

THE CONTRIBUTION OF THE PHYSIOLOGIST,
WILLIAM BENJAMIN CARPENTER (1813-1885),
TO THE DEVELOPMENT OF THE PRINCIPLES
OF THE CORRELATION OF FORCES
AND THE CONSERVATION OF ENERGY

by

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ABSTRACT

Over a dozen natural philosophers of the first half of the nineteenth century have been discussed by historians of science as possible contributors to the emergence of the doctrine of the conservation of energy. One figure who has not been discussed, yet who was deeply interested in the twin principles of the correlation of forces and the conservation of power and attempted to apply them in his work as a physiologist, was William Benjamin Carpenter (1813–1885), one of the most eminent physiologists in Britain during the mid-century. This paper traces the development of his ideas on force through the first forty years of his career, from the mid 1830s to the 1870s, examining a wide selection of his work. Carpenter's ideas on force were initially tentative; but as experimental evidence accumulated from both the life-sciences and the physical sciences, especially from the work of Justus Liebig, William Robert Grove, and Carlo Matteucci, his ideas became more explicit so that by the 1850s they became the basis for his physiology. By then, power or force (these two words were still often used interchangeably) and their principles of conservation and correlation had become paradigmatic in his physiology, just as they had become key concepts for certain natural philosophers in the physical sciences. Correlation was always the main principle for Carpenter, even after he had heard of, and was acknowledging the brilliance of, Mayer's work on conservation. In seeing the conservation principle as subordinate to and, in a sense, contained within the correlation doctrine, Carpenter exemplifies the idea now current among historians of science that a so-called "simultaneous discovery" is rarely, if ever, simultaneous. Rather, an apparently simultaneous discovery is usually a coming together of disparate lines of investigation, where the different investigators ask different sets of questions; and it is only in hindsight that they appear to address themselves to a single core-idea or discovery.

DURING THE last two decades several studies have appeared on the emergence of the idea of energy conservation during the early-mid nineteenth century. The seminal

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paper for this genre of studies was T. S. Kuhn's 'Energy conservation as an example of simultaneous discovery',¹ in which he argued that about a dozen natural philosophers scattered throughout Europe were groping, independently of one another, towards a type of energy conservation principle, all within the period c. 1825–1850. Kuhn concluded that by c. 1842 several lines of research and theorizing had become so concatenated that the conservation principle could and should be enunciated.

The term "simultaneous discovery", as Kuhn pointed out, is not an entirely happy one. For instance, the several contributors to the energy conservation principle did not all express the same idea, neither to their contemporaries nor to their successors, so that "only in view of what happened later can we say that all these partial statements even deal with the same aspect of nature."² Nowadays, it is clear that the several contributors discovered different things because they asked different questions. Thus, Julius Robert Mayer (1814–1878) asserted that forces (*Kräfte*) are indestructible. Hermann Helmholtz (1821–1894) proposed that the net quantity of force (*Kraft*, *bewegende Kraft*, *Spannkraft*, *Arbeitskraft* and *Arbeit* being the words with which he designated it) in the world must be constant; and, being a skilful mathematician, he put his proposal in persuasive mathematical form. James Prescott Joule (1818–1889) asserted that heat and the "mechanical powers" are mutually convertible, from which William Thomson (1824–1907) set the dynamical theory of heat on a firm foundation. Finally, Rudolf Clausius (1822–1888), Thomson, and William Rankine (1820–1872) showed all these conclusions to be mutually related, to be different faces of the same dice.

Underlying these researches was at least one easily identifiable common strand, namely a belief in the interconvertibility of the several forces of nature, an idea that had been developed within at least two different schools of European philosophy during the late eighteenth and early nineteenth centuries. It was a dominating idea within Kantianism and *Naturphilosophie*, being expounded, for instance, by Friedrich Schelling (1775–1854) and Hans Christian Oersted (1777–1851).³ It was also conspicuous in British natural philosophy in the late eighteenth and early nineteenth centuries, James Hutton (1726–1795), Adam Walker (1731–1821), and Humphry Davy (1778–1829) being exponents of it.⁴ Within the British tradition the idea was

¹ T. S. Kuhn, 'Energy conservation as an example of simultaneous discovery', in Marshall Clagett (editor), *Critical problems in the history of science*, Madison, University of Wisconsin Press, 1959, pp. 321–356.

² *Ibid.*, p. 322. This point has been reiterated by other scholars studying this field. See, for instance, Yehuda Elkana, *The discovery of the conservation of energy*, London, Hutchinson Educational, 1974, particularly the introduction and appendix.

³ On the relations between Kantianism, *Naturphilosophie* and early nineteenth-century ideas on the unity of forces a fair amount has been written. See, for instance: G. Martin, *Kant's metaphysics and theory of science*, trans. by P. G. Lucas, Manchester University Press, 1961. B. Gower, 'Speculation in physics: the history and practice of *Naturphilosophie*', *Stud. Hist. Phil. Sci.*, 1973, 3: 301–356. L. Pearce Williams, 'Kant, *Naturphilosophie* and scientific method', in R. N. Giere and R. S. Westfall (editors), *Foundations of scientific method: the nineteenth century*, Bloomington, Indiana University Press, 1974, pp. 3–22. Everett Mendelsohn, 'The biological sciences in the nineteenth century', *Hist. Sci.*, 1960, 3: 39–59; and 'Explanation in nineteenth century biology', in R. S. Cohen and M. W. Wartofsky (editors), *Boston studies in the philosophy of science*, vol. 2, New York, Humanities Press, 1965, pp. 127–155.

⁴ Davy's interest in dynamics is discussed in chapter 4 of my unpublished Ph.D. thesis, 'Some contributions of medical theory to the discovery of the conservation of energy principle during the

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developed by several natural philosophers, being given its most extensive and systematic form by William Robert Grove (1811–1896) in his *Correlation of physical forces* in 1846.⁵

Although the correlation of forces was a foundation-stone for the conservation of energy, Grove and his predecessors are not regarded as the key contributors to conservation itself. Yet, as Kuhn and other scholars have argued, a contribution such as his should not be omitted from the overall picture, since what came to be called in the second half of the nineteenth century the conservation of energy (or *die Erhaltung der Energie*) obtained its identity from several types of questions and discoveries which *together* built up the final doctrine.

If such be the case for Grove, there is in my opinion a similar case for the work of the English physiologist, William Benjamin Carpenter. At first sight it might seem surprising that a medical man was deeply interested in rigorous dynamical relations in nature but, as Kuhn's paper showed, this was by no means a province solely for physicists. Three of Kuhn's figures were working in the life-sciences, namely Helmholtz, Mayer, and Justus Liebig (1803–1873). Another figure whom Kuhn discussed in a footnote as a possible contributor, albeit briefly, was the London physician-physiologist, Peter Mark Roget (1779–1869).⁶

Neither Kuhn nor later scholars have examined Carpenter's ideas on force and energy, yet Carpenter was developing them within his work as a physiologist during the 1840s, and in 1850 he declared his interest explicitly and at great length in a paper read to the Royal Society of London 'On the mutual relations of the vital and physical forces'. Some of his contemporaries were well aware of this contribution to the energy field. Balfour Stewart (1828–1887), professor of natural philosophy at the Owen's College, Manchester, in his book on *The conservation of energy, being an elementary treatise on energy and its laws*, acknowledged Carpenter's work thus: "Joule, Carpenter and Mayer were at an early period aware of the restrictions under which animals are placed by the laws of energy, and in virtue of which the power of an animal, as far as energy is concerned, is not creative but only directive."⁷ William Rutherford (1839–1899), professor of physiology at King's College, London (1869–1874) and then in Edinburgh (1874–1899), described Carpenter's work in greater detail: "Much of the present aspect of physiology is owing to Ludwig, who introduced into biological study the graphic method of recording movement invented by Thomas Young; to Carpenter, who applied to physiological phenomena Grove's principle of the correlation of force and so, much about the same time as Mayer and independently of him, paved the way to the application to physiology of Joule's

late eighteenth and early nineteenth centuries', University of London thesis, 1977. Hutton's interest in dynamics is discussed in the same chapter. Hutton is also discussed, along with Walker, in a paper by P. M. Heimann, 'Conversion of forces and the conservation of energy', *Centaurus*, 1973–74, 18: 147–161.

⁵ Grove's role is discussed quite fully by Kuhn, *op. cit.*, note 1 above, and by Heimann, *op. cit.*, note 4 above.

⁶ See Kuhn, *op. cit.*, note 1 above, p. 343. There is a useful biography of Roget with an excellent bibliography, by D. L. Emblem, *Peter Mark Roget. The word and the man*, London, Longman, 1970. Roget's interest in forces is discussed in chapter 6 of my Ph.D. thesis, *op. cit.*, note 4 above.

⁷ Balfour Stewart, *The conservation of energy, being an elementary treatise on energy and its laws*, 2nd ed., London, H. S. King, 1874, p. 166.

and Helmholtz's great principle of the conservation of energy. . . ."⁸ Rutherford's evaluation of Carpenter's contribution to the mid-century development in dynamics was borne out by one of the earliest reviews of his Royal Society paper. In the *British and Foreign Medical Review* of 1851, his paper was called

. . . the first systematic attempt that has been made, in this country at least, to work out the subject; and as it is mainly an expansion of the ideas which had been put forth in our own pages at the beginning of 1848, the author may claim priority . . . both of Dr. Fowler and Dr. Radcliffe, although to a certain degree anticipated by Mr. Newport. We shall presently find, however, that both these gentlemen were anticipated in a quarter they little guessed; and the whole case is obviously one of a kind, of which the history of physiology, as well as of other sciences, furnishes many examples—in which a connecting idea, developed in another department of inquiry, struck many individuals at once . . . and was wrought out by them in different modes, and with different degrees of success, according to their previous habits of thought.⁹

Carpenter himself, though admitting a considerable debt to Grove's correlation theory and that Mayer was indisputably the first to state the conservation principle, claimed that he had made an original and much needed analysis of the relations (a) of vital forces one to another; and (b) of vital to physical forces. Such an analysis he thought essential if the correlation and conservation principles were to be admitted as real laws of nature and not merely as descriptive generalizations.

The principal aim of this paper will be to evaluate Carpenter's claim. But it will be useful, first of all, to give a biographical sketch of the man, for he is not well known even to medical historians and I wish to establish that, in England at least, his reputation was high and his ideas must have reached a wide and significant audience.

