ENGLISH MEDICINE IN THE ROYAL SOCIETY'S CORRESPONDENCE: 1660–1677

by

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I MUST CONFESS at once that since I have the honour of addressing you this afternoon as your Sydenham Lecturer there is some lack of propriety in speaking of the Royal Society. Thomas Sydenham was never a Fellow, unlike so many of the outstanding English physicians of his time. We may presume I think that this fact implies some disinclination on his part to be elected. All but one of his closest colleagues, Dr. Dewhurst tells us, were Fellows and he was at least casually acquainted with many more. However, it is clear that Sydenham's cast of mind was wholly opposed to the kind of scientific medicine that the Royal Society was endeavouring to foster. 'That anatomie is like to afford any great improvement to the practise of physic, or to assist a man in the findeing out and establishing a true method, I have reason to doubt', he wrote. He had an equal distrust of the attempt to draw lessons from pathology and an even greater aversion to microscopy, which he regarded as a complete waste of time. Thus although I am not aware of Sydenham's actually being hostile to the Royal Society he must have regarded its pursuits as quite vain, if not indeed damaging to the advance of medicine by clinical observation.[1]

As might be expected, therefore, Sydenham's name appears seldom in the early correspondence of the Royal Society; even in the writings and correspondence of his friend Robert Boyle he has left very little trace. Sydenham is mentioned as being convinced, against his initial prejudice, of the reality of the cures wrought by Valentine Greatorix, the Irish stroker.[2] There are records of two copies of *Methodus curandi febres* being despatched by request to the continent.[3] Finally, there is the odd fact that when the Secretary of the Royal Society was thrown into the Tower on suspicion of unpatriotic correspondence with the enemy during the second Dutch war, Sydenham was (he claimed) the only person to speak ill of him; and he declined further acquaintance.[4]

Now this Secretary of the Royal Society was, of course, Henry Oldenburg; he was appointed to that office in the first of the Society's Charters, of 1662, and he continued to be re-elected into it each year until his death, aged about sixty, in 1677. It is upon his correspondence, partly personal but increasingly conducted on behalf of the Royal Society as the years roll by, that this lecture is based. A good deal of it survives, partly very well preserved by the Royal Society itself and much in other libraries and collections. My wife and I have so far edited well over 2,000 letters

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[1] Kenneth Dewhurst, *Dr. Thomas Sydenham*, London, 1966, pp. 63–65, 85.
[2] 18 September 1665; A. Rupert Hall and Marie Boas Hall, *The Correspondence of Henry Oldenburg*, Madison, Milwaukee and London, University of Wisconsin Press, 1966–; hereafter cited as *Correspondence*), I, 512–13. Dewhurst, *Sydenham*, 32.
[3] *Correspondence*, III, 367; VI, 209, 286.
[4] *Correspondence*, IV, 80, 95.

(by no means all of them complete letters, however) and we have still five more years to go with our work. The names of more than two hundred correspondents are known to us. Of these about eighty were resident in the British Isles, the remainder being widely scattered from India to Iceland and from the Bahamas to Stockholm. Not all in this latter and larger group were foreigners, however; a fair proportion were English travellers, merchants, diplomats and colonists resident overseas.

As a background to this large volume of correspondence I must this afternoon take the Royal Society's interest in medical matters largely for granted. At least a fifth of the Fellowship of the Royal Society was seriously concerned for the progress of medical knowledge at this time. I do not mean naturally that all of these were practising physicians or surgeons; only about a tenth of the Royal Society Fellows were also Fellows of the College of Physicians, for example. But taken in relation to the rather large inert mass of gentry, nobility, officials and lawyers, the medical element in the Royal Society was extremely strong; excepting the mathematicians it was the only coherent professional element. Apart from this common membership, including virtually all the most distinguished English medical men from Allen to Willis (with the exception of Sydenham), the Royal Society had almost no contact with the College of Physicians. It was not concerned with clinical medicine, Sydenham's preoccupation, or with the conduct and education of the profession. Its business was largely with anatomy, physiology, pathology and pharmacology. But of course these subjects are by no means clearly separable from 'experimental medicine' in a looser sense than Claude Bernard's, nor should they be taken as excluding a rather ghoulish taste for 'medical curiosities'. For even educated men of the seventeenth century the two-headed calf and the five-legged sheep had a fascination which the showman of today can—or could—only exploit in naive audiences.

Furthermore, the educated man shared with the common man a far more independent attitude to his body and its vagaries than is common in advanced societies at the present time. This point comes out frequently in the more personal letters of our *Correspondence*, and can be substantiated from many journals and other collections of private letters. For reasons which I think are well known the population of seventeenth-century England was far less healthy than that of England today; malaria of course was endemic, people suffered more or less regularly from fly- and water-borne diseases, they endured torments of toothache, rheumatism, bronchial infections, gout and the consequences of dietary rashness. They suffered too from the teachings of an ancient theory that the body is a machine in unstable equilibrium that requires constant tinkering to be kept in balance. Only the very greatest could employ daily professional attendance. Others dosed themselves. And for the poorest masses of the population there was no choice in the matter. While therefore a non-medical Fellow of the Royal Society, for example, would certainly have profound respect for the opinion of a distinguished physician like Sydenham or Edward Browne, he would also have almost daily recourse to a variety of household or traditional remedies and very probably be willing to experiment with a wide range of chemical preparations from antimony wine to calomel or a few drops of dilute sulphuric acid. Indeed I would guess—but no more—that there is almost an education- or class-distinction here in the later seventeenth century: the popular medicine of the upper classes tended to the inorganic, that of the lower classes to traditional organic sources of medicaments. But one can find enough of the latter even in the writings of Robert Boyle. Thus when we read that John Beale discovered how to improve his reading vision not by choosing empirically a pair of spectacles, but by adapting to his eyes a pair of paper cones which had the effect of greatly narrowing the field, this is a somewhat extraordinary and clumsy but still characteristic act of seventeenth-century self-medication.[5] Assuming that Beale suffered from severe astigmatism, for the correction of which contemporary spectacles were of no avail, one can understand the rationale of his strange expedient.

