Medical History, Supplement No. 1, 1981: 121–134

MEDICAL SCIENCE, MEDICAL PRACTICE, AND THE EMERGING CONCEPT OF TYPHUS IN MID-EIGHTEENTH-CENTURY BRITAIN

by

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IN THE current medical nomenclature of the English-speaking world the term *typhus* refers to a small group of acute febrile diseases characterized by central nervous system involvement and skin eruptions, caused by rickettsial infection; while in several European languages the word *typhus* persists as a larger generic term to describe many symptomatically related diseases including typhoid fever. The narrower meaning attached to the word *typhus* reflects a historical development among English-speaking physicians during the last century and a half, when a large number of investigators gradually worked out the distinctive clinical, pathological, and aetiological features of louse-borne typhus fever. They were strongly influenced by the concept of typhus common in British medical circles in the first quarter of the nineteenth century.

At the beginning of the nineteenth century *typhus fever* or simply *typhus* was one of the most extensively studied diseases. The New England physician Nathan Smith, in his 1824 essay on typhous fever, and the French clinician Pierre Louis, in his monumental 1829 study of the *fièvre typhoide* both saw themselves as contributing to a familiar and long-standing tradition of studies on typhus. This study, primarily a British tradition, was much indebted to Scottish practitioners, including Alexander Tweedie, William Alison, John Armstrong, and Philips Wilson [Philip], all in turn heavily dependent on Cullen's fever theory and nosography.

William Cullen (1710–1790)[1] first published his ideas on typhus in 1769 in an outline of nosography prepared for the use of his medical class at the University of Edinburgh[2] and then elaborated them fully in his textbook, *First lines of the practice of physic*, which appeared in four volumes between 1777 and 1784.

Cullen's use of the word *typhus*, like his whole venture into nosography, was a result of his reading François Boissier de la Croix de Sauvages's classification of diseases,

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This publication was supported in part by NIH grant LM 03245 from the National Library of Medicine.

[1] John Thomson, *An account of the life, lectures, and writings of William Cullen, M.D.*, 2 vols., Edinburgh and London, William Blackwood, 1859.

[2] William Cullen, *Synopsis nosologicae methodicae*, Edinburgh, 1769. I have used the edition in John Thomson (editor), *The works of William Cullen, M.D.*, 2 vols., Edinburgh, Blackwood; London, Underwood, 1827.

probably the 1763 Amsterdam edition of *Nosologia methodica sistens morborum classes*. Cullen first showed an interest in nosography in 1764 when he wrote to his pupil David Millar, "Let me know if anybody at London has read Sauvages's Nosologia Methodica; or if anybody enters into such a plan, or approves of others doing it", and from January 1765 this book played a prominent role in Cullen's clinical teaching.[3] The Latin word *typhus* is occasionally found as a transliteration of the original Hippocratic Greek, but as a form of fever it was introduced into modern Latin by Sauvages in 1759.[4] Cullen acknowledged his debts not only to Sauvages but to Carl von Linné [Linnaeus] (1707–1778) and Rudolph August Vogel (1724–1774) whose nosographies he also used in compiling his own, but the diseases Cullen described were related only vaguely to those of other nosographers.

Cullen's discussion of fevers was part of mid-eighteenth-century British medical practice. He divided fevers into periodic and continued, further sub-dividing the latter into three genera – *Synocha*, *Typhus*, and *Synochus*. All three terms were derived from ancient medical writings, but he wrote that the: ". . . distinction is the same with that of fevers into Inflammatory and Nervous, the distinction at present [mid-1770s] most generally received in Britain. To the first, as a genus, I have given the name of Synocha; to the second, that of Typhus; and little studious whether these names be authorized by the ancient use of the same terms, I depend upon their being understood by the characters annexed to them in our Nosology, which I apprehend to be founded on observation."[5] *Synochus*, the ancient term for continued fever, was used to specify those fevers of a complex or mixed type neither purely inflammatory nor purely nervous.

Cullen had developed and taught very similar ideas early in his career, presumably under the names inflammatory and nervous, because in an 1811 reminiscence of his 1746 lectures on medicine in Glasgow, Dr. Robert Wallace, Cullen's student, recalled that: "in his lectures he delivered the same opinion with regard to the Theory of Fever, the Humoral Pathology, and the Nervous System, which have since appeared in his writings."[6] Wallace's recollection, while perhaps not completely accurate, suggests that a full understanding of Cullen's concept of typhus requires an examination of the mid-eighteenth-century British tradition on fevers and particularly the distinction between inflammatory and nervous fevers.

Most eighteenth-century physicians who contributed to medical discussions usually sought to define disease in such a way that it might be treated satisfactorily. A few physicians, with research interests akin to those of the taxonomist, may have been solely interested in differentiating diseases as a scientific problem. For the latter group the long-term benefits to humanity to be obtained from an accurate classification of diseases were sufficiently important that the immediate needs of practice might be ignored. But for most practitioners, the urgent needs of patients have always been the

[3] Thomson, op. cit., note 1 above, vol. 2, pp. 3–4.

