the problem, by rejecting any distinction between the inorganic and the organic spheres.<sup>57</sup> Oxalic acid affords a telling example of the difficulties, for not only was it a well-known component of rhubarb and other vegetables, but it had also been artificially produced by Wöhler in 1824, by the action of water on cyanogen.<sup>58</sup> According to popular accounts, since oxalic acid was an organic product, and since organic products supposedly defied artificial fabrication, it should have been this preparation which spelled the demise of vital force. That it was ignored can be ascribed to two factors: either the artificial production of organic compounds was not a central issue; or oxalic acid was not an organic compound; and both these explanations contravene one horn of the popular conception of a prevalent pessimism and an unequivocal criterion for organic status.

There were chemists who did not regard oxalic acid as "organic",<sup>59</sup> but their reasons for so doing generally derived from their having adopted another demarcation criterion, whereby no compound could be considered "organic" if it contained only two elements and could be represented analogously to the binary inorganic compounds.<sup>60</sup> But was this criterion universally accredited? Again the answer is "no", for even Berzelius incessantly paraded oxalic acid as an exemplary organic compound,<sup>61</sup> and Mitscherlich was bold enough to include ammonia and carbon dioxide in his organic category.<sup>62</sup>

Without delving further into a complex problem it is clear that the basic presupposition of the traditional account—that the concept of an organic compound was non-problematic—is false. We catch a glimpse of the real situation when we observe the enthusiasm with which Liebig's definition of organic chemistry as the chemistry of compound radicals<sup>63</sup> could be received.

<sup>57</sup> Dumas, *J. de Pharmacie*, 20 (1834), p. 267.

<sup>58</sup> Partington, *op. cit.*, p. 325.

<sup>59</sup> Cf. Gay-Lussac, *Cours de Chimie*, Paris, 1828, vol. II, Lesson 24.

<sup>60</sup> Oxalic acid was currently represented as "C<sup>2</sup>O<sup>3</sup>" and appeared to be intermediate between carbon monoxide CO, and carbonic acid CO<sub>2</sub>.

<sup>61</sup> *Annals of Philosophy*, vol. II, 1813, pp. 447-50; IV, 1814, pp. 328 and 329. *Lehrbuch der Chemie*, Dresden, 1827, III, p. 143; *Traité de Chimie*, V (1831), p. 9.

Consequently, in order to explain away the negligible impact of the oxalic acid synthesis, it is not enough to say, as does Benfey (*op. cit.*, p. 15), that "it is likely that oxalic acid, being an acid was considered to fit easily in Berzelius' dualistic scheme. Since it combines with bases to form salts, it reacts similarly to sulphuric acid". Surely this could have been said of all the organic acids; and, moreover, urea itself was no less significant as a base than oxalic acid was as an acid. Urea was known to form crystallizable products with nitric and oxalic acids.

<sup>62</sup> Mitscherlich, *Elémens de Chimie*, Brussels, 1835, 1836, vol. II, p. 3.

<sup>63</sup> Liebig, *Traité de Chimie Organique*, Paris, 1840-1844, vol. I, p. 1.

The author of one review declared that this was the first definition ever proffered: "consult the writings of every author and you will see that everyone will give you only a description of the bodies with which the science of organic chemistry is concerned, but what the science itself is has never been defined".<sup>64</sup>

It may always be argued that Wöhler's urea did contribute to the dismantling of the possibility-of-inorganic-synthesis-alone as a demarcation criterion. This is of course a different question from any related directly to vitalist belief; and yet, even here, there are dangers, for it seems likely that the artificial production of compounds, such as oxamide, which displayed marked analogies with those of the organic compartment, would be just as disruptive of a criterion based on synthesis, as the artificial production of urea.<sup>65</sup> How could one be sure that such an artificially prepared compound was *not* a component of some hitherto unexamined organism?<sup>66</sup>

(D) *The strong and weak senses of "as Nature does"*

Our fourth ambiguity resides in the phrase "as Nature does", for an X-statement like that of Berzelius in 1831—to the effect that the inorganic elements cannot be artificially combined "after the manner of living Nature"<sup>67</sup>—is susceptible of two very different interpretations. If we were to admit that "we cannot prepare urea as Nature does", we might simply be abbreviating a juxtaposition of the two statements, "we cannot produce urea", and "Nature can produce urea". Alternatively we might be saying that, although we can produce urea artificially, we cannot do so by precisely the same processes as Nature. The implications of this distinction are considerable, since it may be said of Wöhler that he undoubtedly prepared urea artificially, but undoubtedly by a method not in the least resembling that employed by an animal. There is a real sense in which Wöhler's discovery was logically irrelevant to a vitalism debate, because it simply did not pronounce on the question of whether a vital force was an ancillary agent in the synthesis *as accomplished by an organism*.<sup>68</sup>

<sup>64</sup> Review of Liebig's "Traité" in *Revue Scientifique et industrielle de Quesneville*, I (1840), p. 523.

<sup>65</sup> Cf. Dumas, "Sur l'oxamide, matière qui se rapproche de quelques substances animales", *Ann. Chim.*, 44 (1830), p. 129, and especially p. 143.

Cf. Liebig, "Sur quelques combinaisons d'Azote", *Ann. Chim.*, 56 (1834), p. 5, and especially p. 23.