Between self-medication and the licensed practice of the College of Physicians there were many levels of medical practice. Many excellent provincial physicians who had proceeded to the Doctorate in Medicine were never members of the College, among them for example Henry Power, Malachi Thruston and Nathaniel Highmore. For those who had unqualified but perhaps real experience of medical practice the M.D. itself could be obtained by a perfunctory visit to a foreign university such as Leiden, or by procuring special dispensations. One who took the former course was the celebrated plant anatomist Nehemiah Grew, who figures a good deal in our *Correspondence* and who succeeded Henry Oldenburg in his Secretaryship; Grew matriculated at Leiden on 6 July 1671 and proceeded M.D. on 14 July with a thesis *De liquore nervosa* which he had obviously brought in his pocket. Another was Nathaniel Fairfax, of whom I shall say more in a moment, who had matriculated at Leiden on 21 June of the previous year and proceeded M.D. twelve days later. There were some eighteen Fellows of the Royal Society who practised medicine in or out of London without the licence of the College of Physicians. Some of these at least had no formal medical degree at all, John Locke, Shaftesbury's physician, being one of them. Another notorious unqualified practitioner was Henry Stubbe, ex-assistant to Bodley's Librarian, physician at Stratford-upon-Avon and Bath, virulent opponent of the Royal Society, and self-styled champion of the College of Physicians. Other men like Locke and of course Robert Boyle who had studied medicine profoundly used their knowledge to advise friends and relations without entering into normal practice. It was not unheard of, I believe, for beneficed clergymen to do the same and certainly in the early 1660s many ejected ministers took to medicine.

Nathaniel Fairfax (1637–90), many of whose letters we have printed in Vols. III–V of our *Correspondence*, was one of these. He was an M.A. of Corpus Christi College, Cambridge, and was therefore an educated man though one would hardly think so from his clumsy English and appalling Latin. He practised in Suffolk and was in some way a protégé of Dr. Thomas Browne. His freedom with anatomical truths that had been held since the time of Galen at least is illustrated by an amusing incident. Fairfax narrates the case of Goodwife Eliot of Mendlesham who passed by urine one of two caliver bullets which she had been induced by a neighbour to swallow for relief of her 'torment of the bowels'.[6] (This heroic measure or the exhibition

[5] See Beale's letters to Oldenburg in Vols. IV and V of the *Correspondence*, extracts from which were printed in the *Philosophical Transactions*.

[6] *Correspondence*, V, 47–49; Fairfax to Oldenburg, 18 September 1668. Dates throughout are in Old Style.

of a massive dose of mercury was a common last resort in cases of the iliac passion.[7]) Fairfax goes on:

> The main use that I would make of the instance (if it be worth mentioning) is to strengthen a suspicion that I have a long time had, of some other passage from the stomach to the bladder besides what anatomists have hitherto given account of. For that this bullet never came at the ureters through the veins, arteries, nerves or lymphducts (the only vessels that can be charged with it) is, I think, beyond dispute.[8]

And so forth; there is a good deal more. Now Oldenburg printed part of this letter in the *Philosophical Transactions*,[9] as he usually did, for Fairfax is rich in 'curiosity'. The curiosity of Fairfax's anatomical speculations did not escape the experienced London anatomists, including Walter Charleton, and some months later Fairfax wrote Oldenburg a humble exculpation for what was 'so hideously beyond dispute that it was very unanatomical and a sorry weakness to hint it so'.[10] It is interesting that at the same time he apologizes for mentioning his prescription of 'patent medicines'—Lockyer's and Matthew's pills—'hereby giving occasion to strengthen the scandal raised on the Society as too friendly to quacks and yourself [Oldenburg] as corresponding with a declared one'. Lest anyone should think that Fairfax was unique in doubting one of the most famous conclusions of Greek experimental physiology, I must add that exactly the same scepticism was shown by Pierre Daniel Huet, leading light of the Scientific Society at Caen, later tutor to the Dauphin and Bishop of Avranches.[11] Huet claimed that having ligatured the ureters of a dog the bladder nevertheless filled with urine. In his reply Oldenburg was able to assure him that when the ureters were effectively blocked in experiments by Dr. Edmund King no urine entered the bladder:

> Our most learned physicians are convinced [he wrote] that there is no passage to the bladder except through the ureters in view of all the investigations which they say have been made with the greatest possible care to discover such a passage. To this they add that having thought it over carefully, they see no need of there being any other, considering the wonderfully rapid circulation of the blood and other fluids through the body and the swift fermentation and percolation of the same in the organs through which they pass.[12]

No doubt this last sentence goes to the root of the matter. It was the very swiftness of the body's action in assimilating and distributing ingesta which made physiology as it was taught by the sicentists of 1670 seem dubious to naive minds.

But to return briefly to Fairfax. Like many physicians he had a taste for natural history—unsophisticated of course in him—and like almost everybody he was excessively preoccupied with the poisonous attributes of spiders and toads. And this gives me occasion to mention here that we have in the *Correspondence* besides Fairfax's credulity the entirely rational and modern-sounding story of the attempt by Tommaso Cornelio of Naples to discredit the extraordinary phenomena universally

[7] For the views of the physicians Allen, Clarke, Ent and Goddard on this use of mercury see *Correspondence*, VI, 25–30; Oldenburg to Segni, 10 June 1669.

[8] *Correspondence*, V, 48; I have modernized spelling and punctuation in this and subsequent quotations.

[9] See no. 40 (19 October 1668), 803–5.

[10] *Correspondence*, V, 505; Fairfax to Oldenburg, 30 April 1669.

[11] Ibid., VII, 206–9; Huet to Oldenburg, 20 October 1670.