[4] *Oxford English dictionary*, s.v. typhus. The word was first used in Sauvages's *Pathologia methodica, seu de cognoscendis morbis*, Leyden, 1759, which Sauvages viewed as the first edition of his *Nosologia methodica* of 1763 ff.

[5] William Cullen, *First lines of the practice of physic* in Thomson, op. cit., note 2 above, vol. 1, p. 517.

[6] Thomson, op. cit., note 1 above, vol. 1, p. 25.

most dominant factor in their studies. The essential medical questions were ultimately therapeutic.

The history of medicine, however, is not simply the history of therapeutics, for theories of pathology have always helped to shape therapeutic efforts. Similarly, anatomy, physiology, and chemistry, which provide an understanding of the normal body function, played a key role in shaping pathology. Yet medicine is ultimately the encounter of the physician and the patient and the perceived success or failure of that encounter determined both the societal and professional judgment of medicine.

The question of what to do to restore the patient to health was always pressing, but particularly so when the physician confronted the acute infectious diseases so dominant in the eighteenth century. Malaria was endemic throughout Europe. Bubonic plague had ravaged Europe repeatedly for 350 years. Smallpox swept through villages and towns in devastating epidemics and was a common childhood disease in cities. Many diseases only identified later – typhoid, epidemic typhus, meningitis, influenza – were probably both endemic and occasionally epidemic.

Each culture sees disease as a complex pattern of symptoms. Eighteenth-century practitioners attempted to understand disease and its treatment in the light of the dramatic scientific and social changes of the seventeenth century. For most, Harvey's discovery of the circulation of the blood stood paramount. Its importance was not strictly therapeutic but, by challenging Galenic physiology, it called into question the pathological assumptions which underlay medical practice. In addition, medicine's pathological and therapeutic foundations were also challenged by the new scientific interests in chemistry and natural philosophy.[7]

The theoretical turmoil caused by new scientific discoveries may have played a role in the social conflict in which the physician's professional control of medical practice and consequently his economic security were challenged by chemists, empirics, apothecaries, surgeons, and other groups, each attempting to improve its own social position. Theoretical uncertainty in medicine was used as a weapon by some spokesmen of such groups, and orthodox physicians responded by reformulating medical theory in terms of the latest scientific ideas.[8] Yet, despite questions and challenges, the efficacy of much of the traditional materia medica and therapeutic procedures continued to be accepted by the medical profession and society.

Many of the procedures of seventeenth-century medicine had been used since the time of Hippocrates; their value appeared to have been confirmed in each generation. Such new additions to the materia medica as heavy metals were eventually explained in terms of the new chemical theory. Medicinal plants from the New World, particularly tobacco and Peruvian bark, were similarly introduced. Cinchona bark, containing the vegetable alkaloid quinine, had an effect on some fevers of malarial origin: an effect so dramatic as to suggest to some practitioners that its greatest advantage was diagnostic.[9] Fevers which responded to treatment with cinchona bark

[7] Audrey B. Davis, *Circulation physiology and medical chemistry in England 1650–1680*, Lawrence, Kansas, Coronado Press, 1973.

[8] Theodore M. Brown, 'The College of Physicians and the acceptance of iatromechanism in England, 1665–1695', *Bull. Hist. Med.*, 1970, **44**: 12–30.

[9] Richard Morton, *Pyretologia*, London, S. Smith, 1694; and Francesco Torti, *Therapeutice specialis ad febres . . .*, Modena, B. Soliani, 1712.

could be distinguished clearly from those which did not.

The new natural philosophy of the seventeenth century also exerted positive influences on medicine by its stress on personal observation and experimentation. The humanistic tradition of the Renaissance had made widely available texts of the Hippocratic Corpus. In particular the *Epidemics*, especially Books I and III, provided models of original observation on the natural history of disease, whose importance was reiterated by Guillaume de Baillou at Montpellier, Thomas Sydenham in London, and Giorgio Baglivi in Italy.

Although fever had been seen as an essential unity, by the early eighteenth century various disease experiences, particularly devastating epidemics, had steadily undermined this unity. Erwin Ackerknecht has suggested that the effort to divide fever was the result of the Black Death. Certainly plague played a role, but apart from plague the concept of fever remained primarily unified into the second half of the seventeenth century. In his first book of 1666, Thomas Sydenham was looking for a method of curing fever, and while he recognized some differences and variations among fevers he does not seem to have considered these too important. Ten years later, in the third edition of his great work, careful observation had led him to see significant differences among various fevers, especially those associated with epidemic experiences of plague and smallpox. Sydenham believed that differences among fevers would and should affect therapy, and he developed the concept of the epidemic constitution of the atmosphere at least in part to explain observed differences among fevers in successive years.[10]