<sup>66</sup> Liebig, for example, could draw attention to the fact that the maleic acid, discovered by Pelouze, had not *as yet* been found in nature. (*Traité*, vol. II, p. 118.)

<sup>67</sup> Berzelius, *Traité de Chimie*, V (1831), pp. 11 and 12.

<sup>68</sup> This is why Liebig did not regard organic syntheses as fatal to vital force: having noted the chemists' ability to secure formic acid, oxalic acid, and urea in the laboratory, he merely concluded that the vital force "*shares*" many properties with chemical forces. (*Traité*, 1840, vol. I, p. xciii.)

Only if Wöhler had procured urea under mild conditions—conditions which were potentially those appertaining to the organisms—only then could it have pronounced on the question of whether an auxiliary vital agent was dispensable. As it was, drastic conditions were demanded for the synthesis of the cyanate.

It may seem that this is a spurious distinction, and yet there are numerous clues in the literature which suggest that for all but the most naïve vitalist, Wöhler's experiment could be dismissed as logically irrelevant. Many years later, Claude Bernard, who had emphatically repudiated his vitalist heritage,<sup>69</sup> could still insist that although the chemist had acquired the skill to generate organic compounds in his laboratory he could not imitate the methods of Nature herself.<sup>70</sup> Such a state of affairs could surely, even then, have granted refuge to any timorous vitalist. Another later writer acknowledged that "All our artificial syntheses can only be achieved by the application of forces and agents which can never play a part in vital processes, such as extreme pressure, high temperature, concentrated mineral acids, free chlorine—factors which are immediately fatal to the living cell. . . . It follows that the animal body has command of ways and means of a totally different character, by which the same object is gained".<sup>71</sup> And if a plant or animal did command ways and means of so "totally different" a character, it would not have seemed, to an earlier generation, a logical impossibility that irreducible vital forces or agents were deployed "in vivo". It is not a ridiculous suggestion to say that far from driving a first or last nail into the coffin of vital force, Wöhler's experiment equipped it with a new lease of life. As Gerhardt declared, in 1843, "one could not begin to compare" the violent processes of the chemist with the tranquil and delicate operations of Nature.<sup>72</sup> This is the context for Liebig's not often quoted remark that a "a rational physiology cannot be founded on mere reactions",<sup>73</sup> and there is absolutely no doubt that the transformation of certain

<sup>69</sup> C. Bernard, *An Introduction to the Study of Experimental Medicine*, transl. H. C. Greene, Dover facsimile (New York, 1957), p. 62.

<sup>70</sup> "le chimiste, qui peut refaire, dans son laboratoire les produits de la nature vivante, ne saurait jamais imiter ses procédés. . .".

Quoted by J. T. Merz, *A History of European Thought in the 19th Century*, vol. II (Dover, New York, 1965), p. 426.

<sup>71</sup> G. Bunge, *Physiological Chemistry*, transl. Wooldridge, p. 313. Quoted by Merz, *op. cit.*, II, p. 426.

<sup>72</sup> Gerhardt, *Revue Scientifique et Industrielle de Quesneville*, XIV (1843), p. 582f.

Cf. Liebig, *Traité* (1840), I, p. CXXX.

<sup>73</sup> Liebig, *Animal Chemistry or Organic Chemistry in its Applications to Physiology and Pathology* (1842), Johnson Reprint ed. (1964), Liebig's Preface, p. xxix.

organic substances into others, under mild conditions, was far more influential in the exorcizing of vital forces than any drastic synthesis.<sup>74</sup>

(E) *Ambiguous "bodies"*

One final ambiguity must be reckoned with if Wöhler's synthesis is to be seen in its true perspective and this stems from the awkward word "body". The French word "corps" is particularly mischievous for it was used to denote both a compound and an organism. Sometimes it is clear as to which sense is intended: when Berzelius stated that "tout corps organique diffère" from "un corps inorganique",<sup>75</sup> his organic "corps" was obviously an organism because he proceeded to refer to its development. But this is not always the case, and nor is the problem confined to French treatises. For William Prout used the word "body" indiscriminately in the context of an organism<sup>76</sup> and a compound.<sup>77</sup> Similarly, Müller was able to distinguish eight senses in which organic "bodies" differed from those of the inorganic domain, but of these eight, only three were applicable to compounds.<sup>78</sup> It might be argued that the refusal of these men and their colleagues to clarify the distinction between an organic compound and an organism has implications for any appraisal of what they did understand by an organic compound, and yet they *did* make distinctions—more in fact than we might be tempted to do.

One such was the distinction between an "organic" and an "organized" body, and this did not coincide with that between a compound and an organism. Thus gelatin, fibrin and albumen would be classed as "organized" bodies, while a mundane vegetable acid would be merely "organic". This is an issue which

<sup>74</sup> Cf. Pelouze and Gelis, "Mémoire sur la production artificielle de l'acide butyrique", *Revue Scientifique et industrielle de Quesneville*, vol. 13 (1843), p. 437.

Cf. P. M. Roget, *Treatises on Physiology and Phrenology*, Edin., 1838, vol. I, p. 300; vol. II, p. 60.