BIOGRAPHICAL SKETCH

I am aware of only one comprehensive study of Carpenter, namely *Nature and man. Essays scientific and philosophical*, by William B. Carpenter, edited and introduced with a lengthy memoir by his son, J. Estlin Carpenter.¹⁰ This was first published in 1888 and reprinted in 1970. There have been two recent papers in which Carpenter's work in areas other than force has been discussed; one is by Don Ospovat on 'The influence of Karl Ernst von Baer's Embryology',¹¹ in which the employment of von Baer's law of embryological development by Richard Owen (1804–1892) and by Carpenter is discussed; the other is by P. J. Bowler on 'The changing meaning of evolution',¹² in which it is shown how Carpenter was one of the first to use the word "evolution" in the Darwinian (i.e., non-embryological) sense and that Herbert Spencer obtained the word from him. Ospovat adduces only one piece of evidence

⁸ W. Rutherford, 'Present aspects of physiology', 1874; a lecture cited in J. E. Carpenter, op. cit., note 10 below, p. 54.

⁹ 'Grove, Carpenter, etc., on the correlation of forces, physical and vital', *Br. for. med. Rev.*, 1851, 8: 206–237. This quotation is from pp. 226–227.

¹⁰ J. E. Carpenter (editor), *Nature and man. Essays scientific and philosophical* by William B. Carpenter, London, Kegan Paul, Trench, 1888. Republished by Gregg International Publishers, Farnborough, Hants., 1970.

¹¹ D. Ospovat, 'The influence of Karl Ernst von Baer's Embryology, 1828–1859: a reappraisal in light of Richard Owen's and William B. Carpenter's "Palaeontological application of Von Baer's Law"', *J. Hist. Biol.*, 1976, 9: 1–28.

¹² P. J. Bowler, 'The changing meaning of evolution', *J. Hist. Ideas*, 1975, 36: 95–114.

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for Carpenter's standing in his own time: T. H. Huxley considered his *Principles of general and comparative physiology* to be "by far the best general survey of the whole field of life and of the broad general principles of biology which had been produced up to the time of its publication",¹³ and that it had greatly influenced biology in England. It is curious that Ospovat cites only Huxley's opinion of him, for Huxley had very little first-hand acquaintance with him, whilst other equally eminent and critical men who had experienced Carpenter as a teacher and as a researcher in physiology and anatomy commented in greater detail and with greater praise on his work.

William Carpenter was the eldest son of Dr. Lant Carpenter (1780–1840), one of the most celebrated Unitarian ministers of the first half of the nineteenth century.¹⁴ Lant Carpenter made his reputation particularly as the minister of Lewin's Mead Meeting in Bristol, where he served from 1817 until his death in 1840. In Bristol he and his wife obtained the bulk of their income not from his post as minister, even though the Unitarians there were numerous and often wealthy, but from their private school. All six of the Carpenter children received their early, and in the case of their daughters their entire, education from their father who taught chemistry, physics, and physiology, with simple laboratory apparatus for practical work, and mathematics, in addition to the traditional subjects of Latin, Greek, history, and geography.¹⁵ Indeed, Lant Carpenter's school (which charged £100 per annum for board and tuition) maintained the Nonconformist tradition for providing the best education, and not surprisingly his three sons were to distinguish themselves in their university training. William had intended to be an engineer, but one of his father's protracted periods of illness meant that there would probably be no money for his training. When the family doctor, Dr. Estlin, offered to take him as an apprentice under the regulations of the Society of Apothecaries, William accepted. He was then fifteen and spent the next half-dozen years training under Estlin, teaching chemistry and probably a few other subjects at his father's school, and attending (though we don't know how frequently) the Bristol Medical School, the Infirmary, and the Bristol Literary and Philosophical Institution. In 1834 he moved to London, becoming a licentiate of the Apothecaries Company and a member of the Royal College of Surgeons. In November 1835 he settled in Edinburgh, pursuing his medical studies there until the summer of 1837. In Edinburgh he came under the influence of William Pulteney Alison (1790–1859), who held the chair in Medical Jurisprudence. One of Alison's interests was physiology on which he had published two major works,¹⁶ and it was probably with his encouragement that Carpenter decided not to go into medical practice but to attempt a living in physiology. The most apparent way to do this was with one's pen, and after a few brief attempts at lecturing in

¹³ Cited by J. E. Carpenter, *op. cit.*, note 10 above, p. 67; and by Ospovat, *op. cit.*, note 11 above, p. 10.

¹⁴ The standard biography of Lant Carpenter is by his son, R. L. Carpenter, *Memoirs of the life of the Rev. Lant Carpenter*, Bristol, Philp & Evans, 1842. There is also much information on him and his family in Jo Manton, *Mary Carpenter and the children of the streets*, London, Heinemann Educational, 1976.

¹⁵ Mentioned in *ibid.*, p. 25.

¹⁶ W. P. Alison, *Outlines of physiology*, Edinburgh, Blackwood, 1831; and *Outlines of physiology and pathology*, Edinburgh, Blackwood, 1833.

Bristol he settled down to writing as his main source of income. Already, he had published several essays which were highly regarded,¹⁷ only one of which need detain us here for its relevance to his later work. That was a prize-essay on a subject proposed by Alison, 'On the difference of the laws regulating vital and physical phenomena' (1838); there, he argued that since "the term *law* expresses the *conditions of action* of the properties of matter" and nothing else, then there could, be "nothing essentially different in the *character* of the laws regulating vital and physical phenomena, either as to their comprehensiveness, their uniformity of action or the mode in which they are to be established by the generalization of particular facts".¹⁸ Moreover, since the properties of any piece of matter seemed to depend solely on its molecular constitution, he proposed that "vital properties are not added to matter in the process of organization; but those previously existing and hitherto inactive are called out and developed". As we shall see, this reductionism was to persist in his ideas on life and on forces, albeit with two modifications. First, as his physiology developed he became less confident that the properties peculiar to living organisms could be explained so easily; indeed, during the 1840s he adopted a somewhat vitalistic view, at least with regard to the processes of cell growth and reproduction. Second, his idea of latent properties being called out to manifest themselves, rather as latent heat was thought to manifest itself when the state of a heat-containing body was altered, would be discarded in the light of Grove's severe criticism of the concept of latent properties of matter.

By 1835 William Carpenter was intending to write "an introduction to the philosophical study of natural history", for the treatises hitherto available seemed to him not to appreciate the basic principles of physiology and often to have too narrow a view. Whether this opinion was his own, or whether he was steered in this direction by his general reading, for instance of Schiller and Carlyle, as J. E. Carpenter suggested,¹⁹ his dissatisfaction was deep, for that year he began a treatise on general physiology with the primary aim of discussing vegetable and animal physiology within a common framework. The treatise appeared in 1839 as *Principles of general and comparative physiology*.²⁰ In 1842 it was complemented by his *Principles of human physiology*.²¹ During the next three decades these two books became standard texts in medical education in Britain and her colonies, going through four and nine editions respectively. Sir James Paget (1814–1899), one of the finest surgical pathologists of his day, had this to say about the development of physiology in England:

I believe that among all the events that have had great influence on the teaching of physiology in our medical schools, none has been more important than the institution of separate courses

¹⁷ W. B. Carpenter, 'On the structure and functions of the organs of respiration in the animal and vegetable kingdoms', *West of England Journal*, October 1835 and January 1836, cited in J. E. Carpenter, *op. cit.*, note 10 above. 'On the voluntary and instinctive actions of living beings', *Edinb. med. surg. J.*, 1837, 132: 22–44. 'On unity of function in organized beings', *Edinb. new phil. J.*, July 1837: 92–116. 'On the difference of the laws regulating vital and physical phenomena', *ibid.*, April 1838, 24: 327–353. 'Physiology an inductive science', *Br. for. med. Rev.*, April 1838, 5: 317–342. 'The physiology of the spinal marrow', *ibid.*, April 1838, 5: 486–539.

¹⁸ *Ibid.* Also cited in J. E. Carpenter, *op. cit.*, note 10 above, p. 21.

¹⁹ *Ibid.*, pp. 16–17.

²⁰ W. B. Carpenter, *Principles of general and comparative physiology*, London, J. Churchill, 1839.

²¹ W. B. Carpenter, *Principles of human physiology*, London, J. Churchill, 1842.

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of physiological lectures. . . . among many things proving its necessity, none I think had more influence than the publication of Dr. Carpenter's two principal works in 1839 and 1842. . . . For many years his books were almost without rival in the London schools; Mayo's *Physiology* soon ceased to be read; the translation of Tiedemann and Blumenbach were disused; the translation of Müller's *Physiology* was too large and in some parts too difficult for all but the best students.²²

Paget's opinion was corroborated by many others, for instance by a reviewer of the sixth edition of Carpenter's *Principles of human physiology* (1864): "It is hardly an exaggeration to say that the majority of medical practitioners, scattered at the present time over the surface of England and her colonies, owe all that they owe of physiological science to some one or other of Dr. Carpenter's treatises on the subject."²³

Inevitably, the question arises whether Carpenter's reputation rested solely on his ability to write essays and textbooks, or whether he was recognized for original work. The reviewer mentioned above implied that both were true. However, Carpenter's own researches were not primarily physiological but were concerned with marine biology and comparative anatomy; for instance, it was his work on the *Foraminifera* which caused him to be one of the earliest supporters of Charles Darwin and earned him Darwin's respect.²⁴

Yet his expositions of general physiology were much more than mere compendia of other men's ideas. He constantly sought basic, general principles in physiology, attempting to relate isolated bits of physiological development to physiology at large. Moreover, he considered a few achievements peculiarly his own; these he enumerated in the preface to the third edition of his *Principles of physiology, general and comparative* (1851):

- I. The mutual connection of the Vital Forces, and their relation to the Physical. . . .
- II. The general doctrine that the truly Vital operations of the Animal as well as the Vegetable organism are performed by the agency of untransformed cells. . . .
- III. The organic structure of the shells of Mollusca, Echinodermata and Crustacea.
- IV. The application of von Bär's Law of Development from the General to the Special, to the interpretation of the succession of Organic forms presented in Geological Time.
- V. The Relation between the two methods of Reproduction, that by gemmation and that by sexual union . . . , with the application of this doctrine to the phenomena of the so-called 'Alternation of Generations'. . . .
- VI. The relation between the different methods of Sexual Reproduction in plants.
- VII. The application of the doctrine of reflex action to the Nervous System of Invertebrata, esp. Articulated Animals (Chap. XX); first developed in the author's 'Prize Thesis', published in 1839.
- VIII. The functional relations of the Sensory Ganglia to the Spinal Cord on the one hand, and to the Cerebral Hemispheres on the other.²⁵

The first of these achievements is the principal concern of this paper; but before discussing it I wish merely to mention a few other aspects of his career. Carpenter moved from Bristol to London in 1845; in the preceding year he had been appointed

²² Cited in J. E. Carpenter, op. cit., note 10 above, pp. 64–65.