Sydenham's explanation of the differing character of diseases ran contrary to the new scientific criteria, and at least initially the concept of the epidemic constitution seems to have exerted little influence on medical thought. While there were important seventeenth-century attempts to construct a new theoretical framework within the limits of the new sciences, which would explain the seeming unity and the differences among fevers, all such attempts were generally overshadowed by the great heuristic synthesis of Herman Boerhaave (1668–1738). In 1701, Boerhaave joined the medical faculty at Leyden, where he eventually held four professorships and dominated early eighteenth-century European medical teaching. He fully appreciated the natural history tradition of Hippocratic medicine and expressed repeatedly his indebtedness to and admiration of Thomas Sydenham. As part of the developing neo-Hippocratism of the eighteenth century Boerhaave taught clinical medicine at the bedside in St. Caecilia Hospital in such a manner as to inspire his students to become hospital physicians and clinical teachers in their own right. He was also a part of the iatrophysical tradition which applied the theories of seventeenth-century mechanics and chemistry to medicine in the belief that the body was of the same basic nature as the physical world. He was also deeply influenced by continuing developments in anatomy and physiology during the later seventeenth century.[11]

In the seventeenth century, anatomists had discovered a number of glands which

[10] Donald Bates, 'Thomas Sydenham: the development of his thought, 1666–1676', Ph.D. thesis, Johns Hopkins University, Baltimore, 1975.

[11] G. A. Lindeboom, *Herman Boerhaave, the man and his work*, London, Methuen, 1968.

possessed ducts as well as some which were seemingly part of the circulatory system itself. The glands appeared to be constructed of tubes and bladders united together, and it was frequently believed that their actions could be explained using the mechanics of fluids in tubes and bladders. Thus around 1700 the vascular supply of these glands was a topic of intensive study. Of particular importance in these investigations were the new techniques of microscopy and injection developed especially by Frederick Ruysch, the anatomist at Amsterdam.

Boerhaave saw the quickened pulse as the single most common element in all fever and one that had to be explained by the physiology of the circulatory system. He suggested that for a wide variety of reasons the blood might stagnate in the vessels and stagnation might produce an abnormal stimulation which either directly or indirectly would affect the heart so as to increase the pulse rate. The chill often associated with fever was due to the stagnation of the blood in the vessels, the hot stage of fever to the violence of the blood flow following the quickened pulse.[12] For Boerhaave stagnation at, or in association with, the malfunction of a gland was only one of many possible causes of febrile action, but for a large number of early eighteenth-century British physicians the new glandular physiology was so impressive as to overshadow other possibilities almost completely.

In 1701 George Cheyne (1671–1743), for example, combined a glandular physiology with Newtonian mechanics to advance the "General Proposition" that: "The general and most effectual cause of all Fevers, is the Obstruction or Dilation of (the complicated *Nerve* and *Arterie*, the *excretory Duct and Conservatory*, one, or rather all these; which, as shall be afterward shown, make up) the *Glands*, and they receive their Denomination, as these, or those *Glands* are more or less obstructed or dilated."[13]

A contemporary of Cheyne's, Sir John Floyer (1649–1734) of Lichfield, attempted to unite the glandular fever pathology and neo-Hippocratism by using the glandular specificity to explain fever differences. The blood would be obstructed or stagnated at a particular gland and as a result the balance of the secretions would be disturbed; that is, the humoral balance would be disturbed. A secretion or humour might be corrupted, or produced either to excess or in insufficient quantities, so that the possible variations among symptoms became quite vast. Changes in the humoral content of the blood naturally influenced the circulation and so could explain the important differences in the pulse used by Floyer as diagnostic criteria in fevers.[14] In 1726 he attempted to illustrate how the clinical observations recorded in the *Epidemics* could be explained by reinterpreting Hippocratic humoral pathology in terms of modern glandular pathology.[15]

[12] This brief summary cannot do justice to the complexity of Boerhaave's ideas on fever as recorded in his *Aphorisms* and in G. van Swieten's commentaries. A slightly more elaborate summary of Boerhaave's medical theory can be found in Lester King, *The medical world of the eighteenth century*, Chicago, University of Chicago Press, 1958; and in Lindeboom, op. cit., note 11 above.

[13] George Cheyne, *A new theory of acute and slow continued fevers*, 3rd ed., London, G. Strahan, 1722, p. 46.

[14] John Floyer, *The physician's pulse watch*, 2 vols., London, S. Smith & B. Walford, 1707.

[15] John Floyer, *A comment on forty two histories described by Hippocrates in the first and third books of his Epidemics*, London, J. Isted, 1726.

From *Epidemics* III he cited the following case:

> The Phrenetic, who was sick, on the first Day Vomited many thin aeruginose Humours, with a *Fever* and Horrour, and much Sweat all over, with heaviness of his Head, and Neck, and Pain, the Urine was thin, and a few clouds dispersed, which did not subside, with many Stools at once; he was very Delirious, and without Sleep; on the second in the Morning he was Speechless, the *Fever* Acute, he Sweat, no Intermission, Palpitations all over, and Convulsions in the Night; on the third Day all Symptoms worse, and he died on the fourth.[16]

Floyer's interpretation was that the blood had become choleric, producing the febrile response, and was stopped in the head producing the phrensy. The thin urine proved the febrile matter was not contained in the circulating blood while the vomit of aeruginose humours suggested that the source of the febrile matter was an inappropriate mixture of the digestive secretions. The indicated therapy – "Bleeding in the Arm, Neck, or Forehead, and Cupping were necessary" – would relieve the stoppage, while clysters could control the digestive disturbance.[17] Regardless of the exact nature of the presumed glandular problem, as long as obstruction and stagnation were seen as the underlying cause of the variations in the pulse, bleeding and other antiphlogistic measures became indicated as the best therapeutic approach. They were intended to relieve the obstruction or stagnation and would control the quickened pulse.