Roget was far more concerned with the apparent interconvertibility of gelatin, fibrin and albumen, by means of "slight causes", than with Wöhler's synthesis. In fact, Wöhler does not even receive so much as a mention in Roget's discussion of urea (*Ibid.*, vol. II, pp. 49, 50).

<sup>75</sup> Berzelius, *Traité de Chimie*, 1831, V, p. 2.

<sup>76</sup> Prout, *op. cit.*, (1834), p. 418, under the title: "Of the Nature and Composition of Organized Bodies in general, as compared with inorganic matter".

<sup>77</sup> *Ibid.*, pp. 485 and 422f. where starch, sugar and their relatives are called organized "bodies".

<sup>78</sup> J. Müller, *op. cit.*, p. 2ff. Organic bodies differed from their inorganic counterparts, in that they were all compounded of so few elements, in that they did not manifest multiple proportions, and in that they could not be formulated after the binary fashion of the inorganic world. Müller's remaining discrepancies referred to properties such as generation, adaptability, life and death—which could not be predicated of a compound.

requires further investigation, for although the distinction was rarely rigorous,<sup>79</sup> it did have significance in the context of synthesis. Hofmann, for example, translated the conclusion from Liebig and Wöhler's joint paper on uric acid, in the following way: "From these researches the philosophy of chemistry must draw the conclusion that the synthesis of all organic compounds which are not organized must be looked upon not merely as probable, but as certain of ultimate achievement."<sup>80</sup>

Consequently when a chemist of the 1820s or '30s utters an X-statement, he may only be expressing scepticism about the artificial production of leeches, livers or lignin—a scepticism which Wöhler's urea could do little to alleviate.

## II. VESTIGIAL VITALISMS?

It is, in fact, the common failure to distinguish two separate issues, namely the peculiarity of a compound and the peculiarity of an organism, which is largely responsible for perpetuating the popular appraisal of Wöhler's work. Traditional accounts of the synthesis have maintained that the difference between the living and inanimate realms was considered equivalent to the difference between inorganic and organic compounds. The two distinct questions, (a) Is there something peculiar about a living organism? and (b) Is there something peculiar about an organic compound, namely that it defies artificial fabrication?, are taken to have been essentially equivalent. In this spirit a recent author can still say that "the uniqueness of life was thought to be a corollary of the uniqueness of organic matter",<sup>81</sup> from which the familiar thesis follows that, in 1828, "the barrier fell", and the concept of a life-force became superfluous.<sup>82</sup> The aim of this section of the paper is therefore to challenge the opinion typified by the same author's remark that "life became explicable in purely physical terms as soon as chemists synthesized the first organic compound",<sup>83</sup> and thereby to emphasize that Wöhler's urea was no more than a minute pebble obstructing a veritable stream of vitalist thought.

It has often been said that the supreme service rendered to physiology by Descartes consisted in his defining the right kind of questions to be asked. And as long as man, rather than Nature, answered his own questions, even he himself could be portrayed after the fashion of La Mettrie's mobile mushroom.

<sup>79</sup> Cf. Dumas' discussion of the "transition" between the chemistry of organic and of organized substances, *Compte Rendus, Ac. des Scs.*, VI (1838), p. 692.

<sup>80</sup> Liebig and Wöhler, *Ann. Pharm.*, **26** (1838), p. 242, translated by Hofmann in his 1875 Faraday Lecture, *The Life work of Liebig*, London, 1876, p. 89.

<sup>81</sup> R. Schubert-Soldern, *Mechanism and Vitalism*, Univ. of Notre Dame Press, Indiana, 1962, p. 24.

<sup>82</sup> *Ibid.*, p. 25.

<sup>83</sup> *Ibid.*, p. 28.

It is not to denigrate the efforts of eighteenth-century physiologists to say that there were features of the most conspicuous physiological processes—of nutrition, secretion, absorption, generation and regeneration—which eluded their mechanical models. To repeat the customary and pejorative epithets attached to all the vital agents that were invoked to supplement or supersede the available models, would be to sacrifice all sympathy. Indeed, within a Newtonian tradition, it is only to be expected that there should be recourse to an additional vital force,<sup>84</sup> having some analogy with gravitational force;<sup>85</sup> and there would be those who could say of it what Berzelius was to say of his addition to the repertoire of forces—catalytic force—“I do not believe that it is a force entirely independent of the electrochemical affinities of matter; I believe, on the contrary, that it is only a new manifestation, but since we cannot see their connection and mutual dependence, it will be easier to designate it by a separate name.”<sup>86</sup> The point that must be emphasized is that whatever “vital force” did specify—and the term was often used purely descriptively—it was not invoked for the sole purpose of rationalizing the apparent incompetence of the chemists. Far more positively, vital forces were called upon to deal with highly selective and teleological aspects of the behaviour of an organism. It remains to demonstrate that Wöhler's discovery, *qua* synthesis, made no significant contribution to the refinement of those crude mechanico-chemical models whose inadequacies were such as to encourage the retention of vital forces long after 1828. As Everett Mendelsohn has indicated, a comprehensive exposition would demand a detailed treatment of the improvement of those models throughout the first half of the nineteenth century, and of the actual rôles assigned to the vital forces.<sup>87</sup> Even a superficial glance will show, however, that serious physiological problems were no nearer solution in 1829 than in 1827.