²³ 'New editions of physiological works', [by Carpenter, William S. Kirkes, and John C. Dalton], *Br. for. med. Rev.*, 1865, 36: 51–71. This quotation is from p. 51.

²⁴ Cited in J. E. Carpenter, op. cit., note 10 above, p. 80.

²⁵ W. B. Carpenter, *Principles of physiology, general and comparative*, London, J. Churchill, 1851, pp. VIII–IX.

to the Fullarian Chair of Physiology at the Royal Institution and had been elected to the Royal Society. He now conducted the course of General Anatomy and Physiology at the London Hospital where he continued to lecture for the next twelve years. In 1847 on the retirement of Sir John Forbes (1787–1861), he became editor of the *British and Foreign Medical Review*, to which he had already contributed articles and reviews. In the same year he was appointed examiner in physiology and comparative anatomy in the University of London, and when the Fullarian Professorship expired the trustees of the British Museum designated him for the Swiney Lectureship on Geology. In 1849 he moved to the chair of Medical Jurisprudence at University College, leaving that in 1856 to become registrar of the University of London, a post he held for the next twenty-three years. From the start of his registrarship until his death his scientific activities were confined to preparing new editions of his *Physiologies*, developing his physiological views to incorporate “mental physiology”, conducting his own investigations in marine biology, and promoting various scientific enterprises such as the voyage of the *Challenger*.²⁶ His later years were also marked by an intense religiosity, so that when he died he was honoured by scientists and churchmen alike.²⁷

THE ROLE OF FORCE IN CARPENTER'S PHYSIOLOGY

There is a preliminary point to make which provides a key to Carpenter's entire natural philosophy. In 1828 he attended a lecture at the Bristol Mechanics' Institute given by a Mr. Thomas Exley (1775–1855) on a new theory of matter. Excited by it, he read Exley's book, *Principles of natural philosophy, or a new theory of physics*²⁸ (1829), from which he got an introduction to the theory of Boscovich and to the notion of matter being explicable wholly by forces. Exley was not an original or particularly influential figure, but he did reflect a considerable interest in dynamical theories of matter which constituted a strong undercurrent in the natural philosophy of his day.²⁹ Exley took his cue from Newton³⁰ and Boscovich,³¹ aiming to construct a simpler dynamical theory of matter than either of them. His theory was essentially Boscovich's, but pruned with Newton's *regulae philosophandi*; the first sphere of repulsion and the last sphere of attraction of Boscovich were retained, whilst the intermediate, alternating spheres of force were discarded since Newton's rules did not allow a multiplicity of causes if few would suffice. The core of his theory can be seen in his 'Definition 24': “Absolute force of an atom is its force at a given distance

²⁶ See Eric Linklater, *The voyage of the Challenger*, London, J. Murray, 1972. Also Sir Maurice Yonge, 'The inception and significance of the "Challenger" expedition', *Proc. roy. Soc. Edinb.* (B), 1971–72, 72: 1–13.

²⁷ For a discussion of Carpenter's religion see J. E. Carpenter, op. cit., note 10 above, pp. 33–46 and 147–152 especially.

²⁸ T. Exley, *Principles of natural philosophy, or a new theory of physics*, London, Longman, 1829.

²⁹ Exley and his impact are discussed by D. M. Knight, *Atoms and elements*, London, Hutchinson, 1967, pp. 65–70.

³⁰ From both *Opticks* and *Principia*, although most of his quotations were from the latter, especially from Book 3 which contained the *regulae philosophandi*.

³¹ R. J. Boscovich, *Theoria philosophiae naturalis*, Vienna, 1758; trans. into English from the 1763 Venetian edition by J. M. Child in 1921 and most recently published in English as *A theory of natural philosophy*, Cambridge, Mass., Massachusetts Institute of Technology Press, 1966.

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from the centre, and this is called its mass or quantity of matter.”³² Exley’s ideas were more extreme and explicit than those of most of his contemporaries, yet it is well known that men like Davy and Michael Faraday were acquainted with Bos-covicheanism and would not have found Exley’s development of it wholly uncongenial. Exley’s theory was around for at least twenty years, for he was still expounding it in 1848 at the annual meeting of the British Association for the Advancement of Science.³³ Whether Carpenter had any contact with him and whether his theory continued to interest him I cannot say, but power or force constituted a permanent, and sometimes a dominant, strand in Carpenter’s science.

Throughout his physiology Carpenter sought functional, and at a deeper level, dynamical relations within the vital economy, relying heavily on the latest experimental work of British and Continental investigators. An early instance of this was his use of von Baer’s principle of development for his paper ‘On the unity of function in organized beings’ (1837). One field where he could apply his extrapolations of von Baer’s principle was sense-perception, a field that several physiologists had been investigating experimentally and with promising results.³⁴ Carpenter speculated that the specialized functions of sight, hearing, smell, and taste might be mere modifications of a fundamental sense of touch; such unity of functions accorded with his tentative belief in a unity among the powers which effected the inorganic phenomena of Nature, which he discussed in a paper in 1838:

Is it possible that these physical and vital properties of matter, which are at present our ultimate facts and axioms, may be hereafter included in a more general expression common to both? On this subject we can only speculate; but the probability appears decidedly in the affirmative. We have already remarked upon the rapid progress of generalization in the physical sciences, rendering it probable that before long one simple formula shall comprehend all the phenomena of the inorganic world; and it is not perhaps too much to hope for a corresponding simplification in the laws of the organized creation. . . .³⁵

This hope also had theological value for him, since every step towards a comprehensive explanation of Nature was evidence of the beauty and harmony of the world and of the mind of its Creator.³⁶

The quest for a unifying theory for vital and inorganic phenomena reappeared in his *General and comparative physiology* (1839). In its final pages much of the 1838 paper was repeated. He also attempted to reconcile the arguments of the vitalists (such as Charles Bell) with those of the mechanists; for instance, although admitting that some functions may be called truly “vital” in that they occur only in living systems, he nonetheless cited experimental evidence to argue that so-called vital powers or forces derive solely from the powers supplied to the organism in its food

³² Exley, *op. cit.*, note 28 above, p. 3.

³³ T. Exley, ‘On the laws of chemical combinations and the volumes of gaseous bodies’, *Rep. Br. Ass. Advmt Sci.*, 1848, pp. 50–51 of transactions of the sections.

³⁴ Goethe had conducted sense-perception experiments on himself. So had the German physiologist-antonomist Johannes Müller (1801–1858) and the Czech physiologist Jan Evangelista Purkyně (1787–1869).

³⁵ This 1838 lecture is reproduced in J. E. Carpenter, *op. cit.*, note 10 above, pp. 155–158. This quotation is from pp. 156–157.

³⁶ *Ibid.*, pp. 157–158.

and oxygen. Certainly, he would not allow an organism to generate its own power—one of the traditional characteristics of vital power being that it was *sui generis*—since there was abundant evidence that the excessive use of power in one physiological function was always compensated by a corresponding reduction in another. Thus in plants there was apparently an “antagonism” between the nutritive and reproductive function, the one always executed at the expense of the other.

To Carpenter, the clearest instance of a dynamical relation between an organism and its environment was respiration.³⁷ This belief in respiration as the primary source of an organism’s power (as “the bent spring which keeps the clock in motion”, to use Liebig’s celebrated phrase of 1842) was not new. Lavoisier and Séguin had proposed it, and it was a prominent idea throughout nineteenth-century physiology.³⁸ However, Carpenter’s discussion is noteworthy as he was anxious not to adhere to it until experimental evidence left no room for doubt; only in his later writings, and particularly after Liebig’s contribution to the debate, did he give his total assent to it.

This reluctance to theorize prematurely can be seen in the gradual development of reductionism in successive editions of his *Physiology*; the later the edition, the more experimental evidence he could cite and the more explicitly reductionist he sounded. If we take the first edition of *General and comparative physiology*, there is only one paragraph which really hints in this direction: “It has been one object of the foregoing pages to show that *vital* properties are as essentially connected with certain forms of matter, as are those usually denominated *physical* with matter under its more common aspects. One more question yet remains. Is it possible that the physical and vital properties of matter, which are at present our ultimate facts and axioms, may be included within a more general expression, common to both?”³⁹ That more general expression was to be none other than the correlation of all forms of force or power and the uncreatability and indestructibility of power itself. Taking his work in chronological order, we find nothing new on this theme in the second edition⁴⁰ (1841). But there was new material in the first edition of his *Principles of human physiology*⁴¹ (1842), where power was an important theme, particularly in the sections on respiration and sexual activity. On respiration, he discussed recent experiments on bees by a Mr. George Newport (1803–1854) who had shown that the quantity of oxygen they consumed was proportional to the heat they generated, indicating that animal heat had a purely chemical origin.⁴² Carpenter knew of a similar conclusion drawn by the French physicist Antoine-César Becquerel (1788–1878) and his anatomist-colleague Gilbert Breschet (1784–1845) from their experiments on higher animals: needles had been inserted deep into active muscles and connected to a

³⁷ Carpenter, *op. cit.*, note 20 above, p. 299 and elsewhere.

³⁸ Among the several studies that have been done on late eighteenth- and nineteenth-century ideas on respiration, two of the most useful are J. Goodfield, *The growth of scientific physiology*, London Hutchinson, 1960; and E. Mendelsohn, *Heat and life, the development of the theory of animal heat*, Cambridge, Mass., Harvard University Press, 1964.

³⁹ Carpenter, *op. cit.*, note 20 above, p. 463.

⁴⁰ W. B. Carpenter, *Principles of general and comparative physiology*, 2nd ed., London, J. Churchill 1841.

⁴¹ Carpenter, *op. cit.*, note 21 above.

⁴² For a brief account of Newport’s work see *Proc. R. Soc.*, 1855, 7: 278–285.

thermo-multiplier; the heat generated had been exactly proportional to the muscles' activity.⁴³ Such experiments convinced Carpenter that animal heat was due solely to the chemical power obtained from respiration and from other reactions within the body's tissues, and that muscle-force was related intimately to respiration since he believed the latter to be a tissue-based process and not, as Lavoisier had said, localized in the lungs.