[12] Ibid., VII, 394–97; Oldenburg to Huet, 16 January 1670/71.

linked with the tarantula of southern Italy. Like Redi's work on spontaneous generation, it is a minor episode in the progress of enlightenment. Unfortunately even such capable naturalists as Martin Lister were not quite able to dispel such traditional fables.[13] Fairfax also relates many medical histories, which give some notion of the byways of seventeenth-century medical practice; he describes a case of Siamese twins,[14] and another of hermaphroditism;[15] he tells of a strange case of attempted suicide by fasting, which was ended by the lady riding to Ipswich and eating buttered peas and a pint of strawberries ('which she told me made her sick'—not surprisingly);[16] and he reminds us of the rarity of the survival of multiple births under primitive medical conditions:

> Goodwife Rivers of Ipswich, a young woman, at her fifth conception last summer brought forth, with good labour, three living infants, two sons [and] one daughter, at a birth; all of which sucked of the mother and throve well for a week, but then fell into a wane and one after the other died within the month.

In another class, less learned but perhaps more skilled in their own way, were the provincial apothecaries and chemists. It would be interesting to try to form a picture of the magnitude and quality of their contribution to the health of seventeenth-century England. Here is a minute morsel. In a letter written from Durham a schoolmaster named Peter Nelson (who was acquainted with a man formerly in Boyle's service and also with John Webster, author of *Metallographia*) writes of the 'physicians' in that city:[17]

> first Dr. Wilson an ingenious man and a good scholar, for the most part a Methodist . . . a diligent peruser of your monthly [*Philosophical Transactions*]
> The next is Mr. Nicholson, a serious young man, and well educated, inclinable to chemistry but no great practitioner.
> Next I reckon one Mr. Selbie, who hath been as much beholding to fortune as education, but a civil man and well-spoken, has been born under a good thriving aspects and is fallen into a notable way of practise; he works sometimes in the fire and has a small laboratory in which he makes some of the medicines he uses.
> There is one Mr. Dancy, a man that is thought to have good skill and hath done divers handsome cures, but hath not had the luck to thrive and is not therefore so considerable as possibly he might have been.

Mutatis mutandis not unreminiscent of *Middlemarch*. Four practitioners of a sort in a remote and lightly populated region where humble coal-miners were already far more numerous than gentry and wealthy bourgeois does not seem an inadequate provision. Unfortunately I can say something more of only one of these four, since Dr. Wilson cannot be definitely identified as deserving the prefix, though local research might uncover more information. Robert Selbie himself also wrote to Oldenburg to give 'a general account of some more than ordinary success in my practice'. At the risk of over-quotation I must try to convey the flavour of this letter:

> The maladies I have observed (of those most feral and truculent) are your dropsies, convulsions and convulsive motions; as your emprostotonos and opistotonos which are much more terrible than a complete convulsion. A diabetes, and lately in twenty days' time a young man of a Scorbutic palsy. Consumptions as also the rickets. But especially an old gentlewoman of this town

[13] See *Correspondence*, Vols. VII and VIII, Index, s.v. 'Spiders'.
[14] Ibid., III, 491–97; Fairfax to Oldenburg, 28 September 1667.
[15] Ibid., V, 376–79; Fairfax to Oldenburg, 4 February 1668/69.
[16] Ibid., VI, 67–71; Fairfax to Oldenburg, 28 June 1669.
[17] Ibid., VII, 326–27; Nelson to Oldenburg, 15 December 1670.

past eighty years of a confirmed dropsy. And if I may further speak without ostentation equally successful in whatever distemper occurs with my fellow practitioners . . .As to medicines that I have used for this six or seven years are these principally, videlicet dissoluble magistry of coral prepared with your acetum philosophorum; your elixir proprietatis after several other menstruums made with spiritum vini subtilissimum et spiritum salis well incorporated by often drawing over; ens veneris in which sublimation I always receive a spirit first of good use; spiritum cornus cervi, antimonium diaphoreticum, salem antimonium. Also a pleasant tinctura antimonii imbodied with tartar which I use upon all occasion where vomits are required; volatile salt of tartar converted into a liquor with which I prepare several cathartics, with many others. My furnaces being for the most part constantly employed.[18]

This is rather Ben Jonson than George Eliot; not that Selbie's letter is at all abnormal by the standards of seventeenth-century pharmaceutical chemistry. It shows how far the teaching of Béguin, Van Helmont and Zwelfer had penetrated. No wonder one reads somewhere of a patient lying helplessly on the floor in an abandon of purgation both upwards and downwards.

Perhaps I ought to add a word on the general issue of Galenicals and chemicals, but really from our correspondence there is little to say. After the Plague the issue was no longer a live one. You will know that an attempt was made, after a good deal of controversy, to found a Society of Chemical Physicians in 1665, and that the attempt came to nothing. The failure was not significant. By now too much powerful influence—that of Boyle, Goddard and Willis, for example—was in favour of iatrochemistry for it to be other than respectable. There are a few trifling allusions to Stubbe's attempt to revive the debate by his attacks on George Thompson in 1670, which naturally were hardly grateful to the Royal Society, but in general chemical remedies seem to be accepted as perfectly normal, as they were by Peter Nelson, for example.

Other novel elements in therapy that appear are the practices of hot bathing and taking spa waters. The egregious Joseph Glanvill contributed a dull letter on the hot baths at Bath which was partly printed in the *Philosophical Transactions*.[19] There are a number of references to the book by Henricus ab Heer, *Spadacrene. Hoc est fons Spadanus accuratissime descriptus,* published in 1647, which seems to have set going the whole spa water movement; and about 1669–70 there was a special interest in the composition of spa waters and the chemical theory behind their effectiveness. Daniel Foote in a rather interesting letter tried to base such a theory on the acid alkali dualism of Otto Tachenius.[20] Robert Wittie (or Witty) and his writings naturally came up for discussion, much to the satisfaction of this defender of Scarborough Spa. Again, I need not enter into his controversy with William Simpson as Dr. Poynter has already done so.[21] Wittie wrote:[22] 'I must ever acknowledge my deep obligations to those noble gentlemen of the Royal Society for their candour and condescension to take notice of my weak endeavour, whom I wish I were able or worthy to serve in any thing.'