As the first decade of the nineteenth century closed, Magendie had to admit that since no analogy had as yet been discovered between the ordinary interplay

<sup>84</sup> Cf. D. Shapere, “Plausibility and Justification in the Development of Science”, *J. of Philosophy*, LXIII, no. 20, p. 615.

Newton himself had envisaged and sanctioned the existence of forces in Nature, as yet unknown to him. (*Opticks*, Dover ed., 1952, p. 376.)

<sup>85</sup> Cf. J. F. Blumenbach, who by the term “*nisus formativus*” wished to designate “not so much the cause [of development] as some kind of perpetual and invariably consistent effect. . . . Exactly likewise we use the name of ‘attraction’ or ‘gravity’ to refer to certain powers whose *causes* nevertheless still remain hidden in Cimmerian darkness”.

Quoted by J. Goodfield and S. Toulmin, *The Architecture of Matter* (Pelican, 1965), p. 368.

<sup>86</sup> J. Berzelius, *Ann. Chim.*, 61 (1836), p. 146f., transl. in Leicester and Klickstein, *Source Book in Chemistry*, New York, 1952, p. 267.

<sup>87</sup> “Physical Models and Physiological Concepts: Explanation in 19th c. Biology,” *Brit. J. Hist. Sc.*, vol. 2, no. 7, 1965, p. 203.

of chemical affinities and the processes of nutrition, the latter must be understood to depend on a special vital force,<sup>88</sup> and this admission is particularly significant since Magendie was—after all—endeavouring to rivet a determinist backbone into the “jellyfish” physiology of Bichat. How was the assemblage of chewed-up food converted into a homogeneous fluid? How was the sustaining material from the food distributed to the appropriate parts of the body? That Wöhler’s urea offered no assistance in explaining these phenomena is clear from subsequent discussions by Liebig and Müller, both of whom resorted to vital forces to account for their highly selective features.<sup>89</sup>

Similarly the high degree of specificity associated with the phenomena of absorption and secretion remained inexplicable, even despite the fact that the chemical phenomenon of selective osmosis had provided an ostensibly helpful analogy.<sup>90</sup> So too did the specificity of catalytic action during the 1830s,<sup>91</sup> which is more than can be said for any organic synthesis, but when, in 1847, William Whewell answered his own rhetorical question, as to “what are the powers by which chyle is *absorbed* from the food, by which bile is *secreted* from the blood, by which the circulating *motion* of these and all other fluids of the body are constantly maintained,”<sup>92</sup> he was obliged to say that no *satisfactory* chemical solution had hitherto been returned. As Davy had earlier retorted, the analogies were “too remote and incorrect”.<sup>93</sup>

The phenomena of muscular action “are . . . among the most mysterious processes connected with the physical functions of life; and if they do not furnish a pretence for calling in the aid of an hypothetical agent, it is . . . not

<sup>88</sup> J. M. Olmsted, *François Magendie*, New York, 1944, p. 28. And he continued to distinguish between “physical” and “vital” phenomena occurring within the organism. (*Leçons sur les phénomènes physiques de la vie*, Paris, 1839, vol. I, p. 15; vol. II, pp. 15 and 65.)

<sup>89</sup> Liebig, *Animal Chemistry* (*op. cit.*), 1842, p. 199f.

Müller: “Each elementary particle of an organ attracts similar particles from the blood, and by the changes it produces in them, causes them to participate in the vital principles of the organ itself”. (Quoted by W. Whewell, *Philosophy of the Inductive Sciences*, 2nd ed., London, 1847, vol. 1, Bk. 9, p. 588f.)

<sup>90</sup> Cf. Magendie (*Leçons sur les phénomènes physiques de la vie*, Paris, 1839, I, p. 93) who reported that it was first thought that the discovery of osmosis would change the face of physiology—and this would never have been said of Wöhler’s urea—“mais jusqu’ici on ne voit point que ces espérances se soient réalisées”.

<sup>91</sup> Cf. Berzelius: it is perhaps “by an action analogous to catalytic force that there occurs secretion of very different bodies which are, however, all drawn from one material: sap in plants and blood in animals” (1836). (Cited in Leicester and Klickstein, *op. cit.*, p. 268.)

<sup>92</sup> Whewell, *op. cit.*, 1847, vol. I, Bk. 9, p. 590.

<sup>93</sup> H. Davy, *Consolations in Travel*, London, 1830, p. 202.

on account of any satisfactory results obtained in the ordinary methods of inquiry".<sup>94</sup> Despite his methodological critique of vital principles and agents<sup>95</sup> Prichard, the author of these words, conceded that there remained the great problem of explaining how the cohesion of the muscular fibres was augmented, on contraction, to enable the muscle to exert a force appropriate to the lifting of large weights.<sup>96</sup> Here again chemical analogies were available but seriously deficient. Cuvier, for example, had compared the prodigious force, instantaneously required of a muscle, with the expansive force generated by the instantaneous freezing of water,<sup>97</sup> and yet it was generally agreed that an analogous chemical change so suddenly actuated by the will, was as difficult to conceive as a directly initiated increase in mechanical power.<sup>98</sup> Inasmuch as the artificial synthesis of an organic compound offered no analogy whatever with muscular phenomena, so would Wöhler's urea be innocuous to any vital force employed to deal with those phenomena.