Also in 1842 Carpenter wrote a review-essay⁴⁴ on Liebig's latest publication, his *Animal chemistry*.⁴⁵ That review is more useful than his *Human physiology* for understanding his ideas on powers for, to his surprise, Liebig had reached similar ideas; and in reviewing Liebig's conclusions it was inevitable that he would draw attention to his own. Liebig too had decided that "vital force" or "vitality" depended upon a certain arrangement of elementary particles, just as chemical force depended on certain arrangements of atoms within molecules. Liebig declared "*There is nothing to prevent us from considering the vital force as a peculiar property, which is possessed by certain material bodies and becomes sensible when their elementary particles are combined in a certain arrangement or form.*"⁴⁶ With this, Carpenter agreed, adding that it had been the essential object of his essay, 'On the laws regulating vital and physical phenomena' (1838). He was delighted to find his own view "strengthened by the independent testimony of so eminent a philosopher. A more valuable example could scarcely be adduced of the fact that fellow-labourers in the pursuit of truth, engaged in the same enquiries and with the same spirit, are likely to arrive at the same conclusions, and may express them in language almost identical."⁴⁷

Carpenter also agreed with Liebig's theory of animal motion, on which he quoted a passage from *Animal chemistry* that is well known to historians analysing Liebig's contribution to the emergence of the conservation of energy. That passage demonstrated Liebig's and Carpenter's view of force (or energy, to use the modern equivalent) so well that it merits quotation here:

We observe, further, that the voluntary and involuntary motions, in other words all mechanical

⁴³ Although Carpenter did not specify which papers by Becquerel and Breschet he had read, the following are the most likely to have influenced him: (i) 'Recherches expérimentales physico-physiologiques sur la température des tissus et des liquides animaux', *C. r. Séanc. Acad. Sci., Paris*, 1836, 3: 771–781; *Annls Sci. nat., (Zool)*, 1837, 7: 94–101. (ii) 'Nouvelles observations sur la mesure de la température des tissus organiques du corps de l'homme et des animaux au moyen des effets thermo-électriques', *C. r. Séanc. Acad. Sci., Paris*, 1838, 6: 429–437; *Annls Sci. nat., (Zool)*, 1838, 9: 271–280; *Sturgeon, Ann. Electr.*, 1838, 2: 467–474. (iii) 'Recherches sur la chaleur animale, au moyen des appareils thermo-électriques', *Archs Mus. Hist. nat. Paris*, 1839, 1: 383–404. (iv) 'Mémoires: 1^{mo}. Sur la détermination de la température des tissus organiques de plusieurs mammifères; 2^{do}. Sur la température différente du sang artériel et du sang veineux', *C. r. Séanc. Acad. Sci., Paris*, 1841, 13: 792–797.

As this list indicates, there is a lot of French physiological material which deserves study from the point of view of the dynamics of the living organism and with possible relevance to the correlation of forces and conservation of energy. So far as I am aware, no such study has been done.

⁴⁴ W. B. Carpenter, 'Liebig's *Animal chemistry*', *Br. for. med. Rev.*, 1842, 14: 492–521.

⁴⁵ J. Liebig, *Animal chemistry, or organic chemistry in its application to physiology and pathology*, edited from the author's manuscript by W. Gregory, London, Taylor & Walton, 1842. There is an excellent modern edition with an introduction by F. L. Holmes, published by Johnson Reprint Corporation, New York and London, 1964.

⁴⁶ This extract is on p. 496 of Carpenter's review and comes from p. 209 of Gregory's 1842 edition.

⁴⁷ Carpenter, *op. cit.*, note 44 above, p. 497.

effects in the animal organism, are accompanied by, nay, are dependent on, a peculiar change of form and structure in the substance of certain living parts, the increase or diminution of which change stands in the very closest relation to the measure of motion, or the amount of force consumed in the motions performed. As an immediate effect of the manifestation of mechanical force, we see that a part of the muscular substance loses its vital properties, its character of life; that this portion separates from the living part and loses its capacity of growth and its power of resistance. . . . all experiment proves that this conversion of living muscular fibre into compounds destitute of vitality is accelerated or retarded according to the amount of force employed to produce motion. Nay, it may safely be affirmed that they are mutually proportional; that a rapid transformation of muscular fibre or, as it may be called, a rapid change of matter, determines a greater amount of mechanical force; and conversely, that a greater amount of mechanical motion (of mechanical force in motion) determines a more rapid change of matter. . . . no other conclusion can be drawn but this, that the active or available vital force in certain living parts is the cause of the mechanical phenomena in the living organism.⁴⁸

In his discussion of this passage Carpenter implied that the quantity of motion plus vital energy expended during movement is proportional to the chemical energy procured by the body through respiration. Of course, Carpenter's own words were not so precise or modern, but there can be little doubt that such was his idea, for by 1850 his words became unequivocal.

One instance of Carpenter's use of power, and of how up-to-date his physiology was, occurred in the discussion of muscle contraction in the third edition of *Human physiology* (1846). There he discussed Helmholtz's research on the chemical changes accompanying muscle activity, which Helmholtz had done the previous year,⁴⁹ and it was evident that he believed in a correlation between organic and inorganic powers, "that the development of the Contractile Force is in some way dependant on the Chemical Change, which seems to be so essential a condition of it; just as the development of the Electrical Force of the Galvanic Battery is dependant on the new chemical arrangements which take place between the bodies brought to act upon one another. . . ."⁵⁰

By now, his belief in the close relationship of at least some of the so-called "vital" and physical powers had almost eliminated any need for a distinctive vital force. By now he felt there could no longer be any doubt that respiration was a purely chemical phenomenon. Indeed, Carpenter seems to have been one of the earliest British physiologists to appreciate and advocate that essentially dynamical view of respiration which originated in Lavoisier's and Séguin's first memoir on respiration in 1789. We might recall that, having measured the different oxygen absorptions when a man eats, fasts, rests, and works, they had concluded that oxygen consumption was a measure of the equivalence of all forms of work. They had written:

This sort of observation permits us to compare the use of forces between which there might appear to be no relation. One could learn, for example, what weight in pounds corresponds to

⁴⁸ p. 499 of Carpenter's review and p. 221 of Gregory's 1842 edition.

⁴⁹ Helmholtz had experimented on frog leg muscles, concluding that muscle activity was accompanied by, indeed probably required, a certain amount of chemical activity in the muscle itself. His experiment was one of the earliest to demonstrate clearly the intimate connexion between muscle contraction (a "vital" process) and chemical reaction (a non-vital process). There is a succinct account of Helmholtz's work in Russell Kahl, *Selected writings of Hermann von Helmholtz*, Middleton, Conn., Wesleyan University Press, 1971.

⁵⁰ W. B. Carpenter, *Principles of human physiology*, 3rd ed., London, J. Churchill, 1846, p. 441.

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the efforts of a man reciting a discourse, or a musician playing an instrument. One could even estimate how much there is of a mechanical nature in the work of a philosopher while reflecting, a man of letters while writing, a musician whilst composing. These efforts, ordinarily considered as purely moral, have something physical and material about them which permits them to be compared, in this respect, with the efforts of a labouring man. It is, then, not without a certain rightness that the French language has confounded under the single designation work [*travail*] the efforts of the spirit with those of the body, and work done in a study with that done in a shop.⁵¹

Carpenter took Lavoisier's and Séguin's suggestion of the equivalence of mental and physical powers seriously, for that was one theme which developed steadily in successive editions of his *Physiologies*, culminating with a treatise specifically advocating it, his *Principles of mental physiology*⁵² (1874).

Returning to respiration and allied topics, the next two editions of *Human physiology* contained hardly anything new, but the sixth edition (1864) had important new ideas on power and defended several ideas which had been only tentative in earlier editions. But before discussing it we might examine two reviews he wrote meanwhile for the *British and Foreign Medical Review* and his Royal Society paper of 1850.

The reviews were of two editions of *Lectures on the physical phenomena of living beings* by Carlo Matteucci⁵³ (1811–1868). The 1847 review was of the first French edition (the original was in Italian) and the 1848 review was of the first English translation. Only one year separated these two reviews of essentially the same book, yet in that year Carpenter underwent a noticeable change of mind which resulted in his previously rather tentative ideas on the dynamical relations between the organic and inorganic world becoming definite and explicit and his own view of life becoming totally mechanistic.

In 1844 the government of Tuscany invited Carlo Matteucci, professor of physics at Pavia, to deliver a series of lectures on the physical phenomena of living beings. Matteucci's lectures, which relied greatly on his own experimental researches, were printed immediately in Italian; a second edition soon appeared from which he supervised a French translation, which Carpenter reviewed in 1847. Carpenter had also heard Matteucci describe his work at the 1846 meeting of the British Association for the Advancement of Science. In the 1847 review⁵⁴ Carpenter commended Matteucci's book as an excellent treatise in experimental and philosophical physiology; he approved Matteucci's aim to account for living phenomena by physical and chemical agents or powers and he agreed with most of his conclusions, for instance his conclusion that nerve-force and electricity, though not identical, were so strongly analogous as to be grouped with other dynamical agents like light and heat. Carpenter

⁵¹ A. Lavoisier and A. Séguin, 'Premier mémoire sur la respiration des animaux' (1789), in *Oeuvres de Lavoisier*, Paris, Imprimerie Royale, 1864–93, 2: 697.

⁵² W. B. Carpenter, *Principles of mental physiology*, London, H. S. King, 1874.

⁵³ First published as *Lezioni sui fenomeni fisico-chimici dei corpi viventi*, Pisa, Minerva, 1844. The first English edition was *Lectures on the physical phenomena of living beings*, translated by Jonathan Pereira, London, Longman, Brown, Green & Longman, 1847.

⁵⁴ 'Review of Professor Carlo Matteucci's *Leçons sur les phénomènes physiques des corps vivants*, édition française publiée avec additions considérables sur la deuxième édition italienne', *Br. for. med. Rev.*, 1847, 23: 377–409.

had only one exception to Matteucci's apparent belief that all forces, vital as well as inorganic, belonged to a single category of natural phenomena: he could not see how physics and chemistry would ever explain cell growth and reproduction. The closest analogy was perhaps inorganic catalysis, but growth and reproduction were so much more complex "as to have an unquestionable claim to rank as a distinct order of phenomena; and we cannot stretch our imagination even so far as to glimpse at the possibility of including them legitimately in the same generalization with those of physics and chemistry".⁵⁵

Consequently, his reaction to Matteucci's *Lectures* was that, although he believed that Matteucci's method—of physico-chemical experimentation—was *the* means for comprehending living phenomena, and that in most cases it would succeed, there were still two phenomena, cell growth and reproduction, which threatened to remain inscrutable.