[18] Ibid., VII, 532–33; Selbie to Oldenburg, late March 1671.
[19] Ibid., VI, 47–51; Glanvill to Oldenburg, 16 June 1669; *Phil. Trans.*, no 49 (19 July 1669), 977–82.
[20] *Correspondence*, VI, 275–78; Foote to Oldenburg, 11 October 1669.
[21] F. N. L. Poynter, 'A Seventeenth Century Medical Controversy: Robert Witty versus William Simpson', in *Science, Medicine and History*, ed. E. Ashworth Underwood, London, Oxford University Press, 1953, II, 72–81.
[22] *Correspondence*, VII, 52; Wittie to Oldenburg, 4 July 1670. For his earlier letter see VI, 605–13. Wittie probably did not realise that the *Philosophical Transactions* were wholly controlled by Oldenburg.

I judge that in this case as in many others the Royal Society and the *Philosophical Transactions* (which in large measure was the contemporary published version of Oldenburg's correspondence), taken together, threw their weight on the side of innovation, properly controlled by analysis and experiment. The Royal Society rarely spoke for conservatism.

We see this in the episode I come to next. For it is time for me to move from the grass-roots of medical practice to the experimental and scientific aspects. The most dramatic of all these is, obviously, the story of transfusion. It has been told so many times that I may be brief. Its origins lie in new ideas about animal poisons, which in turn are not unrelated to the discovery of the swift circulation of the blood. By about 1660 many physicians and pharmacologists—but not all[23]—believed that when a snake (for example) bites, or an insect stings, a fluid poison is injected into the body which, carried by the blood, very quickly causes illness or death.[24] This, of course, contrasts with the alternative theory (seemingly supported by the puzzling contrast between the bites of healthy and 'mad' dogs) that poisoning was the result of the creature's rage, affecting its spirits. This new rationalist interpretation of poisoning immediately provokes a comparison with the action of ingested poisons. If, then, the same poisonous substances could be both ingested and injected—and experiments were performed to show that this was so, and that injected poisons acted more quickly—might it not also be true that health-giving rather than poisonous substances could work very well if injected into the body rather than ingested? Guided by this analogy Christopher Wren proposed and with Boyle carried out such injection experiments on animals not later than 1658. Independently, at a time when this early English initiative had lost impetus, the same notion occurred to Johann Daniel Major of Hamburg who published a little book about it in 1664 which he sent to the Royal Society.[25] And this in turn was followed quickly by the *Clysmatica nova* of Johann Sigismund Elsholtz.[26] We find Oldenburg assuring the continent that the English were first in the field by several years;[27] in fact, some further work on injection had been done in the interval by Timothy Clarke, a (to my mind) unlikeable royal physician, on which he had reported to the Royal Society on 16 September 1663.[28]

It was in the discussion of Clarke's paper that the idea of transfusing blood from an animal to another by means of a pipe was first mentioned. Not all those present could see the medical utility of the procedure, yet the naive logic is obvious enough: if the object of injected medicines was to purify the blood in a sick body, why not achieve the same object more directly by transferring good blood from a healthy animal? Naturally the idea that there might be crucial idiosyncratic differences between the blood of different individuals of the same species and still more between

[23] See *Correspondence*, Vol. VIII, Letters 1940 and 1944; Charas to Oldenburg, 28 and 30 March 1672; Vol. IX, Letter 2037, Platt to Oldenburg, 27 July 1672 and Letter 2038, Magalotti to Oldenburg, 28 July 1672.

[24] Redi held this view in print in 1664; compare Hooke and Merrett on the viper, 26 October and 2 November 1664, in Thomas Birch, *History of the Royal Society*, London, 1756, I, 479, 481, and the letters cited in the previous note. See also M. P. Earles, *Annals of Science*, 1963, **19**, 241 ff.

[25] J. D. Major, *Prodromus inventae a se chirurgiae infusoriae*, Leipzig, 1664; see *Correspondence*, II, 334–38.

[26] Ibid., II, 580.

[27] Ibid., II, 379–80; Oldenburg to Major, 11 March 1664/5.

[28] Ibid., II, 380, note 1; IV, 6, 363–4 and 368, note 8; Birch, *History*, I, 303. No copy of Clarke's paper survives, apparently; if it could be found it would be of extreme interest.

members of different species occurred to no one.[29] Long medical tradition emphasized distinctions between the temperaments or constitutions of individuals—essentially psychological or at least non-mechanistic characteristics—but not between their physiological mechanisms. As you know, even anatomists long found it difficult to accommodate the fact that the simple topographical anatomical structure of all individuals of the same species is not absolutely identical.

