

A Linnaean Thesis concerning
Contagium Vivum:
The 'Exanthemata Viva' of
John Nyander and its Place in
Contemporary Thought

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With a New Translation by

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I
The Thesis in Context
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In his book *The conquest of epidemic disease* (1943), Charles-Edward Amory Winslow noted that:

by 1700 there was available theoretical and observational evidence which should have made possible the formulation of our modern germ-theory of disease. Kircher had advanced the concept of a *contagium animatum*. . . Redi had presented convincing evidence that living things . . . were not spontaneously produced. . . Leeuwenhoek had actually described and figured the protozoa and bacteria in the human mouth and intestine. If an open-minded . . . observer had put the work of these three pioneers together, the germ-theory of disease could have been developed in the seventeenth century instead of the nineteenth.¹

Although these words were published more than fifty years ago, succeeding scholars have not attempted to answer Winslow's implicit question of why "the germ theory of disease"

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¹ Charles-Edward Amory Winslow, *The conquest of epidemic disease: a chapter in the history of ideas*, Madison, University of Wisconsin Press, 1943, repr. 1980, pp. 159–60.

was not developed until the mid-nineteenth century, nor have they seriously challenged Winslow's contention that no earlier medical author attempted to put the work of Kircher, Leeuwenhoek and Redi together in a single synthesis.

Winslow himself noted that Linnaeus presented in the *Systema naturae* six doubtful kinds of "living molecules" that included the "contagion of eruptive fevers", the "cause of paroxysmal fevers", the "moist virus of syphilis" and the "septic agent of fermentation and putrefaction", but he followed Clifford Dobell in dismissing these without further discussion as peripheral to Linnaeus's work.² It would appear, however, that Linnaeus was in fact very interested in furthering speculations about living contagion and instigated the production of several theses on the subject which were published in his edition of student dissertations, the *Amoenitates academicae*, or "Academic pleasures".³ Of these, the most important is the '*Exanthemata viva*', submitted by John Nyander in 1757, and republished in the *Amoenitates* in 1760. The thesis refers to both Kircher and Leeuwenhoek.⁴ Apparently, this dissertation was the only work published under Nyander's name. I have not been able to discover anything at all about the named author, although it is possible that research in Sweden might uncover additional information.

Earlier Evaluations of the '*Exanthemata viva*'

The '*Exanthemata viva*' and other Linnaean works bearing on the same subject are not entirely unknown to historians. Several English-speaking scholars in the first half of the twentieth century referred to the dissertations in the course of larger works on the development of science during this period, implying that they consulted the Latin text in the *Amoenitates*. Aside from Dobell himself, who mentioned the thesis in 1932 in his classic work on Leeuwenhoek, these scholars included Charles Singer in his privately printed history of the concept of *contagium vivum*, published in 1913, and William

² Ibid., p. 159, quoting from Clifford Dobell, *Antony van Leeuwenhoek and his 'Little Animals'*, New York and London, John Bale, Sons and Danielsson, 1932, pp. 377–8. The classification appears in the 12th ed. of the *Systema naturae* (1767).

³ See below for a discussion of the extent to which Linnaeus himself should be considered the "real" author of the work. These books have a tangled publication history. The first volume of the *Amoenitates* was edited by Peter Camper and published at Leiden by Cornelius Haak in 1749. Linnaeus republished vol. 1 at Stockholm, with Lars Salvius, and at Leipzig with Godfried Kiesewetter in the same year. He edited vols 2–7 himself and published them between 1751 and 1769 with Lars Salvius in Stockholm, and with J Wetstenius in Leiden. Vols 2 and 3 appeared in a second, revised, edition in 1762 and 1764 respectively. After his death, his student John Christian Daniel Schreber edited vols 8–10 at Erlangen from 1785–1790, and republished the original seven volumes. Vols 1–3 were also translated into German. Selected theses were translated and published in English by Benjamin Stillingfleet as *Miscellaneous tracts*

relating to natural history, husbandry and physick in 1762. Later these were translated and published in English by J Brand as *Select dissertations from the Amoenitates academicae, a supplement to Mr. Stillingfleet's Tracts*, in 1781. This was supposed to be a two-volume edition but evidently only one ever appeared. Neither the German nor the English translations included the '*Exanthemata viva*'. See J M Hulth, *Bibliographia Linnaeana*, Uppsala, Almqvist & Wiksell, 1907, pp. 65–74, and Heinz Goerke, 'Linnaeus' German pupils and their significance', in Gunnar Broberg (ed.), *Linnaeus, progress and prospects in Linnaean research*, Stockholm, Almqvist & Wiksell, and Pittsburgh, Hunt Institute for Botanical Documentation, 1980, 223–39, on pp. 232–3.

⁴ Johannes C Nyander, Calmariensis, '*Exanthemata Viva* . . . Praeside . . . Doct. Carolo Linnaeo', Upsaliae, L M Hojer, 23 June, 1757, repr. *Amoenitates academicae; seu dissertationes variae, physicae, medicae, botanicae*, ed. Caroli Linnaei, Lugduni Batavorum [Leiden], Wetstenium, 1760, vol. 5, 92–105. Subsequent references are to Dr Cain's translation in Part II of this paper.

Bulloch in his *History of bacteriology*, published in 1938.⁵ In addition, the work was translated into Swedish in 1940 by Yngve Hedlund, who also attempted to identify all the authorities cited by the thesis.⁶ Several other Swedish scholars have mentioned the thesis in the course of more general works on Linnaeus.⁷

However, the work has been generally neglected by scholars for three reasons. First of all, it has been published only in Latin and Swedish. There is no need to discuss the barrier that a Latin text presents to many potential modern readers, and even to many of Linnaeus's contemporaries. Linnaeus's Latin, moreover, is often obscure and idiosyncratic. Although there is a contemporaneous English translation in manuscript among the Heberden papers at the College of Physicians in London, there is no evidence that any previous scholar has consulted it or has even known of its existence.⁸ Unfortunately, this manuscript translation was not accurate enough to publish as it stood, so Professor A J Cain has now provided in the second part of the present paper a completely new translation, thus overcoming the first difficulty for anglophone scholars.

Second, the work of the members of Charles and Dorothea Singer's generation has fallen out of favour with scholars. Referring to later work carried out by the Singers on Fracastorius's theory of contagion, a scholar noted as recently as 1990 that "until the present . . . no attempt was made to go beyond the Singers' data, nor were the consequences of their investigation followed up".⁹ This appears to be as true for their work on contagionism as it is for their study of Fracastorius. There is little modern interest in the early history of the theory of *contagium vivum*, or "living contagion" itself, and the approach used by the Singers and their followers in describing it has become unfashionable, being seen as naive, positivist, whiggish, or progressivist.¹⁰ Instead, recent

⁵ Dobell, op. cit., note 2 above, p. 378, citing William Bulloch, 'History of bacteriology', in *A system of bacteriology in relation to medicine*, London, Medical Research Council, 1930, vol. 1. This chapter was later expanded and published as *The history of bacteriology*, Oxford University Press, 1938, repr. New York, Dover Publications, 1979; references to Linnaeus and Nyander are on p. 37. Charles Singer, *The development of the doctrine of contagium vivum, 1500–1750: a preliminary sketch*, London, for the author, 1913, p. 14. This short and extremely rare work deserves republication.