However, when he wrote his 1848 review⁵⁶ of Matteucci's English edition, he seemed willing to admit that all living phenomena without exception might yield to experimental inquiry. The cause of this change is clear: he had read and had been convinced by Grove's theory *On the correlation of physical forces* (1846), realizing its application to the forces in life. Whereas the 1847 review had not used "correlation" at all, it became the key word for the later review. For instance, Carpenter now wanted to go beyond Matteucci's warning that electricity and nerve-force are not identical; he believed that their extraordinary similarity indicated a real relationship between them:

To use Professor Grove's term, they are mutually correlated. . . . This correlation is incidentally noticed by Professor Matteucci in more than one of his writings; but he does not anywhere (to our knowledge at least), develop it as fully as it seems to us to deserve. For his attention has been fixed so exclusively upon the relation of the nervous force, as manifested in muscular motion, to electricity alone, that he has altogether overlooked the corresponding relation which it bears to those other forces to which electricity is itself correlated. . . .⁵⁷

In his new review Carpenter intended to make up this deficiency of Matteucci's by indicating other probable correlations between vital and inorganic agents. He first chose heat, pointing out that when it is applied to motor nerves, muscle contraction ensues, and when applied to sensory nerves, sensation results. Conversely, there were instances when the production of animal heat could not be explained by "the purely chemical doctrine of calorification" and which indicated that nerve-force might generate animal heat; for instance, sectioning of the spinal cord of a warm-blooded animal was seen often to produce an elevation of temperature in the parts of the body below the section. Therefore, one might conclude that there was "a relation between these two agencies of the same *kind* with that which exists between the nervous force and electricity, though less intimate in *degree*."⁵⁸

⁵⁵ *Ibid.*, p. 380.

⁵⁶ 'Review of *Lectures on the physical phenomena of living beings*. By Carlo Matteucci. . . . Translated under the superintendence of Jonathan Pereira, M.D., F.R.S., Vice-President of the Royal Medical and Chirurgical Society', *Br. for. med. Rev.*, 1848, 1: 228–235.

⁵⁷ *Ibid.*, p. 232.

⁵⁸ *Ibid.*, p. 233.

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On light and magnetism, with regard to their effects on nerves, too little was known for Carpenter to say anything positive. But he mentioned a few phenomena, such as a curious luminosity of certain marine worms, which suggested the actual production of nerve-force by light and vice versa.⁵⁹

Nerve-force was not the only vital force which seemed to be correlated with physical ones. The *vis musculosa* might produce electricity too, according to Matteucci's very latest research, and if recent observations by the French naturalist Armand de Quatrefages (1810–1892) were correct,⁶⁰ muscular contraction seemed to be capable of producing light. The logical extension of these observations, in Carpenter's opinion, was to inquire whether all those agents, usually called stimulants when acting upon the living organism, had their effect

due to a correlation between the stimulating force and the force (of whatever kind) which is the result of the operation. Thus *heat* is commonly said to be a stimulant to the process of nutrition. . . . But it may be reasonably asked, whether the heat does not operate by directly producing the chemical affinities on whose action these processes immediately depend; and whether these again are not similarly correlative to the *vital force* which the tissue, when once generated, is found to possess. We are inclined to believe that such will ultimately prove the case: and we offer the speculation, crude though it may at present seem to be, because we think it may serve to give a useful direction to scientific inquiry.⁶¹

The review ended by emphasizing that a much more intimate relation exists between physical and vital phenomena than had been thought hitherto. Into this pattern the correlation principle fitted most neatly. The only reservation he now expressed was that the correlation theory embraced only the material functions of the organism and not the entirely different phenomena of the mind.

We come now to Carpenter's Royal Society paper 'On the mutual relations of the vital and physical forces'.⁶² We might well consider this as the high-water mark of his scientific career. It is almost certain that he himself thought so, for it was the solution to the question he had asked in the first edition of his *General and comparative physiology*, namely "Is it possible that the physical and vital properties of matter . . . may be included within a more general expression common to both?"⁶³

He had probably been evolving the argument of his Royal Society paper since some time in 1847, for in that year he twice floated it for discussion at the June meeting of the British Association for the Advancement of Science; in a letter to his wife on 25 June he wrote: "In the Medical Section, where I spent most of this morning, I gave some views which I had formed on the correlation of the Vital and Physical Forces suggested by Mr. Grove's pamphlet. I shall bring these forward also in the Physical Section, where I think they will be better appreciated."⁶⁴ Since then he had

⁵⁹ *Ibid.*, p. 234.

⁶⁰ *Ibid.*, p. 235. Carpenter cited a paper on de Quatrefages by M. Milne-Edwards, 'Rapport sur une série de mémoires de M. A. de Quatrefages, relatifs à l'organisation des animaux sans vertèbre des côtes de la Manche', *Ann. Sci. nat.*, troisième série, tom. i, p. 23.

⁶¹ *Ibid.*, p. 235.

⁶² W. B. Carpenter, 'On the mutual relations of the vital and physical forces', *Phil. Trans. R. Soc. Lond.*, 1850, pp. 727–757.

⁶³ Carpenter, *op. cit.*, note 20 above, p. 463.

⁶⁴ Cited in J. E. Carpenter, *op. cit.*, note 10 above, p. 49.

hinted at these views in various writings, for instance his 1848 review of Matteucci. Besides, these views had been germinating in his *Physiologies*, as he admitted in a letter in 1849: "All this I have developed in my *Comparative physiology*—not as fully, however, as I could wish."⁶⁵

In the Royal Society paper Carpenter acknowledged his principal debt to Grove. However, he considered Grove's theory of the correlation of physical forces not an innovation but rather a crystallization of ideas, ideas that men like Liebig and himself had been formulating. His own task now was to bring out the important physiological implications of correlation. Actually, Grove himself had suggested the physiological relevance of his theory,⁶⁶ but Carpenter doubted whether he envisaged a true correlation between physical powers and the most vital, most inscrutable, of organic functions, namely cell growth, development, and reproduction. To plumb these living functions was the task of a physiologist. Grove was not a physiologist; Carpenter was.

The paper's aim was "... that Physiological science should be considered under the same dynamic aspect, as that under which the Physical sciences are now viewed by the most enlightened philosophers."⁶⁷ This aim had for him a philosophical foundation in John Locke (1632–1704), for he agreed with Locke's assertion that all power or force, apart from that which arises from human free-will, proceeds directly from the will of God. Thus, regarding physical forces as various *modi operandi* of a single, fundamental agency, namely the creative and sustaining will of God, Carpenter could not agree with the objections that had been raised against the "metamorphosis or conversion of forces."⁶⁸ He referred particularly to chapter 21, 'On power', in Locke's *An essay concerning human understanding*.⁶⁹

Locke was cited again towards the end of the paper. Starting with Locke's abstract notion of force or power, as that which emanates directly from God's will, Carpenter proposed that such force becomes actual in operating through matter, appearing as electricity, magnetism, heat, light, chemical affinity, and mechanical motion when operating in inorganic matter, and effecting growth, development, chemicovital transformations, etc., in organized beings;⁷⁰ it is further metamorphosed, by the

⁶⁵ *Ibid.*, p. 62.

⁶⁶ W. R. Grove, *On the correlation of physical forces*, London, S. Highley, 1846, p. 49. This suggestion occurs at the end of the book and presumably indicated Grove's view of how his theory might develop. This is borne out by a collection of sixty-five letters between Grove and Matteucci, now located at the Royal Institution of Great Britain. For the whereabouts of these letters I am indebted to my post-graduate student, Mike Cooper.

⁶⁷ Carpenter, *op. cit.*, note 62 above, p. 757.

⁶⁸ *Ibid.*, p. 730. Clearly, the origins of Carpenter's idea of metamorphosis of forces need further investigation. One naturally thinks of the possible influence of *Naturphilosophie*; yet, it is worth emphasizing that he seems to have eschewed all forms of German metaphysics, at least in his printed writings.

⁶⁹ In my study of late eighteenth- and early nineteenth-century British physiologists who were interested in the dynamics of the living organism, I have come across numerous references to Locke's *Human understanding*, especially to chapter 21, 'On power'. See my Ph.D. thesis, *op. cit.*, note 4 above, the Introduction and Chapter 1. There are several lengthy studies of the concepts of power, causality and allied topics in British philosophy from Locke onwards; two that I have found useful are: P. M. Heimann and J. E. McGuire, 'Newtonian forces and Lockean powers: concepts of matter in eighteenth century thought', *Hist. Stud. phys. Sci.*, 1971, 3: 233–306. And Richard Olson, *Scottish philosophy and British physics, 1750–1880*, Princeton, N.J., Princeton University Press, 1975.

⁷⁰ Carpenter, *op. cit.*, note 62 above, p. 752.

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organized structures it helps create, into nervous and muscular powers. In these more philosophical sections of his paper we see some of the extra-scientific roots of his ideas on force, for instance the British philosophers' discussion of power or force; (he cited only Locke, but there was a continuous discussion of power throughout eighteenth- and early nineteenth-century philosophy in Britain, for instance by David Hume (1711–1776), who sought to demolish the certitude with which Locke derived all agents or powers from the ultimate agency of the will of God, and by Thomas Reid (1710–1796) and Dugald Stewart (1753–1828), who both attempted to vindicate Locke against Hume's scepticism. Dugald Stewart was cited not infrequently by Carpenter in his physiological writings.⁷¹) Another source of ideas on force with which Carpenter was acquainted, but of which he seems to have been extremely wary, was *Naturphilosophie*.

Carpenter's paper began with an explanation of how his view of the relation between vital functions and physical forces differed from other physiologists'. Whereas they had envisaged those forces acting only as stimuli on the living organism and rarely, if ever, actually participating in its internal, living dynamics, he sought a more intimate relation. He saw his approach as original in its attempt to be rigorous, for he had never found ". . . in physiological writings any indication of a more intimate relationship between the physical forces and vital phenomena, than that just stated—save on the part of those who have vaguely identified Heat or Electricity with the 'vital principle', with about the same amount of philosophical discrimination as that which was exercised by the iatro-chemists and iatro-mathematicians of the sixteenth and seventeenth centuries".⁷²

Carpenter wished to express that relationship between the physical world and living phenomena in the most rigorous and non-hypothetical way:

. . . that A, operating upon a certain form of matter, ceases to manifest itself, but that B is developed in its stead; and that *vice versa*, B, operating upon some other form of matter, ceases to manifest itself, but that A is reproduced in its stead. The idea of correlation also involves that of a *certain definite ratio* or equivalent between the two forces thus mutually interchangeable; so that the measure of force B which is excited by a certain exertion of force A shall in its turn give rise to the same measure of force A as that originally in operation.⁷³

Although he knew of no conclusive evidence for this conservation of force, he knew of enough quantitatively studied conversion processes, like electrolysis, to make it highly plausible. The most interesting point, however, is not that Carpenter was enunciating a conservation of force, but that he regarded it as subservient to the correlation theory. This attitude persisted throughout his subsequent writings on force and energy, even when he had become acquainted with, and was acknowledging the brilliance of, Mayer's work (of which he knew nothing at the time of this paper).