Almost a century ago, Charles Creighton (1847–1927) observed that the first half of the eighteenth century was a remarkably prosperous period in British history, and against a historical background of general prosperity three striking fever epidemics, 1718–19, 1727–29, and 1740–42, stood out in sharp relief as associated with the three harvest failures and resulting economic distress. Creighton naturally assumed that typhus and relapsing fever played an important role in these epidemics but acknowledged that other diseases also seemed to have played a role and that the question was "more than usually perplexing".[18] Given the vast difficulties of retrospective diagnosis and the differing nature of the symptoms considered significant by eighteenth-century practitioners, we cannot now decide what diseases were in fact being discussed, and we must allow the eighteenth century to speak for itself.

During the first epidemic period of 1718–19, the glandular pathology associated with seventeenth-century anatomy and physical science dominated medicine, but by the 1720s medical concepts had begun to change. As English-speaking students of Herman Boerhaave returned to Britain they brought with them a new respect for the Sydenham tradition of clinical observation. Clinically trained in the neo-Hippocratism of Boerhaave, they possessed a keen appreciation of the theoretical value of physiology as well as of its limits when applied to pathology and therapeutics.[19]

A Boerhaave student who observed the fever of the late 1720s in England was Dr.

[16] Ibid., p. 94.
[17] Ibid., pp. 94–95.
[18] Charles Creighton, *A history of epidemics in Britain*, 2 vols., Cambridge University Press, 1894, vol. 2, pp. 60–67. (Reprinted, with additional material by D. E. C. Eversley, E. A. Underwood, and Lynda Ovenall, 2 vols., London, Frank Cass, 1965.)
[19] E. A. Underwood, *Boerhaave's men at Leyden and after*, Edinburgh University Press, 1977.

William Hillary (1697–1763), who in 1722 graduated from Leyden and settled at Ripon in Yorkshire where from 1726 to 1734 he recorded meteorological observations and their associated diseases. Afterwards Hillary lived in Barbados and finally in London. His 1759 publication on the diseases of the West Indies, particularly yellow fever, was extremely influential and continued to be reprinted for over half a century.[20]

In his account of diseases at Ripon Hillary noted that the fever of 1727 was particularly deadly among the poor. He wrote, "Nor did any other method, which art could afford, relieve them: insomuch that many of the little country towns and villages were almost stripped of their poor people, . . . Bleeding, pectorals with volatiles, and antiphlogistic diluters and blistering, were the most successful."[21] In his account of diseases in 1728, Hillary, relying on his 1727 experience, reported that he began his therapeutic efforts with bleeding but soon had to change his practice as he observed that very few of his patients, "even the strong and robust", could stand the loss of even one-third the amount of blood he had commonly drawn in the previous year. Many patients could not tolerate even a single bleeding. He reported, "the first bleeding often sunk the pulse and strength of the patient so much that I durst not repeat it more than once, and in some not at all."[22]

Hillary commented on symptomatic differences he observed – there was clearly at several points in the epidemic the traditional synochus or continued fever marked with petechiae similar to fleabites. The common denominator throughout the epidemic was the occurrence of central nervous system symptoms: "nervous twitchings", some patients "were comatous", others "had tremors", even "the young and robust, who had more full pulses, were generally delirious". Hillary described many cases as low or faint, and by 1729 he had identified a slow fever which seemed to last much longer than usual, some cases as long as thirty days, leaving the patient greatly debilitated.[23]

At Plymouth, Devonshire, another Boerhaave student, John Huxham (1692–1768), was following a similar course of researches. Huxham had studied at Leyden in 1715 but received his M.D. from Rheims. In 1739 he published his observations on fevers at Plymouth, for which he was elected a Fellow of the Royal Society. Huxham described a particularly slow fever, associated with central nervous system symptoms, as quite prevalent in the autumn of 1727. This slow fever he contrasted with a putrid fever which came on much more rapidly and became epidemic in the mid-1730s. Huxham also began to question the value of bleeding in the particularly slow fever, but was apparently not so positive as Hillary about the therapeutic differences among fevers.[24]

During the 1730s the differences observed by Hillary, Huxham, and others during the epidemic fevers of the late 1720s became incorporated into medical theory. This change was eased by the developing interest in the physiology of the nervous system. The anatomists and physiologists of the seventeenth century had studied the nervous system as well as the circulation. Thomas Willis and Francis Glisson were particularly

[20] *Dictionary of national biography*, s.v. William Hillary.

[21] William Hillary, 'An account of the principal variations of the weather and the concomitant epidemical diseases from 1726 to 1734 at Ripon', bound with his *Essay on the smallpox*, London, G. Hitch, 1740, p. 16.