Here I must really cut short as we move on to very familiar ground, though our *Correspondence* adds many new details to the story. The English experiments were held up by technical difficulties, by Clarke's sluggishness, by the Plague of 1665, and by Wren's transfer of his allegiance from science to architecture. It is well known that the French surgeon Jean Denis first took the rash step of attempting to transfuse blood from a lamb into a human patient on 6 June 1667. Meanwhile the English had achieved seeming success in transfusion between animals of different species.[30] We have published in our *Correspondence of Henry Oldenburg* the whole of Denis' correspondence with the Royal Society concerning this event and its sequelae; and the inevitably many allusions to them in other letters. It is often supposed that the death of a patient after he had suffered a transfusion administered by Denis led to an official termination of such experiments in Paris. As Denis was at pains to point out to the Royal Society, he was at the subsequent inquiry found guiltless of causing the patient's death, and the official judgment was that transfusions should only be performed under the direction of the Medical Faculty of Paris.[31] In practice it must be admitted the result was the same. Nevertheless, Denis persisted in the defence of the transfusion operation for some time, and interest in injection and transfusion lingered on the continent for many years. There is a thorough and intelligent dissertation on the injection of fluids into the veins in a French version of Michael Ettmüller's *Nouvelle Pratique de Chirurgie* at least as late as 1691, and a fairly well-known engraving of a transfusion scene (lamb donor, human recipient) continued to appear at least until 1705.[32] After all, when a patient was pretty sick death at the surgeon's or physician's hands through the more extreme forms of treatment was no rarity; it was common enough among those cut for the stone. The Hippocratic saying, desperate cases justify desperate remedies, could be as well applied to transfusion as to lithotomy or the almost fabulous Caesarian section;[33] the rational way was to experiment carefully.[34] However, the English gave the business up, and we hear no more of it in our *Correspondence* after 1670 save as a subject of priority wrangles.[35] In reply to a rather full and sensible discussion of the general problems of injection therapy written him by the Venetian physician Francisco Travagino,[36] Oldenburg wrote (I translate):[37]

[29] For Denis' discussion of differences in blood between individuals and his conclusion that these are no more significant than the differences between the various sorts of food that enter the bloodstream, see his *Letter to Montmor* of 25 June 1667 [N.S.] and *Correspondence*, III, 480–83.

[30] See, besides *Correspondence*, III, *passim*, *Phil. Trans.*, no. 25 (6 May 1667).

[31] *Correspondence*, IV, 372–87; Denis' printed letter to Oldenburg of 5 May 1668.

[32] Reproduced in *Correspondence*, IV, Plate I.

[33] See ibid., VI, 362–63; Oldenburg to Rudbeck, 9 December 1669; VII, 95–96, Oldenburg to Rudbeck, 23 July 1670.

[34] See ibid., V, 480–83; Martel to Oldenburg, 11 April 1669.

[35] E.g. ibid., VII, 561, 564; Wallis to Oldenburg, 7 April 1671. Compare II, 484.

[36] Ibid., VI, 492–500; Travagino to Oldenburg, 13 February 1669/70.

[37] Ibid., VII, 557–9; Oldenburg to Travagino, 14 March 1669/70.

> Your very learned and skilful remarks about the fluids or spirits to be mixed with the human blood by means of injection-surgery were very welcome to many of our Fellows, who allow as you do that the task of transferring injections of this sort with good success into the art of healing men is full of hazards. Meanwhile, if it shall prove possible to arrive at a more complete knowledge of both the human blood and the spagyrical fluids through all kinds of observations upon both, and through repeated experiments performed properly and faithfully by wise persons, then in my opinion it will be by no means necessary to despair of the outstanding usefulness of that kind of surgery.

So far as it goes this statement is unexceptionable. The Royal Society had clearly realized that, attractive as the potentialities of this new branch, or rather new branches, of medicine might be, they could not be developed safely until a great deal more basic science was known. Only we today can appreciate how much had, indeed, still to be learned.

One element is missing from this appreciation of the situation, however, on which I must dwell an instant: I mean, of course, that Oldenburg (and I presume his medical colleagues) does not also see any necessity for understanding the basic causes of diseases. Supposing the physician already in possession of a very complete knowledge of the constitution, functions and pathology of the blood, as also of the effects of a wide range of injected medicaments, does it not seem necessary that he should also appreciate the causes of pathological states in the blood or indeed elsewhere in the body? One might have thought that experiments with a totally new form of therapy would have stimulated fresh thoughts about the targets, so to speak, at which the new weapons were to be directed: apparently it did not. Either because the physicians of this time were quite unaware of the difference between treating the symptoms of the disease and treating its causes, or because they regarded the causes of disease as sufficiently well known (bad habits, a weak or unbalanced constitution, improper foods and so on) there seems to be in our *Correspondence* amid many case-histories, a multitude of pathological reports, and recurrent discussion of the value and preparation of a great number of drugs, little interest in the origins or communication of disease. Perhaps I may quote the only two that come readily to hand; a German physician, Michael Behm, writes in 1667:[38]

> I have certainly observed that gout and arthritis are caused when the urinous corruption is not separated from the blood by the kidneys and by sweating but is circulated about the body with it, adhering to the colder ligaments around the joints; there it causes rather acute pain and even swellings by the accretion of salt, or because its viscosity occasion stiffness and calcification.

And he goes on to doubt the theory of de le Boë Sylvius that some diseases arise from the effervescence of the acid pancreatic juice with the bile in the duodenum.

In Behm's two examples—the one positive, the other negative—disease (or rather its symptoms) is assigned to physiological malfunction; the kidneys are disordered, or the alchemy of digestion has gone astray. Fair enough. To seventeenth-century medicine it was indeed obvious that the study of normal and pathological physiology is basic to a rational therapy. Did physicians then dismiss as hopeless or unnecessary any attempt to go a step farther back and ask why this patient's kidneys (but not all) ceased to do their work properly? I find this question puzzling.

[38] Ibid., III, 573, 575; Behm to Hevelius, 1 November 1667.

My second quotation is more straightforward. Jean Denis, the transfusion experimenter, replies to Oldenburg:[39]

> You wrote to me in your last that it would be of great convenience in putting transfusion to the test if certain diseases could be conveyed to animals artificially. Upon this I will impart a fact to you that came up here some time ago. A wet-nurse who was attacked by the smallpox communicated it to her child; when this was noticed the child was removed from her and suckled by a goat, which the child sucked at every day. This goat contracted the disease. For, some time afterwards, when milk from this goat was served to two different people who had been ordered to take goat's milk, both of them contracted the smallpox only by drinking its milk each morning.

I do not seek to account for this tale. But it is at least an observation on the *communication* of an identifiable disease from person to person in a rather precise way, which is rare, at least in the medicine of our *Correspondence*.