Section II of the paper sought mutual relations among the "vital functions", by which he meant those processes which occur only in life and whose complexity seemed to raise them above the range of simple chemistry and physics. This distinction

⁷¹ I discuss the ideas on force and power of these philosophers in chapter 1 of my thesis.

⁷² Carpenter, *op. cit.*, note 62 above, p. 729.

⁷³ *Ibid.*, p. 731.

seemed valid, although he would not admit any peculiarly vital *force* as its basis: it was the phenomena or the gross behaviours of organisms which were vital, not the forces underlying those phenomena. He illustrated this view by discussing the ideas on blood of his former Edinburgh teacher, Alison; he agreed with Alison on the vitalistic behaviour of the blood and circulation, but he disagreed with Alison's hypothesis of vital forces subsuming such behaviour.⁷⁴

Carpenter used several arguments, all based on recent researches, to establish that the forces operating in living organisms are mutually correlated. One argument arose from the cell theory of Theodore Schwann (1810–1882). Since all forces operated through a common instrument, the cell, and since all cells in any organism descended from one initial cell, and since in unicellular organisms all functions and forces operated without specialization, all forces in life could therefore be called collectively "cell force". By "cell force" Carpenter did not mean another ontological, incomprehensible "vital force"; the phrase was meant to be like the term "engine-power", which everyone knew was simply a convenient expression for the conversion of heat to mechanical motion in steam engines.⁷⁵

A correlation among vital forces accounted neatly for various physiological phenomena, for example the debility following excessive sexual indulgence. This example had occurred in his *Human physiology*, in words similar to those he now used:

That a relation of reciprocity exists between the forces concerned in the growth, development and maintenance of the individual organism, and those which are employed in the generative act—so that an excessive expenditure of either diminishes the amount of vital force which is applicable to the other—is an idea so familiar to physiologists that the author need not dwell here upon it, further than to point out how completely it coincides with, and illustrates, the view for which he is contending.⁷⁶

Correlation also explained another physiological process, the interaction between nerve-power and muscle contractility. On this he particularly cited Matteucci's work, seeing Matteucci's *Lectures* as corroborating his own conclusions which, he emphasized (as he had done in the 1848 review), he had reached independently. Carpenter closed section II with a physiological version of correlation: so close a mutual relation exists among the forces in life that they might be considered modes of a common cell-force. This also implied (in my opinion) a conservation, or at least a non-creation, of vital power, for only then could his assertion that an organism becomes exhausted following overactivity of any one function, of the sexual function for instance, have a real basis. Indeed, (in my opinion) it would have been pointless to construct a correlation theory for vital forces as rigorously as he tried to do, and there could have been no prospect of testing that theory, if Carpenter had not envisaged the organism as a non-creator of power, just as physicists already envisaged machines. Although section II contained no explicit assertion of conservation of power or force, section III developed along that line.

⁷⁴ *Ibid.*, p. 736. Alison's views can be found in his *Outlines of physiology*, pp. 2 and 4, and in his *Outlines of physiology and pathology*, p. 58, *op. cit.*, note 16 above.

⁷⁵ Carpenter, *op. cit.*, note 62 above, p. 737.

⁷⁶ *Ibid.*, p. 739.

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Section III was called ‘Relations of the vital and physical forces’. Once again, Matteucci was cited, for he had apparently disproved an identity between nerve-force and electricity. However, Carpenter discussed the evidence for their correlation or mutual convertibility—electricity passing through nerve-fibres developed nerve-force, and nerve-force when acting on special apparatus developed electricity.⁷⁷ He also cited Johannes Müller’s (1801–1858) principle of specific nerve sensations,⁷⁸ which accorded with the correlation theory, and suggested to him that the relation between electricity and nerve-force was analogous to that between electricity and magnetism, nerve-force being an effect of electricity in a nerve-fibre, and magnetism being an effect of electricity in iron. Experiments by Humphry Davy, Faraday, and Matteucci on electric fish were cited, for their electrical effects had been found to be “in precise accordance with the amount of nervous force which is transmitted”.⁷⁹ Carpenter therefore concluded that there was a quantitative as well as a qualitative relation, namely a conservation, between the nervous and electric powers during their inter-conversion.

As in his 1884 review, he also discussed the likely correlations between nerve-force and physical forces other than electricity, namely heat, chemical affinity, light, and motion. On animal motion he cited Müller’s *Physiology* and again hinted at a conservation of power: “That the motor force thus generated is always proportional, *caeteris paribus*, to the degree of nervous power exerted will be (the author believes) disputed by no physiologist . . .”.⁸⁰

The least substantiated correlation was between nerve-force and magnetism, on which he cited the researches of Baron Karl von Reichenbach⁸¹ (1788–1869) and Faraday, which indicated a likely connexion.

On correlations between physical forces and vital forces other than the nervous there was abundant evidence. For instance, he adopted Liebig’s recent explanation of muscle-force; since his words summarize his and Liebig’s ideas and aims in physiological dynamics so well, they merit quotation *in extenso*:

These agencies [electricity, heat etc.], however, do not appear so directly concerned in the production of the motor power, as in occasioning that metamorphosis of living, organized tissue into chemical compounds, whereon the development of the muscular force seems to be immediately dependent. It is now universally admitted that the disintegration of a certain amount of muscular tissue, and the new arrangement of its components in combination with

⁷⁷ *Ibid.*, p. 744.

⁷⁸ *Ibid.*, p. 745. See W. Baly’s translation of Johannes Müller’s *Elements of physiology*, London, Taylor & Walton, 1838–1842, p. 1062.

⁷⁹ *Ibid.*, p. 744.

⁸⁰ *Ibid.*, p. 746.

⁸¹ Karl von Reichenbach had been conducting researches on electricity, magnetism, heat, etc., particularly with regard to the living organism, since c. 1840. Although he was outside the mainstream of scientific work, in that he belonged to no university or academic institute and his ideas were often dismissed as eccentric or even worse, he published extensively and two of his major works had been translated into English by 1850. These were *Abstracts of ‘Researches on magnetism and on certain allied subjects’, including a new supposed imponderable*, edited by W. Gregory, London, Taylor & Walton, 1846; and *Physico-physiological researches on the dynamides or imponderables, magnetism, electricity, heat, light, crystallization and chemical attraction, in their relations to the vital force*, edited by W. Gregory, London and Edinburgh, Taylor, Walton & Maberley, 1850. I have discussed his work in chapter 15 of my thesis, *op. cit.*, note 4 above.

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oxygen supplied by the blood, is necessary for the development of its contractile force; and the considerations adduced by PROFESSOR LIEBIG render it highly probable that the muscular contraction may be regarded as proceeding from the expenditure or metamorphosis of the cell-force, which ceases to exist as a *vital* power, in giving rise to *mechanical* agency. The amount of muscular force developed appears to bear an exact correspondence with the amount of urea formed by the metamorphosis of the muscular tissue; and this metamorphosis involves the cessation of its existence as a living structure, and consequently the annihilation of the vital forces which that structure possessed. We are, then, to regard the nervous, electrical and other stimuli, under whose influence the muscular force is called forth, less as the immediate sources of that force, than as furnishing the conditions under which the vital force, acting through the muscle, is converted into the mechanical force developed in its contraction.⁸³

Carpenter considered it relatively easy to correlate the forms of vital force mentioned so far with physical forces. A greater challenge was to show how physical forces effected growth and development, an endeavour he had considered hopeless up until 1848. One of the strongest evidences was the simple fact that growth occurs only where light and/or heat attain a certain level. The boldest hypothesis for growth being totally dependent on these physical forces had been proposed by Jean Baptiste Boussingault (1802–1887), the French chemist who had turned his attention to agricultural and physiological chemistry. Boussingault suggested that each type of plant requires a characteristic quantity of sunlight and heat during its normal lifetime, in other words a definite amount of energy, wherever it grows; its rate of growth would be directly proportional to the rate at which it receives light and heat, and its organizing force would be equivalent at all times to its intake of solar power.⁸³ Carpenter cited him eagerly, for his and others' researches had shown that heat was not merely a stimulus upon an organism, but that it actually participated in the dynamics of the creature's life, manifesting itself as "vitality". In Carpenter's opinion, this ". . . accords with the fact of the restoration to the inorganic world—under some form or other—of all the *force* thus withdrawn from it."⁸⁴ Clearly, the inorganic and organic worlds together constituted a closed system of power or force.

Carpenter believed this view differed significantly from that of other physiologists, a difference especially evident from their accounts of embryological evolution. This difference rested on Grove's correlation theory as applied to physiology, particularly Grove's critique of latent force:

According to the doctrine current among some physiologists, the whole 'organizing force', '*nisus formativus*', or '*bildungstrieb*', which is to be exerted in the development of the complete structure, *lies dormant in this single cell*, the germ (it has been affirmed) being 'potentially' the entire organism. And thus all the organizing force required to build up an oak or a palm, an elephant or a whale, is concentrated in a minute particle, only discernible by microscopic aid. As a refuge from this doctrine, . . . other physiologists, (among whom the author formerly ranked himself), have affirmed that vital force must exist in a dormant condition in all matter capable of becoming organized; that the germ cell, in drawing to itself organizable materials, and in incorporating these into the living structure, does nothing else than evoke into activity their latent powers; and thus that, with every act of growth and cell-multiplication, new vital force

⁸³ Carpenter, *op. cit.*, note 62 above, pp. 746–747.

⁸³ Boussingault's hypothesis is to be found mainly in his *Économie rurale considérée dans ses rapports avec la chimie, la physique et la météorologie*, 2 vols., Paris, Béchét jeune, 1843–44; trans. by G. Law, *Rural economy in its relations with chemistry, physics and meteorology*, London, H. Baillière, 1845.

⁸⁴ Carpenter, *op. cit.*, note 62 above, p. 751.

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is called into operation, whereby the process is continually maintained. This proposition does not involve any manifest absurdity. . . .