Again, the synthesis of an organic compound said precisely nothing about the development of an organism—another problem which was long to resist chemical comprehension.<sup>99</sup> As Müller argued, even if a cluster of crystals could be regarded as an individual, "there is still this great distinction between them and organized bodies—that the molecules of crystals are homogeneous throughout . . . whilst organized bodies are composed of perfectly different members of one whole. . ." <sup>100</sup> And if the generation of an organism inspired a reverence for superior vital forces,<sup>101</sup> so too did the regeneration of wounded parts almost imply the need for a volitional force. To the pertinent question as to how the body knew when to deposit more calcium phosphate, or when to marshal its healing forces, one might well answer either by supposing God to have supplied it with a volitional vital force, or one might, with greater

<sup>94</sup> J. C. Prichard, *A Review of the Doctrine of a Vital Principle*, London, 1829, p. 124.

<sup>95</sup> Cf. *Ibid.*, p. 128: The doctrine of a vital principle "is applied here as in other cases. like a weapon ready to cut every knot, but capable of untying none".

<sup>96</sup> *Ibid.*, p. 125.

<sup>97</sup> *Ibid.*, pp. 126 and 127.

<sup>98</sup> Whewell, *op. cit.*, vol. I, p. 610.

<sup>99</sup> Cf. Claude Bernard, who, for all his critique of the vitalists, still had to posit a "developmental force" in the egg. (J. Goodfield, *The Growth of Scientific Physiology*, London, 1960, p. 148f.)

And for a recent view: "The behaviour of cells towards one another at this time when they are synthesizing tissues and organs is full of imponderables" (Bryn M. Jones in *Science Journal*, Feb., 1967, p. 73).

<sup>100</sup> Müller, *op. cit.*, p. 20.

<sup>101</sup> Prout, *op. cit.*, p. 433.



theological sophistication,<sup>102</sup> suppose that God had so wisely adapted the whole organism for survival, that a non-volitional force would suffice. In either case a vital force of some kind would have seemed physiologically indispensable,<sup>103</sup> until—not the synthesis of an organic compound, but—until some plausible model for a self regulatory mechanism could be advanced.<sup>104</sup>

Post-1828 vital forces were not at all vestigial. Wöhler himself continued to recommend them,<sup>105</sup> and the genuine disjunction of the organism/compound questions is nowhere better expressed than by Liebig, in the fourth edition of his *Chemical Letters*: “If any one assured us that the palace of the king, with its entire internal arrangement of statues and pictures, started into existence by an accidental effort of a natural force, which caused the elements to group themselves into the form of a house—*because the mortar of the building is a chemical compound of carbonic acid and lime, which any novice in chemistry can prepare*—we should meet such an assertion with a smile of contempt.”<sup>106</sup>

### III. A VERDICT FROM THE CHEMISTS

In the paper from which the world first learned of his discovery, Wöhler concluded, not by announcing the death sentence of vital forces, but with the structural implications of his reaction, for his urea and the less stable ammonium cyanate had just furnished an outstanding example of isomerism; i.e. they shared a common empirical formula, and were yet two distinct compounds. “I refrain”, Wöhler wrote, “from all the considerations which so naturally offer themselves, particularly those bearing upon the composition relations of organic substances, upon the like elementary and quantitative composition of

<sup>102</sup> Not only did the universality of *intelligent* principles savour of pantheism and represent a scheme “too visionary for the most imaginative” (Prichard, *op. cit.*, p. 140), it was also dispensable *without injury to natural theology*, for all the latter required was superlative machinery. (Prout, *op. cit.*, p. 545; Liebig, *Animal Chemistry, op. cit.*, pp. 248, 218 and 219.)

<sup>103</sup> It is interesting that Schwann characterized the vitalist debates by saying that the crux of the problem was whether “that which arranges and combines the molecules is a power acting with a definite purpose” or whether the “action goes on . . . quite blindly”. (*Mikroskopische Untersuchungen über die Wachstum der Tiere und der Pflanzen*, Berlin, 1839, transl. H. Smith in *Classics of Modern Science*, ed. W. S. Knickerbocker, (Boston, 1962) p. 245.)

<sup>104</sup> Cf. Liebig’s steam-engine analogy (*Animal Chemistry*, p. 250). Later utilized by Bernard (*Introduction to the study of experimental medicine, op. cit.*, p. 79).

<sup>105</sup> Cf. the comment of Du-Bois Reymond, cited by Merz, *op. cit.*, I, p. 218. And cf. W. B. Carpenter, “On the mutual relations of the vital and physical forces”, *Phil. Trans. Roy. Soc.*, **140** (1850), p. 728.

<sup>106</sup> Quoted by Hofmann, Faraday Lecture, *op. cit.*, p. 137. [Italics mine.] It is argued that this same disjunction is germane to an understanding of “vitalism and reductionism in Liebig’s physiological thought” by T. O. Lipman, *Isis*, **58** (1967), p. 167f.

compounds of very different properties."<sup>107</sup> Indeed this very reluctance to speculate along the isomerism line only confirms the perplexing nature of this new phenomenon, any instances of which would be greeted with enthusiasm. Other commentators<sup>108</sup> have noted that Wöhler's experiment did disclose a conspicuous example of isomerism, but the significance of this for an appreciation of the general reception of the urea synthesis has not yet been sufficiently emphasized. Nor has it been recognized that the isomeric transformation carried fundamental implications for yet another problem vexing chemists in 1828—namely how the arrangement of atoms within a compound could be inferred from a knowledge of the empirical formula and the reactions of the compound. These genuine "structural" implications are reflected in discussions of Wöhler's work during the 1830s—discussions which in their allusions to "singular" and "remarkable" results are liable to mislead the unwary into thinking that the "singularity" consisted in an astonishing threat to vitalism.