⁶ Yngve Hedlund, 'Linnés avhandling Exanthemata Viva', *Svenska Linnésällskapets Årsskrift*, 1940, pp. 39–51.

⁷ See, e.g., Sten Lindroth, 'The two faces of Linnaeus', in Tore Frängsmyr, (ed.), *Linnaeus, the man and his work*, Berkeley and Los Angeles, University of California Press, 1983, esp. pp. 47–8, and essays in Broberg, op. cit., note 3 above, including Albert Johan Boerman, 'Linnaeus and the scientific relations between Holland and Sweden', on pp. 45–6, and P Smit, 'The zoological dissertations of Linnaeus', on pp. 122–3. See also O T Hult, 'Om Linné Och "Den Osynliga Världen"', *Svenska Linnésällskapets Årsskrift*, 1934, pp. 118–28, on p. 121, and Heinz Goerke, *Linnaeus*, trans. by Denver Lindley, New York, Charles Scribner's Sons, 1973, pp. 121–2. I have not read

Ernst Almquist, 'Linné und die Microorganismen', *Zeitschr. für Hygiene und Infektionskrankh.*, 1909, **63**: 151–76 or Frederik Berg, 'Linnés Systema Morborum', *Uppsala Universitets Årsskrift*, 1957, **1**: 1–132.

⁸ Royal College of Physicians, London, MS. 345. The translation, entitled 'The living Efflorescences, by John C. Nyander. Upsala June 23, 1757', appears in a small notebook that was among the "Heberden papers" presented by LeRoy Crummer. It contains several translations of Linnaean theses, with other essays in a contemporary hand. William Heberden Sr. was a member of the Pringle-Fothergill circle but no evidence ties him to this MS. I thank the RCP for permission to copy and refer to this MS and Ernest Heberden for advice on its provenance.

⁹ Vivian Nutton, 'The reception of Fracastoro's theory of contagion: the seed that fell among thorns?', *Osiris*, 2nd Ser., 1990, **6**: 196–234, n. 4, on p. 198.

¹⁰ A referee of this article described Winslow and Charles Singer as authors, "who in this context were concerned merely with awarding credits to precursors of the modern germ theory and debits to those who failed to see that such theories were 'right'", but see also Nutton, op. cit., note 9 above, on p. 198, n. 4, who finds their work "pioneering", and "remarkable for its day".

scholarship has stressed the significance of environmentalism in eighteenth-century medical theory and has turned away from any effort to study the development of the idea of *contagium vivum* within medicine during this period.

Third, those scholars who named Linnaeus at all in this context did so only to dismiss his theories, arguing that his contribution to the understanding of disease was nugatory or perhaps even negative. Such criticisms centred on two issues: first, that the ideas were not original because theories of animate contagion have a long ancestry, possibly dating from classical times and more certainly championed by Fracastorius in the sixteenth century and Kircher, along with many others, in the seventeenth. Second, that Linnaeus's own formulation of these theories was confused, hesitant and inadequate. Charles Singer himself described the theses as a travesty of the careful work of earlier decades.¹¹ Dobell remarked that "Linnaeus and his pupils never understood Leeuwenhoek's 'little animals', and all their attempts at systematization merely created confusion". More recently, the Swedish historian Sten Lindroth commented that the idea itself was not new, since it was first propounded by Fracastorius, and the suggestion that the agent of disease was a mite was "scarcely a step forward, but rather a crude simplification".¹²

Linnaeus's Place in the Development of Ideas about Contagium Vivum

Space does not permit a full discussion here of the work of Singer and his colleagues nor of the importance of the history of contagionism. In articles on British medicine published elsewhere, I have argued that contagionism became increasingly prevalent in the second half of the eighteenth century and that this had important effects on the shaping of eighteenth-century medical theory and practice, regardless of the "correctness" of the theory. Contagionism influenced both the development of particular forms of investigation and the way in which individual manifestations of illness were categorized.¹³

When the Linnaean works were compared to the elegant and painstaking work of Leeuwenhoek, it is no wonder that parasitologists were unimpressed. Linnaeus was no microscopist, and his understanding of microbiology was indeed confused and inadequate. Because Linnaeus was the acknowledged master of the methods of classification, however, the fact that he was also involved in promoting a contagionist disease theory is of interest, regardless of the adequacy of his own delineation of the theory.

Furthermore, the importance of this work to the history of medicine was greater than the comments of earlier scholars would suggest. This thesis did indeed articulate a "germ theory of disease" in a way that had not been developed by medical authors before the eighteenth century and that evidently impressed some contemporary readers. To argue this is also to argue that the history of the idea of *contagium vivum* has been misconceived and thus to claim that there are inadequacies in our current views on the historical development of the life sciences and medicine.

¹¹ C Singer, *op. cit.*, note 5 above, p. 14.

¹² Dobell, *op. cit.*, note 2 above, p. 378; Lindroth, *op. cit.*, note 7 above, p. 47.

¹³ Margaret DeLacy, 'Puerperal fever in eighteenth-century Britain', *Bull. Hist. Med.*, 1989, 63: 521–56, esp. 546–55; *idem*. 'The

conceptualization of influenza in eighteenth-century Britain: specificity and contagion', *Bull. Hist. Med.*, 1993, 67: 74–118, pp. 111–12; and *idem*, 'Influenza research and the medical profession in eighteenth-century Britain', *Albion*, 1993, 25: 37–66.

It may be true that theories of *contagium vivum* were in the air, but they were rarely published in a definitive manner. Historians still debate the extent to which Fracastorius's work propounded such a theory. He undoubtedly believed in active contagious particles, but did not characterize them unambiguously as living entities. His treatment of them often suggests that he thought of them more as a chemical catalyst.¹⁴ Moreover, both Fracastorius and Kircher believed in spontaneous generation. Thus, not only the "*vivum*" but also the "*contagium*" is a problematic term in their formulations of *contagium vivum*. If animalcules could appear spontaneously, the substances that caused disease could be generated anew either in the sufferer's body or in the ambient world. Therefore, even if these substances were alive, it was always possible to explain new cases of disease without reference to case-to-case transmission. As Singer commented, "Neither the doctrine of a *contagium vivum* nor the allied doctrine of the specificity of infections could find a firm intellectual basis while the doctrine of spontaneous generation was in the ascendant".¹⁵

As long as physicians believed in spontaneous generation they did not need to specify whether the particles they had in mind were living: there was no absolute boundary between organic and inorganic. After the doctrine of spontaneous generation was challenged, physicians were forced to choose, and most chose to depict fermentation and putrefaction as chemical rather than vital processes.