The views of PROFESSOR GROVE, however, strike at the root of the notion of *latent force* of any description whatever; all force once generated being, in his estimation, perpetually *active* in any form or another; and its supposed 'latency' being a hypothetical condition, the idea of which is quite unnecessary when the force which has ceased to manifest itself is recognized under some other form. Thus, in his view, when iron is rendered magnetic by an electric current, the development of the magnetic force is rather to be looked on as the result of the conversion of the electric, by the instrumentality of the iron, than as a case of the excitation of one force previously dormant by another which is expended in thus evoking it. Such an analogy should rather lead the physiologist to look for some extraneous source of the organizing force; and to suspect that when organizable materials are applied to the extension of a living structure, and are caused to manifest vital forces, some agency external to the organism is the moving spring of the whole series of operations. And thus, according to the view here advocated, the vital force which causes the primordial cell of the germ first to multiply itself, and then to develop itself into a complex and extensive organism . . . is directly and immediately supplied by the Heat which is constantly operating upon it, and which is transformed into vital force by its passage through the organized fabric that manifests it.⁸⁵

Heat was not the only physical force known to effect growth and development. William Frédérick Edwards (1776–1842) had studied the effect of light on the metamorphosis of batrachia;⁸⁶ the Scandinavian naturalist, Jørgen Christian Schiödte (1815–1884), had investigated the non-development of eyes of subterranean creatures like *Proteus anguineus* and *Amblyopsis spelaeus*;⁸⁷ Matteucci had investigated the influence of electricity on life. Such researches gave Carpenter evidence for his argument.

In the paper's concluding paragraphs Carpenter summarized his dynamical world-picture. He envisaged a continuous interchange and metamorphosis of matter and force between the inorganic and organic realms; the world was a closed system, within which the correlation (and by implication the conservation) of force/power and the conservation of matter were its fundamental, unifying principles. Matter and force were both real, but the chief was force: "So that, on the whole there is strong reason to believe that the entire amount of force of all kinds (as of materials) received by an animal during a given period is given back by it during that period, his condition at the end of the term being the same as at the beginning".⁸⁸

In Carpenter's Royal Society paper we see how several ideas, only tentative in his previous writings, were now explicit. He emphasized this in a footnote on the first

⁸⁵ *Ibid.*, pp. 751–752.

⁸⁶ W. F. Edwards, *On the influence of physical agents on life*, translated from the French by Drs. Hodgkin and Fisher, London, S. Highley, 1832. Cited by Carpenter, *ibid.*, p. 754. In addition to this well-known treatise, Carpenter almost certainly had read one or more of the following papers by Edwards: (i) 'Mémoire sur l'asphyxie, considérée dans les Batraciens', *J. Méd.*, 1817, 40: 308–326; *J. Phys.*, 1817, 85: 149–157. (ii) 'Seconde mémoire sur l'asphyxie. De l'influence de la température dans l'asphyxie des Batraciens', *Annls Chim.*, 1818, 8: 225–241. (iii) 'Troisième mémoire sur l'asphyxie. De l'influence de l'air contenu dans l'eau sur la vie des Batraciens qui y sont plongés', *ibid.*, 1819, 10: 5–29. (iv) 'Note sur les contractions musculaires produites par le contact d'un corps solide, avec les nerfs, sans arc galvanique', *Annls Sci. nat.*, 1825, 5: 51–62; Thomson, *Ann. Phil.*, 1825., 10: 470–471.

⁸⁷ Jørgen M. C. Schiödte, *Specimen Faunae Subterraneae, bidrag til den underjordiske fauna*, Copenhagen, Trykt hos Kgl. hofbogtrykker B. Luno, 1849. Cited by Carpenter, *op. cit.*, note 62 above, p. 754.

⁸⁸ *Ibid.*, p. 756.

page, stating that the mutual relations of vital and physical forces had occupied his attention for several years, at least since his 1848 review of Matteucci's *Lectures*. In that footnote he also cited a paper by Dr. Richard Fowler, entitled 'If vitality be a force having correlations with the forces, chemical affinities, motion, heat, light, electricity, magnetism, gravity, so ably shown by Professor Grove to be modifications of one and the same force'⁸⁹ (1849), in which Fowler had reached similar conclusions to him and Matteucci. Carpenter declared that he had not known of Fowler's work until an abstract appeared in the *British Association for the Advancement of Science Report* for 1850, and he therefore claimed priority. Besides, he also claimed priority for developing the topic systematically, which would "give it a claim to the consideration of physicists and physiologists, such as it scarcely deserves from the treatment which it has received from Dr. Fowler".⁹⁰

Interestingly, Carpenter did not mention Helmholtz in his paper, even though he discussed that aspect of muscle contraction to which Helmholtz's research in the mid 1840s had contributed so much and from which Helmholtz's own ideas on energy conservation derived. Apparently, he was unaware of Helmholtz's paper 'Über die Erhaltung der Kraft' (1847) which has become the *locus classicus* for the enunciation of the conservation of energy and on which historians naturally have focused. Indeed, it is highly likely that he did not know of that paper as it was barely known, and even less appreciated, in Germany itself, and had not been translated. Without a doubt, then, the figure who most influenced him on conservation was Liebig, although he himself was more interested in the correlation theory, where Grove was his mentor.

What was the impact of the Royal Society paper? This is an issue yet to be explored in greater depth than I have been able to do, but we get an impression from the lengthy review of it which appeared in the *British and Foreign Medical Review*. We have already seen one paragraph from that review, where three others—Drs. Fowler and Radcliffe and Mr. Newport—were acknowledged for their work on the same theme. The reviewer was impressed by Carpenter's argument and emphasized those points where Carpenter had departed significantly from other physiologists, for instance on whether external physical agents are mere stimuli or whether they really participate in the dynamics, the very mechanism, of life. The reviewer agreed with Carpenter's assertion that his view, if correct, would ". . . afford a precision to physiological doctrines, which they have never before possessed; and to open out a vast number of new lines of inquiry, which promise an ample harvest of results, not only valuable in a scientific point of view, but likely to be fertile in applications to various departments of the therapeutic art. At any rate, it is very important that physiological science should be considered under the same dynamic aspect as that under which the physical sciences are now viewed by the most enlightened philosophers . . ." ⁹¹

The reviewer also discussed another essay of the same ilk he had recently noticed,

⁸⁹ R. Fowler, 'If vitality be a force having correlations with the forces, chemical affinities, motion, heat, light, electricity, magnetism, gravity, so ably shown by Prof. Grove to be modifications of one and the same force', *Rep. Br. Ass. Advmt Sci.*, 1850, pp. 77–78 in transactions of the sections.

⁹⁰ Carpenter, *op. cit.*, note 62 above, p. 727.

⁹¹ *Ibid.*, p. 757. Cited by the reviewer, *op. cit.*, note 9 above, p. 236.

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Mayer's 'On organic movement in its relations to material changes'.⁹² He was sure that Carpenter and Grove had not known of it whilst formulating their ideas on correlation; and although all three proposed essentially the same theory, in the reviewer's opinion, there were enough differences for them to be regarded as complementary to one another. For instance, Mayer "does not enter, like Dr. Carpenter, into an analysis of the phenomena of Growth and Development; but fixes his attention rather on the production of Heat, Light, Electricity, and (above all) Motion, by living bodies."⁹³

It is noteworthy that although the reviewer mentioned all three as asserting a conservation principle, the main principle that they conveyed to him and that he considered most important was correlation. He saw even Mayer's essay as an exposition essentially of correlation.

Curiously, despite this glowing review and the later acknowledgements by men like Balfour Stewart and Rutherford, it seems that the Royal Society paper did not have much immediate impact. J. E. Carpenter wrote that "it made little impression at the time. Its line of argument, however, secured more and more attention, and its conclusions were finally accepted as a part of the general doctrine of the conservation of energy. . . ."⁹⁴ Clearly, a more detailed study of its reception would be worthwhile.

CARPENTER'S PHYSIOLOGY AFTER 1850

Carpenter next used the correlation theory in the third edition of *General and comparative physiology*⁹⁵ (1851). There, four points were notable in his ideas on force:

- (1) Forces and their laws were employed much more than in previous editions.
- (2) He discussed more fully the theological implications of correlation and conservation, being especially concerned whether "force" and "law of nature" expressed realities or were only convenient labels. On this he quoted Locke, meaning by "force" only "affections of matter" which, as Locke had asserted, were the direct operation of the primal, all-sustaining First Cause; so far as human understanding could go, they were ultimate facts, their regularity of action indicating only one thing more fundamental, namely the constancy of God's will.⁹⁶ On this basis, "vital forces" for Carpenter were in exactly the same league as physical and chemical forces.

This natural theology of force should not surprise us. As one scholar recently wrote, the principles of the correlation and conservation of force together "expressed a clearly-articulated theology of nature, in which divine providence was seen to be manifested in the wisdom and foresight by which God had established cosmic order in the indestructibility of forces. . . . For Faraday it provided a framework for research; for Grove it was a fundamental doctrine which he wished to explicate and popularize; while for Joule it provided a framework for the interpretation of his experimental discoveries".⁹⁷ To this, we may add that for Carpenter it provided a

⁹² J. R. Mayer, *Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel. Ein Beitrag zur Naturkunde*, Heilbronn, Verlag der C. Drechlerschen Buchhandlung, 1845.

⁹³ Op. cit., note 9 above, p. 237.

⁹⁴ J. E. Carpenter, op. cit., note 10 above, p. 54.

⁹⁵ W. B. Carpenter, *Principles of physiology, general and comparative*, London, J. Churchill, 1851.

⁹⁶ *Ibid.*, p. 85 and elsewhere.

⁹⁷ Heimann, op. cit., note 4 above, p. 149.

platform on which physiology could stand beside physics and chemistry as a true science.

It is perhaps no coincidence that between the second edition of *General and comparative physiology*, where there was scarcely any natural theology of force, and the third edition where it was conspicuous, Carpenter underwent a religious renewal. He had never lost his Unitarian faith; indeed, with a father such as he had it would have been difficult to do so, but from about 1846 he found a new and exceptionally congenial spiritual home at Rosslyn Hill Church, Hampstead, where for the next seventeen years he was organist. At the same time he began to read the German biblical scholars—Strauss's *Life of Jesus* and Gföerer's *Origin of Christianity* in particular. He also became deeply interested in St. John's Gospel.⁹⁸ It is therefore highly probable that the new quasi-theological passages in his physiological writings, particularly those on the correlation of forces, represented profound aspects of his general world-view; they were not mere glosses to enliven dry science.⁹⁹

(3) Carpenter discussed the primary role of the sun in the dynamical system of the world. One likely reason for this was that he had read and was quoting John William Draper's (1811–1882) recent treatise on *Forces which produce the organization of plants*¹⁰⁰ (1845). Draper was another who was deeply interested in dynamical relations among (a) physical forces; and (b) physical forces and life.¹⁰¹ It is unlikely that Carpenter had read Draper's book before writing the second edition of his *Physiology*.