[22] Ibid., p. 26.

[23] Ibid.

[24] John Huxham, *Observationes de aëre et morbis epidemicis 1728–1737*, London, S. Austen, 1739.

interested in neurological questions. Giorgio Baglivi had applied neurophysiology to the explanation of disease and Boerhaave's physiology often depended upon mechanical actions conducted by the nerves. Friedrich Hoffmann placed even more stress on the pathology of the nerves. During the early eighteenth century, diseases of the nervous system achieved greater prominence in English medicine. Cases of spleen, vapours, lowness of spirits, hysteria, hypochondria, palsy, apoplexy, and epilepsy were explained as malfunctions of the animal spirits or nervous juice – interpreted as manifestations of neuropathology in the same way that inflammation and fevers were related to circulatory pathology. George Cheyne's 1733 monograph explained that nervous diseases were the result of the affluence of the English, who because of their wealth and ease were not observing a proper regimen. Improper regimen resulted in maladjustments of the vital fluids, possibly resulting in a nervous fever.[25] The nervous fever was prone to attack those of weak constitution who were subject to other nervous maladies. Its chill was not strong or violent but slow and imperceptible, taking longer to develop. The hot stage was not so intense as in common or inflammatory fever due to the blood, nor was the pulse "so quick, strong, or full, . . . but small, oppressed, and sometimes interrupted". The patient was lethargic, sometimes comatose, the urine limpid without sediment, and the disease extremely prolonged. Cheyne noted: "This State continues, or grows worse, from the *fifteenth* to the *twentieth*, or *thirtieth*, or sometimes the *fortieth* Day, if they live so long." He suggested blistering and vomits as the appropriate therapy with blood let only once. Diaphoretics were valuable, and attention to diet was essential to correct the underlying causes.[26]

In 1734 Ebenezer Gilchrist (1707–1774), a Scottish physician practising in Dumfries and remembered for his efforts to revive classical remedies, wrote on nervous fevers.[27] Like Cheyne, Gilchrist considered that regimen was an important factor in nervous fevers which were so common among the poor but rare among the well-to-do He suggested that the then current fashion of drinking hard liquor might be related to the disease, observing that wine was a good prophylactic and remedial agent. In fact, he knew of no cases of nervous fever among those who preferred wine to stronger drink. Those who drank wine may have had less occasion to drink water, because wine did not need to be diluted with water, as did strong spirits. He also urged warm baths and close attention to regimen, and warned that bleeding was contraindicated.[28]

In 1735 Theophilus Lobb (1678–1763), a nonconformist minister and physician of London known for his excellent treatise on smallpox (1731) and his support of inoculation, published a work on fever therapy. This was based on an understanding of the pathology of animal fluids; that is, blood, lymph, secretions, and nervous fluid, and was similar in principle to the ancient humoral pathology. In disease the fluids were imbalanced in quality or quantity and therapy should be directed towards restor-

[25] George Cheyne, *The English malady or a treatise of nervous diseases of all kinds*, London, G. Strahan, 1733.

[26] Ibid., pp. 226–233.

[27] *Dictionary of national biography*, s.v. Ebenezer Gilchrist.

[28] Ebenezer Gilchrist, 'An essay on nervous fever', *Edinb. med. Essays*, 1734, **4**: 347–407.

ing the balance. Lobb was particularly cautious on bleeding in fevers since it seemed improbable to him that all fevers were the result of too much blood. He specifically criticized Cheyne's theory of acute continued fevers, caused by obstruction, and seriously questioned the value of bleeding even if obstruction was the cause. Finally, he repeatedly cautioned against bleeding; if the patient's strength was "sensibly abated", if the pulse became weaker or quicker, or if "the Body becomes too cool", then an initial bleeding had "done Harm, and must not be repeated".[29]

In 1735 Browne Langrish (d. 1759) published an influential monograph entitled *Modern theory and practice of physic*, which was, in fact, a study of fevers. Langrish, a newly elected Fellow of the Royal Society, was an authority on mechanical physiology. His election was owed to his 1734 monograph on muscle physiology, and in 1747 he delivered the Croonian Lecture. He had begun practice as a surgeon at Petersfield in Hampshire, but had earned the extra licence of the Royal College of Physicians in 1734.[30] An acute clinical observer, in his monograph of 1735 Langrish considered the various types of fevers which were clinically important – intermittent, inflammatory, slow, hectic, and simple. The simple, inflammatory, and hectic were all related to inflammation and seem, at least in retrospect, to be distinguished primarily by their severity and rate of progress in the patient. These fevers differed substantially from the slow or nervous fevers which came on quietly and lasted longer, leaving the patient severely debilitated. The major practical distinction was therapeutic, for, while bleeding was helpful in the other fever types, Langrish cautioned the student never to bleed in a slow fever: "But above all things be sure to refrain the use of the lancet as you value the life of your patient and your own reputation".[31]