For example, Richard Mead, the best-known contagionist author of the early eighteenth century described contagious matter as consisting of "a kind of Fermentation . . . a volatile active Spirit"; language that to a modern reader might suggest a living substance, but a few pages later, Mead referred to this matter as "an active Substance, perhaps in the Nature of a Salt", making it clear that he viewed the activity as chemical rather than vital.¹⁶ When eighteenth-century writers commented on earlier works, they often assumed that earlier authors had viewed fermentation and putrefaction as chemical processes. In citing older theories of disease transmission, the '*Exanthemata viva*' described Fracastorius as favouring putrefaction as a cause of disease, and correctly noted that Kircher believed that sickness came from "a putrefaction that produces worms". Neither was cited as a proponent of a doctrine of living contagion.¹⁷

¹⁴ Hieronymi Fracastorii, *De contagione et contagiosis morbis et eorum curatione, libri III*, trans. and annot. by Wilmer Cave Wright, New York and London, G P Putnam's Sons, 1930, *passim*, and see Charles and Dorothea Singer, 'The scientific position of Girolamo Fracastoro (1478?–1553) with especial reference to . . . his theory of infection', *Ann. med. Hist.*, 1917, 1: 1–29, esp. p. 12. My interpretation coincides with that of the Singers, but see also p. 211, n. 51, in Nutton, *op. cit.*, note 9 above.

¹⁵ C Singer, *op. cit.*, note 5 above, p. 13, and see also *idem* and D Singer, *op. cit.*, note 14 above, p. 12.

¹⁶ Richard Mead, *A short discourse concerning pestilential contagion, and the methods to be used to prevent it*, London, Sam. Buckley and Ralph Smith, 1720, pp. 11 and 17.

¹⁷ See Part II, below, p. 175. The inclusion of the "septic agent of fermentation and putrefaction" in the class of "living molecules" in 1767 suggests that

Linnaeus himself later thought that fermentation might be a vital process. On Kircher, see the exchange between William A Riley and Fielding H Garrison following the publication of Riley's communication 'Early references to the relation of flies to disease', in *Science*, 18 Feb. 1910, 31(790): 263–4, with later letters by Garrison on 1 April 1910, pp. 500–2 and 3 June 1910, pp. 857–9, and Riley, on 29 April, 1910 in vol. 31(800): 666. Dobell, *op. cit.*, note 2 above, pp. 366–9, and C Singer, *op. cit.*, note 5 above, pp. 9–11, both discuss Kircher; Dobell scornfully. See also Luigi Belloni's 'Athanasius Kircher: Seine Mikroskopie, die Animalcula und die Pestwürmer', *Medizinhist. J.*, 1985, 20: 58–65. An English writer, Marchamont Nedham or Needham (1620–1678), quoted Kircher at length, but I have found no evidence that Linnaeus ever read Nedham, who should be distinguished from the microscopist John Turberville Needham (1713–81), to whom Linnaeus does refer.

By the 1730s and 1740s, earlier theories of animalcular contagion had fallen into obscurity. Indeed, Singer claims that nothing of real value on the question of living contagion appeared after 1725 until the work of Pasteur in the nineteenth century.¹⁸ Yet the mid-eighteenth century was precisely the time when the arguments of Leeuwenhoek and Redi against spontaneous generation had gained brief but widespread acceptance, thus providing a new context for ideas of *contagium vivum*. Linnaeus himself was an avowed opponent of spontaneous generation.¹⁹

For physicians who accepted the contention that animals could not appear spontaneously, the question of whether the matter that caused disease was animate gained a new urgency: if morbidic substances were chemical then they could perhaps be generated from fortuitous combinations of matter, from dirt or human effluvia. If, on the other hand, pathogens were living matter, then even bad conditions could not generate disease *de novo*, but only serve as a favourable environment for the growth of a particular germ. Only if disease was seen as the result of the invasion of living particles did the denial of the spontaneous generation of *diseases* follow directly from a rejection of the spontaneous generation of *animals*.

Thus, an important element of the '*Exanthemata viva*' is the fact that it clearly identifies specific diseases with different species of animalcula: animalcula that were to be defined by their parentage, not by the conditions that bred them. By implication, Linnaeus was rejecting the spontaneous generation of eruptive diseases, as well as the spontaneous generation of animals. Another important feature of this thesis is the way that it ties the theory of contagion to the ability of particular pathogens to remain viable outside a host. To summarize, it implied that all eruptive diseases were caused by living pathogens, that each disease was specific to its causative organism, and that, because of differences in the viability of these organisms, some of these diseases could be transmitted *only* by contagion, at least in certain climates, whereas others could be caused by organisms that persisted in particular sites such as wooden drinking vessels. I have not studied the work of earlier authors with Singer's thoroughness, but it appears that no seventeenth-century author drew all these conclusions together. Similar theories were propounded by just one British author of the early eighteenth century: Benjamin Marten, but this work was not generally known at the time.²⁰ The thesis is also notable for

¹⁸ C Singer, op. cit., note 5 above, p. 15.

¹⁹ Erik Nordenskiöld, *The history of biology, a survey*, New York, Tudor Publishing Co., 1946, p. 210. See also John Farley, *The spontaneous generation controversy from Descartes to Oparin*, Baltimore, Johns Hopkins University Press, 1977, p. 1–30.

²⁰ Benjamin Marten, *A new theory of consumptions: more especially of a phthisis or consumption of the lungs*, London, R Knaplock, A Bell, J Hooke and C King, 1720, and see Charles Singer, 'Benjamin Marten, a neglected predecessor of Louis Pasteur', *Janus*, 1911, 6: 81–98, and Bulloch, *History*, 1938, op. cit., note 5 above, pp. 32–6. Marten drew on the work on parasitology by Nicolas Andry de Boisregard, *De la génération des vers*, which appeared in 1700 and also rejected the idea of spontaneous generation. See Farley, op. cit.,

note 19 above, p. 19. Marten's work must have found readers, because a second edition appeared. I have not found any mention of it in the work of Linnaeus or his British associates. The American campaigner for inoculation, Cotton Mather, quoted Marten with approval in his 'Angel of Bethesda', but this MS remained unpublished and unknown to contemporaries. Several of Marten's contemporaries, such as Richard Bradley, Thomas Fuller, and Jean Baptiste Goiffon, also published contagionist works, but these differ significantly. Fuller, for example, believed that pathogens bombarded the earth from the upper atmosphere. See his *Exanthematologia: or, an attempt to give a rational account of eruptive fevers*, London, Charles Rivington and Stephen Austen, 1730, p. 77. It is unlikely that Linnaeus, who read neither English nor French, had read these vernacular works on contagion.

suggesting that the life cycle of the animalcules might account for certain disease phenomena such as the paroxysms of some diseases, and that pathogens might survive for a long time hidden as "eggs".