(4) He now explicated respiration in clear dynamical terms: respiration was the means of replenishing an animal's powers and maintaining its temperature. He thus regarded it not primarily as a chemical reaction but rather as a dynamical interchange, as an issue in physics rather than in chemistry.¹⁰²

The next stage in Carpenter's discussion of power was an essay on 'The phasis of force'¹⁰³ (1857), one of the most metaphysical of his scientific writings. Again, there was no explicit assertion of conservation, although it was implicit. Much of the essay dealt with Liebig's chemical-physiological ideas on force; indeed, the following synopsis could easily apply to large sections of Liebig's *Animal chemistry*. Carpenter suggested that, apart from animal heat, the most characteristic means whereby animals restore force to their environment is by movement. Movement being an expression of force, whence came that force in the animal organism? The answer, he said, lay in Liebig's assertion that every muscle contraction involves the death and oxidation of a quantity of muscle proportional to the force it exerts. The motion produced by each contraction could be considered an expression of the so-called vital force which is superseded by chemical force, having a similar relation

⁹⁸ These details are from J. E. Carpenter, *op. cit.*, note 10 above, pp. 33–46.

⁹⁹ A point made by J. E. Carpenter throughout his introductory memoir.

¹⁰⁰ J. W. Draper, *A treatise on the forces which produce the organization of plants*, New York, Harper, 1845. Cited by Carpenter on p. 46. Draper developed his dynamics considerably in his later book, *Human physiology, statical and dynamical, or the conditions and course of the life of man*, London, Sampson Low, 1856.

¹⁰¹ I have examined Draper's ideas on force in chapter 8 of my thesis, *op. cit.*, note 4 above.

¹⁰² Carpenter, *op. cit.*, note 95 above, p. 764.

¹⁰³ W. B. Carpenter 'On the phasis of force', *National Review*, April 1857, reproduced in full by J. E. Carpenter, *op. cit.*, note 10 above, pp. 173–184.

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to that chemical force as a voltaic current has to the oxidation of zinc in a battery.¹⁰⁴ As had Liebig, he proposed that the nitrogenous constituents of plants and animals possess large concentrations of chemical force which, on being released, manifest themselves as fermentations, motion, heat, and other physiological phenomena. Just as Liebig regarded the death and oxidation of living tissue as supplying an organism with power to generate heat and motion, so did Carpenter regard nervous power: “. . . its source lies, like that of muscular power, in the chemical changes involved in the death and decomposition of the peculiar tissue which manifests it”.¹⁰⁵

In this essay Carpenter still admitted the reality of both matter and force; but force seemed to be growing in stature, for it was the paradigm which most promised a unified comprehension of all natural phenomena. Carpenter also believed that it would enable him to construct a physiology of the mind as well as of the body. He wrote: “We shall have greatly failed in our purpose, however, if we have not by this time led our readers to perceive how complete is the distinction between *matter* and *force*, and close is the relation between *force* and *mind*. Matter is in no case more than the embodiment or instrument of force; all its so-called active states being merely the manifestations of an energy which, under different forms, is unceasingly operative.”¹⁰⁶

In the sixth, much revised edition of his *Human physiology*¹⁰⁷ (1864) the conservation of energy was at last discussed explicitly. This edition was edited by his young physiologist friend, Henry Power (1829–1911), who rewrote almost the entire book to keep down its length. The first two chapters, however, were Carpenter’s own original composition, and chapter I, ‘Of life and its conditions’, was essentially the physiological application of the correlation and conservation principles, in which his earlier ideas became explicit. He discussed those philosophers, besides himself, whom he considered had made original and seminal contributions to those principles, namely Liebig, Mayer, Grove, and Joule. In a section on the origins of nervous and muscular forces Carpenter appended two noteworthy footnotes. One enumerated again the architects of the correlation and conservation principles, but this time conservation was spoken of as a distinct theory in its own right.¹⁰⁸ The other footnote mentioned later, less original contributors to the theme, among whom was Helmholtz.¹⁰⁹ In the text Carpenter argued that animal life entails a continuous expenditure of motor force, heat, and, in the case of man, psychic power, and that these derive from chemical force stored in the body’s tissues and food. Again, Liebig was acknowledged as the originator of this view, although he felt that Liebig’s ideas were being superseded:

. . . he seems to have regarded the motor-force produced as the expression of the vital force by which the tissue was previously animated; and to have looked upon its disintegration by oxygena-

¹⁰⁴ See especially Part 1 of Liebig’s *Animal chemistry*. This discussion occurs on p. 180 of Carpenter’s paper in *Nature and Man*.

¹⁰⁵ Carpenter, *op. cit.*, note 103 above, p. 181.

¹⁰⁶ *Ibid.*, pp. 182–183.

¹⁰⁷ W. B. Carpenter, *Principles of human physiology*, 6th ed., London, J. Churchill, 1864.

¹⁰⁸ *Ibid.*, p. 14.

¹⁰⁹ *Ibid.*, p. 15.

tion as simply a consequence of its death. The doctrine of the 'correlation of forces' being at that time undeveloped, he was not prepared to recognize a source of motor power in the ulterior chemical changes which the substance of the muscle undergoes; but seems to have regarded them as only concerned in the production of heat. The earliest distinct expression of the current doctrine is to be found in the very remarkable treatise of Dr. Mayer, in which he worked out from the two fundamental axioms "*Ex nihilo nil fit*" and "*Nil fit ad nihilum*", the whole system of doctrine which has since come to be known as that of the 'correlation of forces' and the 'conservation of force', in its application alike to physics and chemistry and physiology. Prof. Grove was simultaneously engaged in the development of the doctrine of the 'correlation of the physical forces'; and without any knowledge of Dr. Mayer's previous labours, the author of this treatise developed the doctrine in the form stated in the text, in his memoir 'On the mutual relations of the vital and physical forces', published in the *Philosophical Transactions* for 1850.¹¹⁰

This passage is important for the historian of mid-nineteenth-century dynamics, particularly *vis-à-vis* physiology, since it illustrates both the simultaneity of various expressions of the force-principles as well as their differences. From the point of view of physiology, it is useful since it reveals the extent, as well as the limitations, of Liebig's contribution. It also shows how Carpenter considered the primary principle to be correlation, with conservation only a special aspect of it, whose explicit announcement occurred in the metaphysical work of Mayer. This supports my contention that Carpenter's idea of correlation entailed a rough awareness of conservation, and that the former made rigorous sense only if there was an implicit admission of the latter, or at least of the non-creatability of force. Carpenter apparently thought these two principles so intimately related that they were two sides of the same coin. Only thus can one explain his lack of enthusiasm for conservation *per se*, for he regarded it as only a slight modification of Grove's more empirical, less speculative, principle of correlation. In his opinion, the latter was the result of extensive experimentation and careful reflection by eminent practising scientists like Liebig, Grove, and Matteucci, not to mention himself, whereas conservation was the metaphysical brainchild of an obscure medical practitioner—albeit a remarkable brainchild, as he readily admitted.

The last of Carpenter's works I shall discuss is his Presidential Address to the British Association for the Advancement of Science in 1872, entitled 'Man the interpreter of Nature'.¹¹¹ Departing from the custom for the president to review developments in a particular science, he preferred to give a general philosophic lecture whose main concern was to check the growing belief that, through science, man might fully understand Nature and comprehend her absolute laws. Such a reaction against the growing confidence of science was not new,¹¹² but what was original was that it arose from his own impeccable scientific background and from his own acquaintance with methodological and philosophical difficulties, particularly in his own science of physiology. Carpenter distinguished between two types of scientific law: one was only a generalization of phenomena and was therefore not a proper law; such were the so-called laws of chemical combination. The other was a true law since it described

¹¹⁰ Ibid., p. 14, footnote.

¹¹¹ W. B. Carpenter, 'Man the interpreter of Nature', *Presidential address to the 42nd meeting of the British Association for the Advancement of Science, Brighton, 1872*, reprinted from the Association's *Report*, London, J. Murray, 1873. Reproduced in full by J. E. Carpenter, *op. cit.*, note 10 above, pp. 185–210.

¹¹² See, for instance, B. Willey, *Nineteenth century studies*, London, Chatto & Windus, 1949.

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the effects of a force, and force was an ultimate fact—the laws of gravitation and energy conservation (which he attributed solely to Mayer) were therefore real laws. Since the only bases of natural laws were the forces or powers whose effects they described, man's greatest task was to study those powers. Carpenter's own studies had led him to realize the unity of Nature and God's ever-active power; they gave him a world-view which reconciled the highest discoveries of science with his deeply ingrained theology. So, with evident approval he quoted Pope:

All are but parts of one stupendous whole,
Whose body Nature is, and God the soul.¹¹³

These themes were developed in a new work of his, the *Principles of mental physiology*¹¹⁴ (first edition, 1874). There, he especially intended to extend his ideas on forces, particularly the correlation principle, to the study of mind; and if the frequency of successive editions is any measure, his book fulfilled an immediate need in Britain with considerable success. However, this and his later interests, such as his study of spiritualism, hypnotism, etc., lie beyond the scope of this paper.

CONCLUSION

This paper has been an attempt to rehabilitate Carpenter in the history of physiology, specifically with regard to his application of the correlation of forces and, rather incidentally, the conservation of power to the living organism. I have discussed his work into the 1870s to show that his early ideas on force, as they appeared in his physiological writings in the 1830s and early 1840s, were not casual dreams but did constitute a definite though roughly hewn conception of the essential unity and rigorously quantitative interconversion of forces. Moreover, as his physiological textbooks were revised and as new evidence came in, especially from the Continent, we see how his correlation theory became more rigorous and useful and resulted eventually in the explicit acknowledgement of the conservation of energy, which had been only implicit in his earlier work.

This study of Carpenter supports Kuhn's contention that "previously separate problems were gaining multiple interrelationships" from the 1830s on, and that this new feature "proved to be a major requisite for the emergence of energy conservation".¹¹⁵ This study has been a preliminary attempt to examine that phenomenon, namely the different fragments of a simultaneous discovery, from the point of view of a physiologist. I suspect that much remains to be done along this line by medical historians.

¹¹³ Carpenter, *op. cit.*, note 111 above, p. 209.

¹¹⁴ W. B. Carpenter, *Principles of mental physiology*, London, H. S. King, 1874.

¹¹⁵ Kuhn, *op. cit.*, note 1 above, p. 324.