While congratulating Wöhler on his "important and beautiful" findings, the effect of Berzelius' response to the urea preparation has nevertheless been accurately described as if Berzelius were advising Wöhler, "Yes, the preparation of urea is very interesting but do not waste too much time with it at the expense of accomplishing something more important".<sup>109</sup> Berzelius preserved absolute silence on vitalistic implications, but when, later, it came to the question of structural inferences, Berzelius was vociferous: Wöhler's transformation was, he surmised, "a highly remarkable change", since urea, while possessing the same composition as a salt, was not a salt. In short, it showed that "compounds with the same relative number of atoms of the elements, can be different by reason of the different way in which the simple atoms are placed relative to one another".<sup>110</sup>

Again, those who would have Wöhler deliver a death-blow to vitalism overnight<sup>111</sup> invariably appeal to Liebig's appraisal of 1837: "The extraordinary and to some extent inexplicable production of urea without the assistance of vital functions . . . must be considered one of the discoveries with which a new era in science has commenced."<sup>112</sup> However, closer scrutiny of Liebig's report only confirms that what is "extraordinary" and "inexplicable" is not the fact that urea has been artificially produced, but is rather the curious character of the transformation reaction: ammonia and cyanic acid, Liebig continues, "when

<sup>107</sup> Wöhler, *Pogg, Ann.*, **12**, p. 256, transl. in Leicester and Klickstein, *Source Book*, *op. cit.*, pp. 311-2.

<sup>108</sup> T. O. Lipman, *op. cit.* (note 3) and O. T. Benfey, *op. cit.*

<sup>109</sup> T. O. Lipman, *op. cit.* (note 3), p. 454.

<sup>110</sup> Berzelius, *Traité de Chimie*, VII (1833), pp. 379 and 380.

<sup>111</sup> Cf. E. Campaigne, *J. Chem. Educ.*, **32**, (1955), p. 403.

<sup>112</sup> Liebig, Report to the British Association, *op. cit.*, p. 38f.

first combined, form cyanate of ammonia, a salt analogous to every other ammonia salt . . . but . . . a few minutes after the combination has taken place, all these properties disappear. We can no longer detect either ammonia or cyanic acid; a new substance has been formed."<sup>113</sup> It is the isomeric change, the fascinating conversion of one isomer into the other that Liebig finds so exciting.<sup>114</sup>

Of course, we have to ask why the phenomenon of isomerism should be so alluring; but the answer is a simple one. During the period prior to 1828 it had been generally assumed that the proportions of the elements in a given compound made a dominant contribution to its properties,<sup>115</sup> and the very possibility of the existence of two distinct compounds, sharing the same empirical formula, was often denied.<sup>116</sup> A particularly compelling example is contained in a paper devoted to urea, and published by Vauquelin in 1824. Having described the spontaneous "decomposition" of a urea solution into a "sub-carbonate" of ammonia, the author admitted that it seemed to be the case that the elements of urea were reunited in exactly the same proportions as in the ammonia salt. But, he proceeds to state, without any further comment, that this is "impossible".<sup>117</sup> In other words Vauquelin could not entertain the idea that two different compounds, urea and a salt of ammonia, could both enjoy the same empirical formula. To those, like Vauquelin, for whom the existence of two isomers was a conceptual impossibility, but for whom, in all probability, the artificial production of an organic compound was not a conceptual impossibility Wöhler's transformation would constitute contrary and convincing evidence.<sup>118</sup>

<sup>113</sup> *Ibid.*, p. 39.

<sup>114</sup> As far as the concepts of organic chemistry were concerned, Liebig could elsewhere state that the *preparation* of cyanogen by Gay-Lussac (and not Wöhler's *preparation* of urea), "a été la plus féconde de toutes celles qui ont été faites en chimie organique". (*Traité*, vol. I (1840), p. 123.)

<sup>115</sup> Cf. the laws elaborated by Thenard in his *Traité de Chimie*, 4th ed., Paris, 1824, III, p. 560f.

<sup>116</sup> Cf. Berzelius to Marcet 23rd Sept., 1817: "il est clair qu'il doit y avoir une différence déterminée entre la composition de ces deux substances [sugar and gum], puisqu'elles diffèrent tant dans leurs propriétés" (*Bref.*, Bd. I, 3, p. 155).

<sup>117</sup> Vauquelin: "Note sur la décomposition spontanée de l'urée", *Ann. Chim.*, 25 (1824), pp. 423-6, p. 426.