In addition, 'Exanthemata viva' was first published in 1757, immediately after the introduction of smallpox inoculation into Sweden, which occurred between 1754 and 1756.²¹ Indeed, the introduction of inoculation may have encouraged this line of thought, since inoculation showed that a febrile exanthematous disease was caused by a tangible substance that could be seen and manipulated. Moreover, since inoculation caused only smallpox and no other diseases, it also encouraged efforts to differentiate between fevers.²² Bonomo and Cestoni had identified the itch mite as the source of scabies in 1687 and their work gained a wide audience when Richard Mead published it in the *Philosophical Transactions* in 1703.²³ However, most physicians considered local cutaneous diseases such as scabies to be entirely different from eruptive febrile diseases such as smallpox. Thus, the introduction of inoculation for smallpox precipitated an intellectual as well as a medical upheaval.²⁴ Whether the arguments of the thesis itself had been influenced by the debates over the introduction of inoculation, the practice would certainly have been in the minds of those who read the *Amoenitates*, and must have lent the thesis additional plausibility.

Thus, however confused and inadequate they may appear to us to be, these speculations concerning animalcular contagion appeared in a new context and at a critical time. This theory was soon echoed by the Viennese contagionist Marcus Antonius Plenciz, whose collected works appeared in 1762, but Plenciz does not seem to have attracted the same attention. Plenciz was familiar with some of Linnaeus's work, as any scientist of his period would be, but he does not refer to the *Amoenitates* and probably developed the idea independently, drawing on many of the same sources used by Linnaeus. Plenciz generally depicted contagious matter as "seeds", rather than "animalcules".²⁵

Authorship of the Theses

Although Linnaeus's views were sometimes controversial, his authority was immense, and the fact that the theory appeared under his name attracted a new audience for these speculations. There can be no doubt that Linnaeus himself was sympathetic to Nyander's theory, but the question of actual authorship is not as straightforward as it might at first appear. In the eighteenth century it was customary for Continental doctoral supervisors to

²¹ Donald R Hopkins, *Princes and peasants: smallpox in history*, University of Chicago Press, 1983, p. 51. Inoculation is mentioned in passing in the thesis itself, see p. 184.

²² See Genevieve Miller, *The adoption of inoculation for smallpox in England and France*, Philadelphia, University of Pennsylvania Press, 1957, and DeLacy, 'Conceptualization', note 13 above, p. 82, n. 22.

²³ Richard Mead (trans.), 'An abstract of part of a letter from Dr. Bonomo to Dr. Redi concerning some observations concerning the worms of humane bodies', *Philosophical Transactions*, 1702/3, 23: 1296–9. On this subject see also R Hoeppli,

Parasites and parasitic infections in early medicine and science, Singapore, University of Malaya Press, 1959. On p. 43, Hoeppli notes that Hildegard of Bingen also thought that itch mites ("suren", perhaps Linnaeus's "sirones") caused skin diseases.

²⁴ Miller, op. cit., note 22 above, pp. 271–6.

²⁵ Marcus Antonius Plenciz, *Opera medico-physica, in quatuor tractatus digesta, quorum primus contagii morborum, ideam novum una cum additamento . . . Secundum de variolis, tertius de scarlatina*, Vindobonae [Vienna], Johannis Thomas von Trattner, 1762. See also Bulloch, *History*, 1938, note 5 above, p. 37.

be closely involved in the composition of theses and to take full responsibility for their arguments. These might be taken from lecture notes or even dictated to the candidate, whose main responsibilities were to translate the thesis into passable Latin, pay for its publication, and defend it in a public debate. Linnaeus was not an exception to the general practice: indeed, he sometimes referred to passages in the *Amoenitates* as his own work and he used the dissertations as a vehicle for the early publication of certain sections of his works on taxonomy including his work on the classification of disease.

Moreover, Linnaeus himself later edited these dissertations, changing certain passages, and published them in the *Amoenitates*, tying them even more closely than was common to his personal authority. A modern historian has concluded that “The authorship of the Linnaean dissertations poses no problem: the author is Linnaeus . . .”.²⁶ This is the consensus of modern scholars and was also understood at the time: contemporaries who quoted from these theses commented that they represented Linnaeus’s own views.²⁷

Nevertheless, some grounds for doubt remain. First, not all theses in the *Amoenitates* had been written at the same stage in the academic process. At Uppsala, medical candidates presented two theses: the first, known as a *dissertatio pro exercitio*, was intended to demonstrate the fact that the candidate was fluent in Latin: the second, or *dissertatio pro gradu*, qualified the candidate to receive a doctor’s degree; the latter usually being headed “*pro gradu doctoris*”.²⁸ According to one scholar, 146 of the theses published in the *Amoenitates* were *pro exercitio* and only 40 were theses *pro gradu*: most of these were on medical subjects.²⁹ The headings in the *Amoenitates* do not distinguish between the two types of thesis: the title pages of some of the original theses state that they are “*pro gradu*” or “*pro gradu doctoris*” but others do not proclaim their status at all.

It is universally assumed that Linnaeus himself wrote his own dissertation, for the doctorate that he obtained from the University of Harderwijk, and it is at least possible that Linnaeus allowed more latitude to candidates writing doctoral dissertations than to other candidates. This question has apparently not been studied by other scholars: my own work has not uncovered a consistent difference between the theses headed “*pro gradu*”, and the others.³⁰

²⁶ Frans A Stafleu, *Linnaeus and the Linnaeans: the spreading of their ideas in systematic botany, 1735–1789*, Utrecht, International Assoc. for Plant Taxonomy, 1971, p. 144. See also Goerke, *op. cit.*, note 7 above, p. 116, but see also P Smit, p. 119, and n. 6, p. 130 in Broberg, *op. cit.*, note 7 above.

²⁷ See, e.g., Pringle, *Observations*, 5th ed., pp. 249 and 255 n, *op. cit.*, note 52 below.

²⁸ This information was provided by Carol Clausen, Librarian at the National Library of Medicine, who read it from Sten Lindroth, *A history of Uppsala University, 1477–1977*, Uppsala University, 1976. I have not been able to obtain a copy of this work. See also W T Stearn, ‘Introduction’ to Carl Linnaeus, *Species plantarum: a facsimile of the first edition 1753*, 2 vols, London, Ray Society, 1957, vol. 1, pp. 51–5. Unless otherwise stated in the text, theses cited were not described as “*pro gradu*”, or “*pro gradu doctoris*”, on the title page.

²⁹ John Ramsbottom, ‘*Caroli Linnaei Pan Suecicus*’, *Trans. Bot. Soc. Edinburgh*, 1957, 38: 151–67, on p. 151.

³⁰ I am indebted to Professor Tore Frängsmyr of Uppsala University for assistance and information on the sequence of degrees at the university. In a private communication, he has commented that “we can never be sure when it does not say if [a thesis was] ‘*pro exercitio*’ or ‘*pro gradu*’, but we can assume that in these cases it is ‘*pro exercitio*’ . . . It is impossible to know how much a student participated in the writing of the dissertation, and it is impossible to know if he was more or less active in the *exercitio* or *gradu* dissertation. The only thing we know . . . for sure is that the ideas were [Linnaeus’s].” Frängsmyr believes that Linnaeus should be considered to be the author of all the dissertations.