The early 1740s witnessed Creighton's third epidemic fever; he traced it from an outbreak of ship fever at Plymouth in 1740, reported by John Huxham.[32] This fever persisted at Plymouth into summer 1741 when the fever assumed a bilious form,[33] but by 1741 the epidemic fever was widespread in England and Scotland. It was connected by various authors to the 1741 Spring Assizes and to jails, and was commonly attributed to the economic distress current among the poor. It possessed characteristics which would suggest real and important differences from the epidemics of earlier years. Huxham described it as pestilential rather than slow; Gilchrist believed its fever to have a more malignant nature than the nervous fever he had first described in 1735.[34] Its epidemiology also appeared different – John Altree of Wolverhampton, in a letter to the *Gentleman's Magazine*, noted that it was a disease of the poor in the larger towns, particularly those in workhouses and prisons. Country people were generally exempt. In the epidemic of the 1720s the disease had begun in the country and only slowly spread to the towns. In addition, Altree noted what he thought were therapeutic differences, writing, "The medicines to which the usual inflammatory,

[29] Theophilus Lobb, *Rational methods of curing fevers*, London, J. Oswald, 1734, pp. 308–309.
[30] *Dictionary of national biography*, s.v. Browne Langrish.
[31] Browne Langrish, *Modern theory and practice of physic*, London, A. Bettesworth & C. Hitch, 1735, p. 343.
[32] Creighton, op. cit., note 18 above, vol. 2, pp. 78–79.
[33] John Huxham, *Observationes de aëre et morbis epidemicis*, 2nd ed., 3 vols., London, J. Hinton, 1752.
[34] E. Gilchrist, 'Essay on nervous fever', *Edinb. med. Essays*, 1742, **6**: 505–573.

intermittent or nervous Fevers yield here seldom effect much".[35]

Medical opinion at the time was divided as to the nature of the epidemic fever. Very early in the epidemic a London apothecary, Daniel Cox, published some observations on cases in London in which he found bleeding and other antiphlogistic measures helpful, and was confirmed in his belief that the epidemic was essentially an inflammatory fever.[36]

John Barker (1708–1749) also recorded his observations on the disease as it occurred at Salisbury, Wiltshire.[37] Barker was well educated. In addition to studying with Boerhaave, he had been a student at Oxford and St. Thomas's Hospital, London. He eventually settled in London, serving on the staff of the Westminster Hospital. Barker's detailed analysis of the epidemic was influential; Creighton called him "the best medical writer upon the epidemic".[38] Barker's purpose in undertaking his study of the 1741 epidemic was therapeutic, particularly to determine "whether it be in general right to *Bleed* in the present *Epidemick Fever*?" In order to answer the question properly, Barker believed it necessary to describe the nature and cause of the epidemic. In doing so, his therapeutic experience would have value for other practitioners as an analogy, and it would be apparent how analogous epidemics had provided him with guidance in 1741. In Barker's opinion, the exclusive appeal by a physician to his own experience was the weakest support of a method of practice. Over the long term, medicine would be improved by "the Hippocratick Method of *judicious Observation* and wise *Reasoning*".[39]

Barker examined various ideas on the causes of epidemics, contrasting the notions of Hippocrates, whose epidemics were related to manifest qualities of the air, with Sydenham, who related epidemics to unknown qualities of the air. Barker believed that the two authorities agreed more than they disagreed, concluding that both types of atmospheric qualities influenced epidemics and that it was necessary to examine a particular epidemic to determine the relative roles of various qualities of the atmosphere. He then pointed out that in recent years epidemic fevers had been divided into inflammatory and nervous types but "the real Difference between the *Nervous* and *Inflammatory* kinds of Fevers, seems not yet to have been settled, nor the genuine Cause of each assigned."[40]

Barker undertook to show the nature of the distinction by using his "Hippocratick method". Fever, according to Sydenham, was the effort to remove morbid matter from the body and so restore health. To remove morbid matter Barker believed that nature excited "certain praeternatural Motions in the Body". Such motions constituted a fever and thereby explained Boerhaave's observation that the pathognomonic sign in fever was an increased pulse. But since the nerves "are the only Instruments of motion in the Body", the increased motion of fever "must arise either

[35] John Altree, 'Observations relating to the present epidemic fever', *Gent. Mag.*, 1741, **11**: 655–656.

[36] [Daniel Cox], *Observations on the epidemic fever of the year 1741*, 2nd ed., London, W. Meadows & T. Cox, 1742.

[37] John Barker, *An inquiry into the nature, cause, and cure of the present epidemick fever*, London, T. Astley, 1742.

[38] Creighton, op. cit., note 18 above, p. 79. See Underwood, op. cit., note 19 above, pp. 160–161, for biographical details.

[39] Barker, op. cit., note 37 above, pp. 3–5.