<sup>118</sup> It might be objected that Liebig and Wöhler had already established the isomerism of silver cyanate and silver fulminate in 1824. There is a sense in which this is true, but apart from the fact that these isomers were not interconvertible, the very *resistance* to Liebig and Wöhler's results is yet another measure of the prevalent suspicion of the new phenomenon. Not only did Berzelius remind his contemporaries that "almost every Jahresberichte since [1824] reported an attempt to explain these results through a previously unnoticed dissimilarity in their composition . . ." (Jahresberichte, 1832, transl. by Benfey, *op. cit.*, pp. 27 and 28); but Liebig himself proceeded to distinguish between the composition of cyanic and fulminic acid. (*Traité*, I (1840), p. 124.)

In fact, the phenomenon of isomerism was to be a source of consternation and speculation throughout the next two decades: Berzelius insisted that the authenticity of the phenomenon could be accepted only after "severe" examination,<sup>119</sup> and at least five theories<sup>120</sup> were in circulation as to how two compounds could boast identical proportions of their elements, and yet display divergent properties.<sup>121</sup>

Wöhler's experiment, then, had an interest quite independently of any relevance *qua* synthesis, and this interest has not yet been exhausted, for the structural aspects of his work had implications which transcended even the establishment of isomerism as a phenomenon to be reckoned with. For these deeper structural implications we must inspect a paper published by Dumas in 1830—a paper which commences with the words: "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."<sup>122</sup> Dazzled by this prelude, elated commentators have ventured no further into Dumas' discussion.<sup>123</sup> Had they done so they would have realized that these opening sentences constitute a calculated concession, for Dumas proceeds to emphasise that Wöhler's principle only "*seemed* to give the key" when he informs us that he is about to offer a few

<sup>119</sup> Berzelius, *Traité* VI (1832) p. 710.

<sup>120</sup> Cf. Gay-Lussac, *Cours de Chimie*, Paris, 1828, vol. II, Lesson 32. Berzelius, "Essay on the allotropy of simple bodies . . .", Taylor's *Scientific Memoirs*, IV, p. 240. Prout, *op. cit.* (1834), p. 426f. Mitscherlich, *op. cit.* (1835-36), II, pp. 6 and 7.

<sup>121</sup> At this point the story has an almost ironic twist. Whereas McKie has proclaimed the insignificance of Wöhler's discovery on the grounds that his synthesis was a mere transformation, there is an important sense in which, even for the physiologists, it was the isomeric transformation which was significant. It has already been argued that Wöhler's synthesis was effectively complete, but that *qua* synthesis it was logically irrelevant to physiological issues because of the drastic conditions employed. In its sporting an example of isomerism, however, it may have caught the physiologist's eye, for here was a novel chemical variable with which to play. Granted the current underestimation of the complexity of the spatial variables underlying the phenomena of isomerism, it was natural to remark, as did the English physiologist Bostock (*cf.* Prichard, *op. cit.* p. 102) that the existence of isomers rendered it probable that a very minute variation in the chemical affinities of their elements might have originally produced the one or the other of the isomers, or subsequently interconverted them. That two compounds could be so diverse on the phenomenological level, and yet in reality differ only very minutely in their construction, definitely encouraged the conviction that chemical variables might ultimately be able to cope with the diversity of physiological phenomena. William Gregory was later to state that the principle of isomeric and polymeric transformation was constantly in action in living organisms, and needless to say he quoted the urea synthesis to illustrate isomerism. (*Elementary Treatise on Chemistry*, Edin., 1855, pp. 263 and 264.)

<sup>122</sup> Dumas, "Note sur la composition de l'urée," *Ann. Chim.*, **44**, (1830), p. 273.

<sup>123</sup> Partington, *op. cit.*, Campaigne, *op. cit.*, Benfey, *op. cit.*

facts which will demand a reconsideration of Wöhler's achievement.<sup>124</sup> To what door, then, did Wöhler's result seem to give the key? The remainder of Dumas' paper precludes the reply "that of the artificial production of organic compounds", since there is no subsequent reference to artificial synthesis. In fact the "principle" which Wöhler's work *had* illuminated was one appropriate to solving a major problem of the day—namely how to ascertain the arrangement of elements within an organic compound, once its empirical formula was fixed.<sup>125</sup> If urea could be prepared from ammonia and cyanic acid, it was, as Dumas maintained, "natural" to infer that urea was nothing else than ammonium cyanate, in which the ammonia and cyanic acid existed as preformed parts,<sup>126</sup> and this conclusion then permitted a formula for urea to be posited which represented the arrangement of the elements accordingly.<sup>127</sup> Now the new facts that Dumas announces—those he believes speak for a re-evaluation of Wöhler's work—are not facts which somehow cast doubt on the integrity of the synthesis; rather, they present a challenge to the idea that Wöhler's preparation had given a clue as to how the arrangement of elements could be inferred. The burden of Dumas' paper is that a formula for urea, derived by analogy with that of oxamide—a compound which behaved analogously to urea—came into conflict with that postulating preformed cyanic acid and ammonia.<sup>128</sup>

Nor was the oxamide analogy the only factor to jeopardize the preformed groups. It might be in order to infer the pre-existence of ammonia and cyanic acid within ammonium cyanate, but surely the transformation of the latter into urea implied a scrambling of the elements? Now it is precisely arguments of this kind which characterize discussions of urea during the 1830s, and so in addition to exemplifying isomerism, and quite apart from vitalist reference, Wöhler's urea was so widely discussed because it raised in an acute form the question of preformed groups within organic compounds. To put it crudely: chemists could inject ammonia and cyanic acid into urea, but could not drag them out again; and the precise implications of this for a structural programme

<sup>124</sup> "Ce n'est donc qu'à regret, en quelque sorte, que je viens offrir aujourd'hui quelques faits de nature à changer le point de vue qui a si heureusement dirigé M. Wöhler." (*Ann. Chim.*, **44** (1830), p. 273.)