Reverting to the quotation from Oldenburg's letter to Travagino that I read a few moments ago, it is scarcely necessary to assert that all our evidence indicates a confident belief in the present and future progress of medicine. To hasten this progress was one of the great objects of the organized, co-operative investigation into Nature that Oldenburg tirelessly advocated in his letters of exhortation; it was *the* most obvious way in which this investigation was useful to mankind, more than the satisfaction of intellectual curiosity. As to the method of this investigation a significant geographical division between Oldenburg's correspondents (and the publications with which they were associated) may be noted. In England, France, Holland and Italy the scientific movement was, of course, composed of mathematicians and astronomers as well as physicians and iatrochemists; the two groups were members of the same societies and read the same journals. In eastern and northern Europe at this time the movement was almost entirely composed of physicians, as you may easily see by glancing at the *Miscellanea curiosa* or the *Acta Hafniensia*. (The *Acta Eruditorum* did not yet exist.) Moreover, while the scientific physicians of the west and south devoted their efforts to basic biological science—to comparative anatomy, physiology, microscopy, medical chemistry, embryology and so forth—those of the east and north were largely preoccupied with the rarities of clinical practice and pharmacology. The mysterious iatrochemistry of which J. J. Becher was the archpriest was much in vogue, while men like Malpighi, Redi, Bellini, De Graaf, Swammerdam, Croone, Willis, Lower or Lister were rare indeed beyond the Rhine.

I make this doubtless exaggerated and rash generalization simply to justify my contention that, although one may discover in the Royal Society's correspondence a farrago of medical curiosities and chemical wonder-drugs, such evidence of triviality or misguided enthusiasm among English medical practitioners of all levels of sophistication is not important when viewed in the context of the age and when set against the mass of learned publication devoted to basic science.

I suppose I could make some kind of a case for maintaining that natural history is the most basic of all sciences, even medical sciences, since it describes the inescapable environment of human life which (in seventeenth-century terms at least) not only occasions many ills but provides the cures for them. Certainly Oldenburg was fond of proclaiming that a true natural history is the *sine qua non* of sound

[39] However, the two creators of the science of plant anatomy, Malpighi and Grew, were both physicians by profession.

natural philosophy. It was routine in the Royal Society's correspondence to ask in this way about the medical experience of any region of the globe in relation to climate, topography and so on, seeking (somewhat ineffectually, it must be admitted) to assemble the elements of a medical geography. Moreover, although the age of geographical discovery was nearly two centuries old, the lure of the exotic was strong upon the philosophical physician. And that it should be so was not irrational. The botany and dietetic properties of non-European food-plants were almost unknown in England and elsewhere; even the potato and maize were still rare; cassava, yams, cocoa-palm, tropical nuts and fruits were hardly more than names and crabbed woodcuts; even the tea and coffee plants had not yet reached European herbaria. Though systematic botany was passing from the hands of physicians to the care of non-medical specialists like Ray and Tournefort[39] the belief was still general that a more thorough knowledge of plants, both European and exotic, would yield the discovery of many useful materials. This belief is very evident in the letters of Martin Lister, for example. With so many hitherto unvisited regions of the globe open to European commerce or settlement, there was a strong desire to learn more precise facts to substantiate the inadequate accounts of exotic drug plants that had already reached Europe. So, for instance, the Hamburg physician Martin Vogel continually urged Oldenburg to exploit English trade links with the East and with North America to obtain botanical specimens. If the boasted virtues of guaiacum had proved fraudulent, the physiological effects of Jesuits' bark, not to say tobacco, had proved perfectly real; and (as we now know all too well in some cases) so are those of the Indian *bhang* (marijuana), cocculus Indicus, or various species of *Hyoscyamus* and *Datura* about which we find Vogel inquisitive.[40]

As I hinted at the beginning, Oldenburg did establish frail lines of communication with Iceland, the Bahamas, New England (whence John Winthrop sent various parcels of natural curiosities and Indian craft to the Royal Society) and with British agents in the Near and Far East, but whatever geographical or ethnological fruits these secured him, they brought in little of medical interest. Nor were his pressing inquiries of the distinguished band of Oxford oriental scholars any more profitable. But the most extraordinary product of his efforts in this direction were the 'Inquiries for Brazil' which he concocted in August 1671.[41] The world-wide missionary activities of the Society of Jesus and notably the studious activities of Father Matteo Ricci in China were of course known in England, if not exactly well understood; Oldenburg long had it in mind to exploit these far-flung Jesuits as sources of scientific intelligence. Finally, through an English merchant in Lisbon named Thomas Hill, probably a younger brother of Abraham Hill, the Royal Society's treasurer, Oldenburg was promised communication with a learned and intellectually active Jesuit father at Bahia (that is, Salvador, then the capital city of Brazil). These inquiries were destined for his attention; if they had ever received adequate attention (which so far as we know at present they did not) they would have required the work of a lifetime. They are based on the books devoted to the natural history and medicine of the Indies published by Wilhelm Piso and Georg Marggraf in 1648, which are indeed of funda-

[40] See, for example, Vol. IX, Letter 2048, Vogel to Oldenburg, 13 August 1672.
[41] Vol. VIII, Letters 1747 (Hill to Oldenburg, 13 July 1671); 1780 and 1780a (Oldenburg to Hill, 19 August 1671).

mental historical importance to this day. Apart from many inquiries relating to ethnology and zoology (the skunk, the porcupine, the rhea, the humming-bird, the anaconda and all South American fishes were complete mysteries to Europeans) there is much of medical interest. Oldenburg naturally used the Indian plant names which he found in his sources. He inquires not only about food plants and dye-stuffs but about plants of the pilocarpus group (yielding pilocarpine), *Operculina macrocarpa,* a source of jalap, sarsaparilla, copaiba, *Pithecolobium avaremotemo* (the 'Brazilian astringent bark' of nineteenth-century pharmacy), nux vomica and other *Strychnos* species, ipecacuanha, and many more. While he can only think of the Indians of Brazil as savages, Oldenburg clearly believes that these remote primitives possess a potent herbal medicine and a mastery of poisons unknown to Europeans. Placed by God in a region of the world which was clothed by plants completely different from those of Europe and the Near East, plants possessed of different and perhaps more powerful virtues, they have learned how to convert these virtues to human use and misuse. There is more than a hint of the concept of the Noble Savage, with medical overtones:

> Is it true that the natives grow to puberty early and age slowly, and then without loss of hair or teeth? Do Brazilian mothers laugh at our way of dressing and bringing up children, which, they say, impedes the perspiration and causes much catarrh? Are no squinting, purblind, lame or hunchbacked persons found among them because infants are never swathed in linen or bound up in swaddling clothes, but are frequently washed with cold water? Are the Brazilians rarely affected with illhealth? Do the more thoughtful among them attribute their good health and longevity to these causes, namely, that they have strength from birth, and are exposed to the excellent calmness and constancy of the air and winds, as also, that they hardly know what care is, what is heaviness of heart or bodily delights; that they always enjoy the same dress and diet, and those of the simplest? . . . Do the natives mostly employ as their usual healthful drink the very clear water of their rivers and springs, which even when drunk copiously cause no wind nor pains in the belly or abdomen, and far from weakening the stomach fortifies it remarkably?

I confess to complete ignorance of the long and complex story of the introduction of exotics into European medicine, nor could I say whether primitive simplicities have influenced medical thinking; but Oldenburg's tremendous epistle is at least worth noting in the former context.

I have only a few minutes left in which to refer briefly to anatomy, physiology and embryology as they appear in the Royal Society's correspondence. As I remarked before, we now deal with correspondence between or concerning the authors of well-known books. You will not be surprised to learn that we are publishing the full correspondence of Malpighi and Oldenburg, or Oldenburg and Regnier De Graaf. In the late 1660s and 1670s virtually all scientific communication between England and the continent passed through Oldenburg's hands and was known to the Royal Society. I need hardly remind you that Swammerdam dedicated a part of his work on the human uterus to the Society, or that (under Oldenburg's management) the Society published both of Malpighi's embryological studies. In some cases our correspondence adds further details about these various exchanges and corrects established errors. In others it contains opinions about the significance of the work of such English and continental medical scientists.

I will venture on two general comments. Firstly, these men did not nearly so often as one might idealistically imagine visualize their scientific work as related to their medical practice. All too often—and we can judge how human this is—the daily business of the physician appeared a mere drudgery, necessary to support life and family, which merely impeded the urgent task of scientific research. Vogel, Malpighi, Thomas Bartholin, Rudbeck, all voice this complaint. Swammerdam actually gave up medicine altogether in order to pursue microscopy. English physicians do not so complain—whether because the already greater wealth of English upper-class society in effect gave them greater leisure, or for other reasons, I do not know.

Secondly, one discovers a virulence of national pride. In the Germans it took the form of emulation of France, Britain and Italy and envy of the rich patronage which they knew the Italians and French and believed the English to have enjoyed. The French enjoyed a calm sense of superiority in philosophy and civilization over the rest of Europe; though they admired candidly English experimental achievements in medical science they were not above strident (and sometimes doubtful) claims of priority. The English were extremely vociferous in asserting their own discoveries. 'Philpatris comme tous les Anglois', Huygens once remarked. Swammerdam never wrote more welcome words than when he addressed the Royal Society in sending his presentation copy of *Miraculum naturae*:[42]

> I am not unaware how fate has brought it about that, just as Christendom owes no slight advancement of its religion to the English people, so in these recent very difficult times there was discovered among them the method of setting aside the empty disputations of the Schoolmen and of placing the useful arts and sciences on a solid basis; And as this is not the least part of Britain's glory, so it is the reason why no one dares or ought to dare, in matters of natural philosophy, to resort to any other tribunal than the Royal Society.

We shall fail to understand scientific communication in the seventeenth century if we fail to take the operation of this intense feeling of nationalism into account. If on the one hand, true native roots might be ascribed to a seemingly foreign innovation, it might flourish. Thus, although a German like Ettmüller will fairly allow Wren's priority in injection therapy, he and other Germans found the *effective* origin of this innovation (at least for German medicine) in Daniel Major's *Prodromus;* they took it up—rather in theory than in practice—as a German technique. Similarly Denis was able to (so to speak) naturalize transfusion in France by going from experiments on animals to the bold step, before which others had hesitated, of experiments on man, which the English could then only tamely imitate. It was of little use for Timothy Clarke to write a verbose statement of the English priority in injection and transfusion, and complain of the way in which the wily foreigner grasped for himself discoveries in anatomy and medicine first made by Englishmen.[43] The positive nationalism of the foreigners had led to further advances (if such, for the sake of argument, they may be termed). On the other hand *passive* nationalism, hugging a little bit of trivial priority to one's national pride so as to exclude a foreign investigation, could produce nothing but obscurantism. This seems to have happened—but the matter would be worth fuller investigation—to De Graaf's work on mammalian reproduction so far as England is concerned. De Graaf, whose conduct so far as I

[42] See *Phil. Trans.*, no. 84 (17 June 1672), 4098, and *Correspondence*, IX, Letter 1996, Oldenburg to Swammerdam, 13 June 1672.

[43] See *Correspondence*, IV, 350–69 reprinting and translating from *Phil. Trans.*, no. 35 (18 May 1668), 672–82, and many related references.

can judge was honest, patient and modest, was passionately anxious to have his researches properly esteemed in England, with its galaxy of medical talent and Royal Society. He was met with the charges—from Timothy Clarke chiefly but at first from other English physicians also—that in so far as his discoveries had not been previously known in England they were false, and in so far as they were true they had been anticipated. Clarke brought in a battery of names—Vesalius, Riolan, Tilman Trutwin, Glisson, Wharton—in an attempt to convict De Graaf of ignorance of the previous anatomical literature;[44] but the point at issue, of course, is not whether anyone before De Graaf had described the anatomy of the testis with approximate accuracy, but whether De Graaf had improved significantly on these earlier descriptions and their interpretation. I take it—but I am no anatomist—that the modern view is that he had.