[40] Ibid., pp. 7–19.

from some cause acting immediately upon the Nerves themselves, . . . or mediately, by means of the Fluids . . .". Barker believed the cause acting on the nerves was "nothing but an Acrid Stimulating Body", while that acting on the fluids was an obstruction.[41] He quoted extensively from Cheyne on nervous fevers and presented comparative cases of his own to illustrate differences among fevers, concluding that acrimony produces nervous fever while obstruction produces inflammatory fever. Most commonly, however, the two varieties of cause acted together or successively to "form a Fever of *Mixt* or *Compound* Kind, partly partaking of the Nature of the one, and partly of the other".[42]

In an effort to determine the variety of fever which produced the epidemic of 1741, Barker traced the spread of the epidemic, which he said had its origin among the prisoners at Exeter and was called gaol fever. From Exeter it spread through the west of England, but in such a manner as to leave the question as to "whether or no it was propagated by Contagion . . . uncertain". Barker then described the typical course of the illness, stressing pain in the head, lassitude and loss of strength, and the slow but steady worsening of all symptoms. He observed that the fever was similar to that described by Sydenham as new fever in 1684–85, and that the environmental conditions preceding both fevers were very similar. He also compared it to the epidemics of 1728–29, particularly as recorded in the works of Friedrich Hoffmann. These and other analogies combined with the early experiences of physicians of Barker's acquaintance at Bristol and at Exeter, and his own in Salisbury where bleeding was apparently harmful to the patients, caused him to disagree with Daniel Cox and to conclude that the epidemic fever in the west was principally of the nervous variety.[43]

In support of his diagnosis, Barker referred to the authority of a wide variety of British writers of the 1730s. He cited passages from Gilchrist, Lobb, and Langrish, among others, to show that bleeding was contraindicated in slow nervous fever. In the present epidemic bleeding was bad for the patient because the fever manifested symptoms compatible with the nervous diagnosis. Thus supported by analogy, experience, reason, and authority, Barker completed his inquiry and offered a method of treatment based on the diagnosis, Nervous Fever. The method included blisters, cordials, and gentle evacuation of the gastrointestinal tract to remove the acrimony.

Barker's opinions on fever were probably representative of British opinion in the 1740s. They are certainly compatible with the ideas of Cullen as recorded in his writings of the 1770s and, if Wallace's recollections may be trusted, with Cullen's opinions of 1746. Barker was aware of current research on the nerves and cited in particular the neurophysiology of Malcolm Flemyng, a fellow-student of Boerhaave and a London teacher of physiology. In the decade following Barker's work, research in neuroanatomy and physiology accelerated with the publication of Albrecht von Haller's *First lines of physiology* in 1747, Robert Whytt's essay on the vital and involuntary motions of animals in 1751, and Haller's monograph on sensible and irritable parts of the body in 1752.[44] During the 1740s, John Huxham completed his second volume of

[41] Ibid., pp. 19–22.
[42] Ibid., p. 30.
[43] Ibid., pp. 86–88.
[44] Karl E. Rothschuh, *History of physiology*, trans. G. B. Risse, Huntington, N.Y., Robert E. Krieger, 1973, pp. 123–132.

observations on the weather and disease as well as his classic *Essay on fevers* (1750).[45] Also in 1750, Sir John Pringle's first study of camp or hospital fever was published in a letter to Dr. Richard Mead.[46]

The works of Huxham may have helped Cullen to refine his ideas, because in his lectures Cullen cited Huxham's essay as the classic study of nervous fever.[47] Huxham's distinctions were similar to Barker's and Cullen's. He described the ardent or inflammatory fever, cured by evacuations, and the slow or nervous fever, with its origin in lax fibres and debility. Huxham is remembered for his accurate clinical distinction between the slow nervous fever and a contagious fever that he called putrid, malignant, petechial fever. In his nosography, Cullen denied the existence of putrid fever and took no notice of Huxham's distinctions, considering nervous fever and putrid fever as varying grades of typhus.[48]

William Cullen was an excellent clinician and a careful observer. His medical practice, revealed in his consultation letters, shows a careful, conscientious practitioner whose therapies did not always conform to the system of medicine he taught.[49] It is possible that the demands of pedagogy produced a system which, while designed for students learning medicine, was misapplied by practising physicians and medical theorists throughout their careers.

Cullen may have taught his early students to distinguish putrid and nervous fevers according to Huxham. Certainly Cullen's early Edinburgh student and lifelong friend, George Fordyce (1736–1802; Edinburgh M.D. 1758), distinguished them in his London medical lectures throughout the last third of the eighteenth century. Fordyce described a violent fever "otherwise called Putrid, Malignant, Jail, Camp, Hospital, or Petechial FEVER". It was similar to plague, and was to be prevented by the hygiehic measures described by John Pringle, and distinguished from inflammatory fevers, nervous fevers, and the mixed type of continued fevers.[50]

Cullen, like Barker, Boerhaave, Sydenham, and many other practitioners, believed that reasoning was unavoidable in medical practice. A physician used experience and reasoning in combination, the one supplementing and supporting the other.[51] Armed with analogies and the best understanding of physiological and pathological processes, the physician approached the bedside. He must always be aware of the limits of his theory, and Cullen cautioned students about the limits of his own. He wrote: "I shall myself direct your doubts, and on some occasion, I shall tell you cordially that our system is entirely defective".[52] Physiology, pathology, and nosography were all guides to help to organize analogies and reasoning into the most useful form for the novice.

The disease typhus as understood by William Cullen, whether he was applying the

[45] John Huxham, *An essay on fevers and their various kinds*, London, S. Austen, 1750.