The 'Exanthemata Viva' of John Nyander

In any case, the 'Exanthemata viva' is one of the unlabelled theses and thus probably was a thesis *pro exercitio* for which Linnaeus was unquestionably responsible. Moreover, the thesis itself states that it was Linnaeus who suggested to Rolander that his dysentery might be due to identifiable animalcules and encouraged him to investigate. There are also several other theses in the *Amoenitates* that include references to a *contagium animatum*. For example, an earlier thesis by Michael A Baeckner on harmful insects, ascribed many cutaneous diseases to the Acari including "herpes", "serpigo", "elephantiasis", and tinea. Baeckner also suggested that dysentery, syphilis, measles, smallpox, typhus ["petechia"], plague, and other exanthematic and contagious diseases should be traced to Acari.³¹ A thesis "*pro gradu doctoris*" of 1765 on 'Lepra' attributed this skin disease, which may have been leprosy, to exanthematic animalcula, probably derived from a parasite of fish, the sea hair-worm. In language reminiscent of other theses, the author added that it could hardly be doubted that herpes, "serpigo", tinea, syphilis, measles, smallpox, and plague were due to subtle animalcula from the analogy of their eruptions, from their multiplication by heat and repulsion from cold, and from their susceptibility to treatments that expelled insects.³² Giving an example also used in the 'Exanthemata viva', the author noted that Dr Schreiber freed the Russians from the plague with mercury.³³ Indeed, he continued, it would be difficult to explain how contagions spread except by living animalcula; sometimes phthisis, hemoptysis and peripneumonia could also be contagious, and might be due to similar causes.³⁴ He referred in passing to the work of Baron Munchausen on spores.

Finally, a thesis of 1767 on "the invisible world", by J C Roos discussed the findings of biologists such as Réaumur, Leeuwenhoek and J T Needham on micro-organisms. It mentioned the theories of physicians concerning the origins of exanthematic and contagious fevers and again took up the same arguments: the comparable effects of heat and cold, scents and stenches, sweets and bitters, and mercurials on both disease and animalcula. It also again referred to the theory of Munchausen that the fungal spores that caused grain diseases were really the eggs of animalcula. However, its author noted that many investigators had examined the matter from smallpox, searching for living animalcula, but had not succeeded in finding anything.³⁵

Variations and Contradictions in the Theses

Although the examples and comments in all of these theses suggest that a single sensibility was behind their composition, other theses took a different, and in some cases contradictory, view of the generation of disease. For example, in the 1757 thesis 'Morbi

³¹ Michael A Baeckner, 'Noxa insectorum', thesis, Uppsala University, Holmiae [Stockholm], Laurentii Salvii, 1752, p. 9. Page references are to the original printed theses, not the *Amoenitates* reprints. I thank the Huntington Library, San Marino, California, for providing copies. See also Brand, op. cit., note 3 above, p. 379. There are no modern equivalents of the skin diseases named in these theses: for example "elephantiasis", could refer to a wide range of modern disease categories including leprosy and filariasis. See Hoeppli, op. cit., note 23 above, pp. 27–38.

³² Isacus Uddman, 'Lepra', thesis, Uppsala University, 1765, p. 8.

³³ Ibid.

³⁴ Ibid. On other eighteenth-century proponents of fish consumption as a cause of "leprosy" see Major Greenwood, *Epidemics and crowd-diseases: an introduction to the study of epidemiology*, London, Williams and Norgate, 1935, pp. 254–5.

³⁵ Johannes Carolus Roos, 'Mundum Invisibilem', thesis, Uppsala University, 1767.

expeditionis classicae', which appeared in the same volume of the *Amoenitates* as '*Exanthemata viva*', P Bierchen stated that the principal diseases of the North Sea naval expedition were diarrhoeas, "Upsala fever" [perhaps typhus/typhoid], and scurvy.³⁶ Bierchen discussed the hierarchy of causes that lay behind illness, including immediate causes such as obstruction of the blood and predisposing causes such as debilitation. He blamed salted meat, want of exercise and the use of fat and lard, which were difficult to digest, for the prevalence of scurvy. The fevers were due to impeded perspiration, combined with cold, damp, foul air and inadequate clothing. These disorders were contagious and were cured by emetics. Dysenteries were due to a saline and putrid acrimony in the food, bad air, and want of exercise. Underlying these were impure air and homesickness ("nostalgia"). The theoretical frame of reference was clearly derived from the work of Boerhaave who had depicted disease in terms of a mechanical breakdown.³⁷

This is common eighteenth-century fare, but it stands in contrast to the contemporary claim of '*Exanthemata viva*' that these traditional theories of disease causation should be reconsidered. '*Exanthemata viva*' attributes dysenteries to "an internal itch of the intestines" due to minute "acari" and criticizes the tendency of physicians to attribute "the general causes of all diseases" to a "corrupted mass of blood", or an "evil predominating in the air".³⁸

Among the other diseases '*Exanthemata viva*' specifically attributed to animalcular contagion is plague, but a thesis "*pro gradu doctoris*" of 1765 entitled '*Morbi artificum*' stated that physicians contracted epidemic diseases such as plague, petechial fever [typhus], dysentery, intermittent fevers and coughs from the bad air of patients' rooms and from the melancholy caused by brooding.³⁹ This was the same constellation of bad air and depression as had been offered by Bierchen, apparently unchanged by the intervening years and theses. Similarly, '*Morbi artificum*' attributed dysentery and "Hungarian continued fever" [perhaps typhus] among soldiers to adulterated food and water and "exanthematic fevers" [probably also typhus] among sailors to the stench of bilge water.⁴⁰

Linnaeus's own 1735 doctoral dissertation on malaria, reprinted in the first volume of the *Amoenitates* in 1749, attributed the prevalence of "intermittent fever" to water impregnated with clay and dismissed as causes such popular culprits as sudden chills, lack of exercise, anxiety, and moist wind from the sea or from swamps.⁴¹ Yet the 1757 doctoral thesis on the '*Febris Upsaliensis*' by Andreas Bostrom, published in the same year as the '*Exanthemata viva*' argued that "intermittent" and "exacerbating" fevers were due to moist, fetid air and standing water, and the 1771 doctoral thesis '*De varia febrium*

³⁶ Petrus Bierchen, '*Morbi Expeditionis Classicae MDCCLVI*', thesis, Uppsala University, Upsaliae, L M Hojer, 1757, pp. 4–5.

³⁷ *Ibid.*, and see Christopher Lloyd and Jack L S Coulter, *Medicine and the navy, 1200–1900*, vol. 3: 1714–1815, Edinburgh and London, E and S Livingstone, 1961, p. 299. Linnaeus had studied with Boerhaave.

³⁸ See Part II, p. 185.

³⁹ Nicholas Skragge, '*Morbi Artificum*', thesis, Uppsala University, 1765, p. 6.