To some extent De Graaf's originality was vindicated in English eyes during the subsequent wrangle, especially after De Graaf had sent to London the testis of a dormouse prepared in his own special way. But because of this wrangle his second and more important work on the female reproductive organs and the mammalian ovum (as he saw it) seems to have had a cool reception in England. Hence also, perhaps, the exaggerated fervour of Swammerdam's letter that I quoted just now. Confidence in their own national priority appears to have convinced English physicians and anatomists that they had nothing to learn from foreigners in the theory and anatomy of reproduction. The similar and far more serious case involving Newton is well known in the history of mathematics.

With Malpighi there was no such sad history. Technically, in the study of plant anatomy Nehemiah Grew had priority over him; William Croone could claim if not priority at least independent observation of the chick in the unincubated stage of the egg.[45] But the situations were different from those of De Graaf. Oldenburg, as our *Correspondence* shows, handled them with great tact, whereas previously he had submitted to Clarke's authority. Moreover, though Malpighi was by no means of a placid phlegmatic temper, he was not minded to make priority an issue; he was confident in the originality and importance of his observations. It was soon evident that while Malpighi's and Grew's study of plant tisssues did not produce violent conflict, they were in many ways different—in fact Malpighi's is much superior. Hence Oldenburg after much soft-pedalling when he finally despatched a copy of Grew's *Anatomy of Vegetables Begun* to Malpighi at Bologna could predict that

> you may assure yourself [by examining the book itself] that you have developed this investigation most worthily by another method and also extended your observations further.

Hence, he goes on, the Royal Society was most anxious to have from Malpighi his drawings elucidating the text, so that Malpighi's essay could be properly printed in

[44] See *Correspondence*, V, 268–72, Clarke to Oldenburg, 20 December 1668 and the many subsequent exchanges between De Graaf and Clarke via Oldenburg. Also Malpighi's letter of 10 November and notes (Vol. VII, 243–45).

[45] Croone's paper *De formatione pulli in ovo* was mentioned by himself on 29 February 1671/72 and read on 28 March; it is printed in Birch's *History of the Royal Society*, III, 30–40. Malpighi's first embryological essay had been read on 22 February: see *Correspondence*, Vol. VIII, Letter 1879, Malpighi to Oldenburg 22 January 1671/72, and subsequent correspondence, also Birch, *History*, III, 16.

London.[46] As for Croone, as an embryologist he was not in the same league with Malpighi. This was at once pointed out with great fairness by John Wilkins when Croone stated his case:[47]

> The Bishop of Chester desired that, notwithstanding this, Signor Malpighi might have the honour of this discovery, since Dr. Croone had never brought into the Society an account or a figure of this discovery, as Signor Malpighi had now sent to them an accurate description of this discovery, accompanied with very neat and laborious schemes.

We now know that Croone's observation was quite false, based on an accidental conformation of the vitelline membrane within the egg.[48]

If I have dwelt at some length on the investigation of reproduction and embryology it is because this investigation figures largely in the correspondence of the late 1660s and early 1670s with which we have been concerned in the last few years. I cannot also consider the scraps of information concerning the study of respiration, of muscle-nerve action, of histology, and of the brain, since time does not permit. It would be interesting too to review the attitude of English physicians to the iatromechanical theory developed by Descartes, Bellini, and G. A. Borelli. Then, at a more directly medical level, there is the promising episode of the attempt to find a really effective styptic, but this we have not come to yet in our work. I need hardly add that such questions of medical history cannot be studied in our *Correspondence* alone, but must be followed in the publications of the men concerned, Birch's *History*, and the *Philosophical Transactions*, as well as in much other correspondence which is not our immediate concern. Nor have I touched on the history of the relationship of English medicine—or sometimes the frustration of an attempted relation—with such distinguished foreign investigators as Rudbeck and Thomas Bartholin, Steno,[49] Pecquet, the *Academia curiosorum* of Leipzig, and so forth.

In conclusion, may I say how grateful I am for this opportunity to convey to medical historians something of the interest for them which may lie in the fruits of the labours which have engaged my devoted wife and myself for over a decade; it has been sometimes an arduous task, and therefore one hopes a profitable one. The wheels of scholarship grind slowly, and it is only after a long lapse of time that one begins to perceive that the bread one has cast upon the waters is nourishing the ducks. In this lecture I have of set purpose touched on a multitude of facets of medical history to catch your attention, and omitted a great deal of agonising detail. Let me with my last words ask your indulgence; in our edition of the *Correspondence of Henry Oldenburg* we have doubtless omitted much, and made many errors with respect to bibliography, medicine, physiology, anatomy, zoology, and botany. In committing at least two million words to paper in eleven years not every one can be beyond reproach.

[46] See *Correspondence*, IX, Letter 1969, Oldenburg to Malpighi, 26 April 1672.
[47] Birch, *History*, III, 17.
[48] F. J. Cole, *Early Theories of Sexual Generation*, Oxford, 1930, p. 47; Joseph Needham, *A History of Embryology*, Cambridge, 1934, p. 146. Malpighi's relations with the English physicians are also considered in his biography of Malpighi by Howard B. Adelmann, *Marcello Malpighi and the Evolution of Embryology*, Ithaca, Cornell University Press, 1966, Vol. I.
[49] See Dr. F. N. L. Poynter's recent papers, 'Nicolaus Steno and the Royal Society of London', *Analecta Medico-Historica*, 1968, 3, 273–80; and 'Italian Doctors and the Royal Society' in *Communicazione presentata al XXI Cong. Int. di Storia della Medicina, 1968*, Rome, 1969, 325–33.