[46] John Pringle, *Observations on the nature and care of hospital and jayl-fevers*, London, A. Millar & D. Wilson, 1750.

[47] Cullen, op. cit., note 5 above, vol. 1, pp. 522–524.

[48] Cullen, op. cit., note 2 above, vol. 1, p. 256.

[49] Guenter B. Risse, '"Doctor William Cullen, Physician, Edinburgh": a consultation practice in the eighteenth century', *Bull. Hist. Med.*, 1974, **48**: 338–351.

[50] George Fordyce, *Elements of the practice of physic*, 4th ed., London, J. Johnson, 1777, p. 162.

[51] William Cullen, 'Method of study', in Thomson, op. cit., note 2 above, pp. 415–417.

[52] Ibid., p. 439.

term to the nervous or putrid fever of Huxham, was clinically associated with debility. The common experience of eighteenth-century British physicians was that bleeding was harmful in both varieties. Because the various forms of fever received essentially the same treatment, the clinical distinctions became of secondary importance in the training of practitioners.

Cullen is often viewed as a systematist whose work may either be dismissed or presented as an example to illustrate the uncritical character of eighteenth-century rationalism. As Lester King wrote, Cullen "tried to create an over-arching system that would account for all the data and would explain them in a logical and satisfying manner", and in particular, his "nosology ... illustrates well the rationalistic tendencies of the time".[53] According to Gernot Rath, Cullen's success was due to his having "arranged his ideas into an ingenious, easily comprehensible, and teachable system".[54] Within limits, this is true. Cullen himself admitted as much, although he preferred the classical term dogmatist, which he defined for his students as a physician "who employs his reason, and, from some acquaintance with the nature of the human body, thinks he can throw some light upon diseases, and ascertain the proper methods of cure".[55]

In examining Cullen's medical theories, historians have rightly noted the importance of earlier work on the nervous system by Glisson, Willis, Haller, Whytt, and, in particular, the medical system of Friedrich Hoffmann. Cullen was greatly assisted also by earlier nosographers such as Sauvages, Linnaeus, and Vogel; and through them he was a direct intellectual descendant of Thomas Sydenham. Cullen also assimilated the mechanistic tradition in physiology and medicine, and perhaps owed a greater debt than he knew to Boerhaave and Descartes in the development of his system of medicine.[56]

The philosophical and scientific roots of Cullen's ideas explain only part of the evolution of his concepts of fever. His system, while important, was not paramount; it was a tool to be used to "ascertain the proper methods of cure". Cullen was quite logically dependent on the current state of eighteenth-century medical science to explain disease, but equally important was the clinical setting in which Cullen applied his science. His students had little or no clinical experience of their own on which to draw, and to assist them Cullen systematized the cumulative clinical experience of the time in his textbooks; medical teachers do the same thing today.

Consequently, Cullen taught his students to make distinctions which were important in medical practice. In the mid-eighteenth century some cases diagnosed as

[53] Lester King (editor), *A history of medicine*, Harmondsworth, Middx., Penguin Books, 1971. I have used these quotes from Dr. King because I feel they are particularly illustrative of a common attitude. I suspect, from Dr. King's recent treatment of the philosophical concepts of rationalism and empiricism in medical history, that he might express himself differently today.

[54] Gernot Rath, 'Neural pathology: a pathogenetic concept of the 18th and 19th centuries', *Bull. Hist. Med.*, 1959, **33**: 526–541, p. 531.

[55] Thomson, op. cit., note 1 above, vol. 1, p. 111.

[56] Inci Algug Bowman, 'William Cullen (1710–90) and the primacy of the nervous system', Ph.D. thesis, Indiana University, 1975, is one of the most complete and recent summaries of the sources and structure of Cullen's medical system. See also C. J. Lawrence, 'Early Edinburgh medicine: theory and practice', in R. G. W. Anderson and A. D. C. Simpson (editors), *The early years of the Edinburgh Medical School*, Edinburgh, Royal Scottish Museum, 1976, pp. 81–94.

fever were helped or at least seemed to be relieved by bleeding; some cases of fevers were not helped and indeed seemed to be harmed by bleeding. This practical difference needed to be explained in such a way as to assist students and practitioners in making the proper diagnosis and prescribing the proper method of cure. These practical needs help to explain why he was so strongly influenced by the tradition of research in neurophysiology.

By the same token, Cullen's textbooks were not definitive treatises on fever. He fully acknowledged the limits of his theories. As a result clinical distinctions of little therapeutic importance did not play a large role in his teaching. The distinctions between Huxham's putrid and nervous fevers might be important specific distinctions within the genus typhus, but they were treated in very similar ways. Thus recognition and explanation of the generic distinctions were important, while the difference between species of typhus, which might be touched on in a clinical lecture, was not of first importance in clinical practice and so did not receive significant attention in Cullen's textbook.

Cullen's concept of typhus, one of the most long-lived and influential concepts in his system, was also one that despite its debt to neurophysiology, was heavily dependent on the clinical and particularly the therapeutic realities of mid-eighteenth-century British medicine.