⁴⁰ *Ibid.* The phrase was "*a fetore aquae alcalinae navis*". "*Morbus Hungaricus*", came from an epidemic that began there in 1566. Typhus and

typhoid were not distinguished until the nineteenth century, and were confused with many other diseases, so the identification is tentative. See August Hirsch, *Handbook of geographical and historical pathology*, trans. Charles Creighton, 3 vols, London, New Sydenham Society, 1885, vol. 2, pp. 548–9. See also Edmund Berkeley and Dorothy Smith Berkeley, *Dr. John Mitchell: the man who made the map of North America*, Chapel Hill, University of North Carolina Press, 1974, pp. 80–1.

⁴¹ See P C C Garnham (trans. and annot.), 'Linnaeus' thesis on malaria in Sweden', in Broberg, *op. cit.*, note 3 above, pp. 80–97.

intermittentium curatione' argued that intermittent fevers were caused by foul or acid air which interfered with perspiration.⁴² Such views are similar to those expressed in the Bierchen thesis of 1757 and the doctoral thesis on workers' diseases in 1765, but differ considerably from the views expressed in the theses of 1752, 1757, 1765 and 1767 which support a theory of *contagium animatum*.

These variations suggest that more than one medical sensibility shaped the arguments in the theses. Given the chronology, it is unlikely that the divergence can be explained simply by assuming that Linnaeus changed his mind over the years, since there is no obvious shift from one theoretical framework to another. It is not that Linnaeus started with a theory of "clay" and moved through animism to miasmaticism. Rather, we find him making the same assumptions at different stages in his development, while simultaneously putting forward different interpretations of the same diseases.

It is not unusual to find conflicting assumptions about disease causation in the work of a single eighteenth-century author, or even to find contradictory assumptions within a single work. It is also possible that the cause of the apparent contradictions was the confusion of Linnaeus's own taxonomy. It was in this very period that Linnaeus was attempting to construct an improved nosology: from about 1749 to 1759 he was using his own classification in his medical teaching before publishing it as a doctoral thesis entitled '*Genera morborum*' in 1759.⁴³ (It was republished in volume VI of the *Amoenitates* in 1763 and also separately by Linnaeus in the same year.) This was a revision of the classification system developed by François Boissier de Sauvages. Linnaeus attempted to modify Sauvages' symptomatic system to include the causal role of various agents including contagion, but the ultimate effect was confusion. Moreover, it is likely that Linnaeus had only limited personal clinical experience of some of the epidemic diseases he was describing and it is evident that his use of terms was often very imprecise.

Whereas most mid-eighteenth-century authors saw a complete divide not only in symptoms but also in etiology between malaria, or "intermittent fever", and the other fevers, such as typhus or smallpox, it often is not clear that Linnaeus is making the same distinction. Thus, the "Upsala fever" seen by Bierchen in the North Sea expedition was probably typhus; the thesis uses other common synonyms for typhus such as "nervous" fever.⁴⁴ On the other hand, the "Upsala fever" of Linnaeus's own doctoral dissertation was an intermittent fever associated with marshes and was probably primarily malaria. A few years later, the doctoral thesis '*Febris Upsaliensis*' describes the disease as a "semi-tertian" that frequently changed its form with the season and became petechial and contagious.⁴⁵

Some of the apparent contradictions in Linnaeus's etiology, therefore, might be due to transformations in his own nomenclature, but even when this is taken into account, it still

⁴² Andreas Bostrom, '*Febris Upsaliensis*', thesis, Uppsala University, Upsaliae, L M Hojer, 1757, pp. 5–9, and Petrus C Tillaeus, '*De varia febrium intermittentium curatione*', thesis, Uppsala University, Upsaliae, Edmannianis, 1771, p. 16.

⁴³ Richard Pulteney, *A general view of the writings of Linnaeus*, 2nd ed., London, J Mawman, 1805, p. 140.

⁴⁴ Bierchen, op. cit., note 36 above, pp. 4–5: "*Febri Upsaliensi* . . . quae alias *Amphimerina catarrhalis maligna*, *Febris mesenterica*, *nervosa*

anglorum, quin immo *stomachalis* plerumque appellari solet. . . quibus petechiae apud nonnullos & vibices accedebant."

⁴⁵ Bostrom, op. cit., note 42 above, pp. 2–3: "quibusdam annis typum suum mutavit, interdum admodum fuit malignus cumque petechiis, quo in casu contagiosus, indeque nomine *febris petechizantis* insignitus fuit, interdum stomachum maxime affecit, & dictus fuit *febris mesenterica* sive *catarrhalis*; a nonnullis autem *febris nervosa*".

seems more likely that different hands and different sensibilities created some of the contradictions and variation of emphasis in the theses. Linnaeus's greatest interest and real reputation was in botany although the structure of science during that period had compelled him to become a physician and to supervise medical dissertations; it seems reasonable to assume that he permitted his students a freer hand in subjects that were peripheral to his own botanical works and allowed them to defend their own ideas or pursue their own interests. This is especially likely in cases where the students were drawing on their own medical experience, as in the case of Bierchen who actually served as the physician to the Swedish fleet during the North Sea expedition described in his thesis.⁴⁶ Overall, although we may assume that Linnaeus approved the medical theses and took final responsibility for their arguments, it may be overstating the case to assume that he was the sole author and to discard the names of the student authors entirely, even though this was sometimes done by contemporaries and even by Linnaeus himself.

Linnaeus and Animalcular Contagion

Even if the authorship of the theses is uncertain, there can be no doubt about Linnaeus's own acceptance of the hypothesis of animalcular contagion. He never publicly defended the hypothesis *in propria persona* although there is one tantalizing reference to the subject in his oration on insects, which was delivered in 1739 and published in volume II of the *Amoenitates*.⁴⁷ At the very end of his speech, he discusses the subject of harmful insects such as the cheese mite and the codling moth and then adds,

but who can enumerate their multiplied tribes? the Supreme Disposer of all Things gives his command to those minute Animalcules the Sirones, and the whole man becomes one loathsome contagion: not to mention those ministers of disease and death who bring down upon us the plague, small pox, spotted fever, and other infectious and spreading disorders. — Our time is elapsed we must come to a conclusion.⁴⁸

It seems evident from this abrupt ending that Linnaeus did accept the hypothesis, and was willing to hint as much, but was reluctant to commit himself to it personally in such a way as to require him to defend the idea. Linnaeus probably realized that a full airing of the hypothesis of *contagium animatum* would embroil him in a heated and probably endless medical controversy. He was already suspected of being not quite sound theologically,⁴⁹ and an impassioned defence of a theory of living contagion would have led his colleagues to fear that he had, so to speak, "gone off the deep end". Such ideas were generally associated with a handful of visionary enthusiasts, many of whom were political or theological revolutionaries.

Linnaeus had enough to do in defending the central tenets of his botanical system against determined critics and had no reason to involve himself in defending speculations that would only diminish his own authority. So he encouraged his students to speculate on this subject, and published their views under his own name as "pleasures". Contemporaries accepted them in the way that they were intended to be understood: as the

⁴⁶ Pulteney, *op. cit.*, note 43 above, p. 411.

⁴⁷ *Trans.* in Brand, note 3 above, pp. 309–44.

⁴⁸ *Ibid.*, p. 340.