<sup>125</sup> That this *was* the major problem is obvious from Dumas' earlier discussion of oxamide. (*Ann. Chim.*, **44**, (1830), p. 142f.)

<sup>126</sup> Wöhler himself had said that "ammonium cyanate is urea" (letter to Berzelius 22nd Feb., 1828, *op. cit.*), and Gay-Lussac too had written, "Ainsi l'urée n'est que du cyanate d'ammoniaque avec un atome d'eau; elle doit donc être considérée comme un véritable sel" (*op. cit.*, 1828, II, Lesson 33, p. 12f.).

<sup>127</sup> In Dumas' notation:  $[N^2C^1O + N^2H^6 + H^2O]$ .

<sup>128</sup> Since oxamide appeared to be  $[C^4O^2 + H^4N^2]$  Dumas rewrote urea as  $[C^4O^2 + H^4N^2]$   
 $H^4N^2]$

for organic compounds exercised the ingenuity of all chemists.<sup>129</sup> By way of perspective, Mitscherlich, in a five-page disquisition on urea, in his textbook,<sup>130</sup> devoted a mere two lines, *en passant*, to record the fact that urea had been produced from inorganic material. He made no mention of vital force, and the not insubstantial remainder of his account was concerned with the properties of urea, but primarily with the structural implications of the preparation. And by way of consolidating this kind of perspective, it is noteworthy that of the nine secondary accounts of Wöhler's urea which have sponsored it as an epoch-making discovery, eight were composed for the centenary celebration.<sup>131</sup>

### CONCLUSION

From the foregoing analysis, two assertions can confidently be made. Firstly, a failure to explore a whole series of ambiguities, enshrined within the literature of the period, has sustained the view that Wöhler's urea had immediate implications for physiology. Secondly, a failure to investigate just what the chemists were deliberating when they reviewed the synthesis has also allowed this view to survive. When, however, the physiological-significance claim is obliterated, Wöhler's achievement can be seen in its true light. Primarily its fascination lay in its implications for a "structural" programme for organic chemistry—in its spotlighting the novel phenomenon of isomerism, and in its raising the "preformation" question. But it would be a mistake to maintain that for the chemists, Wöhler's preparation, *qua* synthesis, was entirely devoid of significance. The artificial production—be it transformation or synthesis—of any organic compound was of interest in its own right, and Wöhler's urea could not have been less "remarkable" than the formic acid Döbereiner obtained when he abandoned ants for sugar.<sup>132</sup> That Wöhler's success provided some practical incentive to his fellows cannot be denied, for one could at least hope that the action of cyanic acid on a relative of ammonia would yield an organic

<sup>129</sup> Thus Berzelius considered urea in the context of enquiring whether organic compounds could be structured after the binary fashion of the inorganic domain: "on *peut* se représenter l'urée comme composée d'acide cyaneux, d'ammoniaque et d'eau; *mais* on n'y retrouve aucune des propriétés d'un sel ammoniacque. . .". (*Traité*, V (1831), p. 7f.)

Similarly Mitscherlich seized upon the isomeric transformation as a vindication of his idea that organic compounds, like the sugars, incorporated no preformed groups (*op. cit.*, 1835-6, I, p. 162).

<sup>130</sup> Mitscherlich *op. cit.*, 1835-6, I, p. 159f.

<sup>131</sup> See J. R. Partington, *op. cit.*, p. 260, footnote 6.

<sup>132</sup> As early as 1821 Döbereiner had discovered that formic acid could be prepared artificially by the oxidation of sugar or tartaric acid, and discoveries such as this were described as "remarkable" (*cf.* Gay Lussac, *op. cit.*, 1828, II, Lesson 25, p. 6).

relative of urea.<sup>133</sup> There is also a sense in which Wöhler's urea—along with artificially prepared analogues of reputable organic compounds—would deter chemists from taking the impracticability of synthesis as a demarcation criterion for distinguishing organic from inorganic compounds. It was in precisely this context of assessing the convenience of conjectural demarcation criteria, and not in the context of vitalist belief, that Hofmann, many years before his later exaggerated verdict, referred to Wöhler's urea as of "considerable importance."<sup>134</sup> But even he acknowledged the real source of chemical interest when he reported that "the remarkable change which cyanate of ammonia undergoes in all its properties, when passing over into urea, has greatly engaged the attention of chemists. Many speculations as to the mode in which the molecules may be arranged in urea have been brought forward, in order to explain the new character which the compound assumes".<sup>135</sup>

<sup>133</sup> Cf. Laurent's discussion in his *Chemical Method, op. cit.*, 1855, p. 255.

<sup>134</sup> Hofmann, Introductory Lecture, Royal Institution, *Medical Times and Gazette*, vol. 6, 1853, p. 131f; and Lecture IX, *Ibid.*, p. 621.

<sup>135</sup> *Ibid.*, p. 622.