⁴⁹ See A J Cain, 'Was Linnaeus a Rosicrucian?', *The Linnean*, 1992, 8: 23–44. See also Goerke, *op. cit.*, note 7 above, pp. 68–9, and Lindroth in Frängsmyr, *op. cit.*, note 7 above, p. 51.

trial balloons of a man acknowledged to be an expert in an allied field. Nevertheless, there is evidence that they took an interest in these essays and that a few medical authors were prepared to take this argument for *contagium animatum* seriously.

Linnaeus's Influence in Britain

The extent to which binomial classification has permeated modern botany has perhaps obscured the extent to which all of Linnaeus's work was contested at the time it appeared. Nevertheless, it made an immediate impact and proved particularly successful in Great Britain, where it gained rapid acceptance among a small group of botanical enthusiasts who took responsibility for diffusing it further. Many of these botanists were physicians; naturally enough since botanical knowledge was central to an adequate understanding of the *materia medica*. Especially prominent was a small coterie of men interested in all aspects of the new science: men who became both contagionists and medical reformers. It was natural that physicians who were already interested in Linnaeus's botanical work would also take an interest in the medical papers of the *Amoenitates*. Both the Edinburgh Medical Society and Edinburgh University purchased a set of the *Amoenitates*, including volume V, the volume containing Nyander's thesis, so it was readily available to many scientists who received their medical training there.⁵⁰ It is possible that it was in these essays that they encountered speculations on *contagium animatum* that led them for the first time to take the theory seriously, although none of them was prepared to commit himself to it entirely.

The most striking case is that of John Pringle, physician-general to the Army, whose book *Observations on the diseases of the army*, published in 1752, rapidly became a medical classic. A well-connected Scottish Unitarian, Pringle later became a Fellow of the College of Physicians (1763), baronet (1766), Physician to the King (1774) and President of the Royal Society (1772).⁵¹ As an army physician, Pringle became deeply interested in the problem of the etiology of contagious diseases, and two years after settling in London in 1748 he served as a consultant to the government during a prison epidemic of typhus. He became an acknowledged expert on putrefaction, which he saw as a chemical process. In the fourth edition of his *Diseases of the army* (1764), however, Pringle commented that he had been inclined to attribute the spread of dysentery to "a putrid ferment", but after having seen Linnaeus's thesis "in favour of Kircher's system of contagion by *animalcula*, it seems reasonable to suspend all *hypotheses*' till that matter is further inquired into".⁵²

Pringle then added in a note a long extract in Latin from the '*Exanthemata viva*'; the extract consisted of most of the section on dysentery, including the story of Rolander. Although most physicians were supposed to be fluent in Latin, many were not, and it is

⁵⁰ *Index librorum Societatis Medicae Edinensis*, Edinburgh, Philosophical Society, 1766, Bodleian Library, Gough, Scot. 263(3). *Catalogus librorum, ad rem medicam spectantium in Bibliotheca Academiae Edinburgensae secundum auctorum nomina dispositus*, Edinburgh, Balfour and Smellie, 1773.

⁵¹ Dorothea W Singer, 'Sir John Pringle and his circle', pts. 1, 2, and 3, *Ann. Sci.*, 1949–50, 6: 127–80, and pp. 229–61; Sydney Selwyn, 'Sir John

Pringle, hospital reformer, moral philosopher, and pioneer of antiseptics', *Med. Hist.*, 1966, 10: 266–74.

⁵² John Pringle, *Observations on the diseases of the army*, 4th ed., London, A Millar, D Wilson, & T Durham, 1764, pp. 257–8 and pp. 265–6. The reference to Linnaeus was also in later editions such as the 5th of 1765, the 7th of 1775 and the "new edn." of 1812. See also Selwyn, op. cit., note 51 above.

quite possible that few bothered to read the long note.⁵³ Nevertheless, this publication marked an important moment in the history of British disease theory. The hypothesis of *contagium animatum*, which had long lived in the shadows, had been encouraged, if not endorsed by one of Britain's leading physician-scientists. Moreover, Pringle clearly attributed the hypothesis to Linnaeus, one of Europe's leading scientists, and a man whose work was greatly respected in Britain.

Linnaeus, who had visited England in 1736, was in frequent correspondence with other members of Pringle's circle who were also interested in both contagionism and botany: indeed, he may have derived some of his medical views from them instead of the other way around. For example, Pringle's friend and ally, the Quaker botanist Dr John Fothergill, had published in 1748 a study of epidemic sore throat that concluded it was caused by a "putrid virus or miasma sui generis" and spread by contagion through the breath. Fothergill was an intimate friend of the Quaker merchant Peter Collinson, who was Linnaeus's principal British correspondent.⁵⁴ In 1750, Fothergill, writing to an American botanist, mentioned both his own recently completed treatise on contagion and the first volume of the *Amoenitates*, which had just appeared and was dedicated to Collinson.⁵⁵

Fothergill's amanuensis and protégé, John Coakley Lettsom, shared a house with Collinson when he first arrived in London. Like Fothergill, Lettsom was a contagionist, a reformer, and a keen botanist.⁵⁶ His doctoral dissertation, dedicated to Fothergill, was on the tea plant, a subject discussed in the *Amoenitates*. His work led to a correspondence with Linnaeus himself. Another associate of Fothergill's was Dr Richard Pulteney who contributed a letter to Fothergill's survey of the influenza epidemic of 1775, to which Pringle had also contributed.⁵⁷ In 1781, Pulteney published *A general view of the writings of Linnaeus*, which provided a summary of every Linnaean publication including the *Amoenitates*. Of Nyander's thesis he commented that it was "ingenious, and well worthy the attention of all those who wish to be acquainted with the doctrine it favours".⁵⁸

Another British physician who wrote an important work on epidemiology in the later eighteenth century and who may have owed his introduction to the question of *contagium animatum* to the *Amoenitates* was Edward Whitaker Gray. Gray edited and digested a large collection of letters on the influenza epidemic of 1782 for the Society for Promoting Medical Knowledge, and produced a book that argued strongly that influenza was contagious.⁵⁹ Little is known about Gray except that he came from a family of seed

⁵³ Graduates of residential programmes such as those at Oxford and Cambridge were usually fluent Latinists. However, many physicians bought degrees after a very perfunctory process. Some even purchased their theses. The French psychiatrist Pinel was said to have supported himself by ghost-writing theses.

⁵⁴ R Hingston Fox, *Dr. John Fothergill and his friends: chapters in eighteenth century life*, London, Macmillan, 1919, ch. 13, pp. 157–81.

⁵⁵ John Fothergill to James Logan, London, 4.3.1750, in Betsy C Corner and Christopher C Booth (eds), *Chain of friendship: selected letters of Dr. John Fothergill of London, 1735–1780*, Cambridge, Mass., Belknap Press of Harvard University Press, 1971, pp. 137–8.

⁵⁶ James Johnston Abraham, *Lettsom: his life, times, friends and descendants*, London, William Heinemann, 1933, pp. 72–5.

⁵⁷ DeLacy, 'Conceptualization', note 13 above, p. 115. For further discussion of the medical and scientific network during this period see D Singer, op. cit., note 51 above, and DeLacy, 'Influenza research', note 13 above. The 2nd ed., of Pulteney's *General view of the writings of Linnaeus*, note 43 above, contains an excellent biography of Pulteney, written by William George Maton, on pp. 1–30.

⁵⁸ Pulteney, op. cit., note 43 above, p. 419.

⁵⁹ See DeLacy, 'Conceptualization', op. cit., note 13 above, and 'Influenza research', op. cit., note 13 above.

suppliers. He became the Curator of Natural History at the British Museum and incurred some opposition for arranging the exhibits according to the Linnaean classification. Gray's brother, Samuel Frederick, translated Linnaeus's *Philosophia botanica* into English.⁶⁰

Other British physicians who were presumably familiar with this thesis include Sir James Edward Smith, of Norwich, who translated one of the theses from the *Amoenitates* and later founded the Linnean Society in London; John Rotherham Jr., of Newcastle, who studied with Linnaeus in Uppsala and later taught at Edinburgh and St Andrews; and William Withering, an enthusiastic Linnaean, whose discovery of the value of digitalis in dropsy represents the summit of eighteenth-century medical botany.⁶¹ Withering was a close friend of Pulteney, whom he had met at Edinburgh University. In 1776, Withering published a botanical text on the British flora arranged according to the Linnaean classification. Later, he became a member of the Linnean Society, as did Pulteney, Rotherham and Gray. In 1778, Withering followed in Fothergill's footsteps by publishing *An account of the scarlet fever and sore throat, or scarlatina anginosa*. This provided the first unambiguous clinical depiction of scarlet fever. Withering stated that the disease was undoubtedly contagious and could be contained by isolating the patient, but he was unable to determine whether the cause was animate. Although he was undoubtedly familiar with the *Amoenitates*, however, his immediate source for the hypothesis was not the work of Linnaeus but that of Plenciz, who wrote a treatise on scarlet fever that attributed the disease to "seminal particles", which might lie dormant in the body or travel through the air.⁶²

Perhaps most important of all was the subtle but pervasive influence of Linnaeus on the work of William Cullen, particularly in his great work on medical taxonomy. Cullen never publicly espoused a theory of *contagium animatum*, but he did believe in contagion, attributing it to "human effluvia". Cullen's study of Linnaeus's work led him to a completely new understanding of disease classification based on the concept of individual "species" of disease that were each caused by a separate species of matter.⁶³

⁶⁰ See the entry for Gray in the *Dictionary of national biography*, London, Smith, Elder, 1908–9, and A E Gunther, *The founders of science at the British Museum, 1753–1900*, Halesford, Suffolk, Halesworth Press, 1980, pp. 30–7.

⁶¹ On Smith, see A Batty Shaw, 'The Norwich school of lithotomy', *Med. Hist.*, 1970, 14: 221–59, and Andrew Thomas Gage and William Thomas Stearn, *A bicentenary history of the Linnean Society of London*, London, San Diego etc., Academic Press for the Linnean Society of London, 1988, pp. 4–11 and 17–19; for Rotherham, see Richard Welford, *Men of mark 'twixt Tyne and Tweed*, London and Newcastle, Walter Scott Ltd, 1895, and for Withering, see Louis H Roddis, *William Withering and the introduction of digitalis into medical practice*, New York, Paul B Hoeber, 1936; John Fulton, 'The place of William Withering in scientific medicine', *J. Hist. Med.*, 1953, 48: 1–15; and T

Whitmore Peck and K Douglas Wilkinson, *William Withering of Birmingham, M.D., F.R.S., F.L.S.*, Boston, John Wright and Sons, and London, Simpkin, Marshall Ltd, 1950.

⁶² William Withering, *An account of the scarlet fever and sore throat*, Birmingham, M Swinney, and London, G G and J Robinson, 1793, p. 61, and see also Plenciz, op. cit., note 25 above.

⁶³ William Cullen, 'Introductory lectures on nosology', in *The works of William Cullen, M.D.*, ed. John Thomson, vol. 1, Edinburgh, William Blackwood, and London, T & G Underwood, 1827, pp. 449–61. See also Margaret DeLacy, 'Nosology, mortality and disease theory in the eighteenth century', paper given at the Conference on the History of Registration of Causes of Death, Bloomington, Indiana, November, 1993, issued as Working Paper #CD4 by the Population Institute for Research and Training, Indiana University.

Conclusion

Eighteenth-century physicians were familiar with Leeuwenhoek's observations, but Leeuwenhoek never claimed that the organisms he saw were a cause of disease. In arguing that microscopic animalcules were the cause of epidemics, Linnaeus and his students were piecing together the work of several seventeenth-century scientists. However, they were poor microscopists and Linnaeus's depiction of the suspect organisms was hesitant and confused. We do not know what it was that Rolander saw through his lens, nor whether it was responsible for his illness. It is possible that he saw actual mites, ingested them, and found them in his excreta but that they were not responsible for his illness. It is also possible, however, that he was suffering from an illness caused by *Cryptosporidia*, *Giardia lamblia*, *Entamoeba histolytica* or similar organisms, or a mixture of organisms, and that he had in fact identified the cause of his recurrent diarrhoea. For example, giardia are relatively large (about twice the size of a human red blood cell), can be spread by water and beverages, are similar in appearance to mites, are visible in the excreta of patients, tolerate acid, and can cause recurrent bouts of diarrhoeal illness. They were first seen by Leeuwenhoek in his own excreta in 1681, but for centuries they were considered to be merely commensals. However, both amoeba and giardia are usually ingested by patients in the form of cysts. At that stage they are not motile and would not resemble mites.⁶⁴

Rolander's case was perhaps the most conclusive evidence available to an eighteenth-century scientist, but it was considered insufficient by itself. Physicians were well aware of the diminutive size of the organisms that would be involved. They believed that their own skills and instruments could not provide a decisive conclusion on this issue. So they reserved judgement, but continued to report the hypothesis of *contagium animatum* as a legitimate possibility that deserved further investigation. In the meantime, they accumulated evidence that suggested that many epidemic diseases spread by case-to-case contagion. Both their conclusions and their evidence continued to receive a respectful hearing and provided the immediate intellectual background for the epidemiologists of the early nineteenth century.

Most doctors avoided any open commitment to the theory itself, but they certainly read both the *Amoenitates* and the work of Pringle, which became the standard book on the subject of military medicine. It was during this period that Edinburgh became one of the greatest medical schools in the Western world and British medicine gained universal respect; students from Edinburgh and London spread British medical ideas throughout the

⁶⁴ I thank David Clark, of the Dept. of Biology, Portland State University, for advice on mites, which can act as disease vectors, but rarely cause illness in Europe. I thank Margy Woodburn, head of the Nutrition and Food Management Department of the College of Home Economics and Education, Oregon State University, and Ernest Meyer, Professor of Microbiology at Oregon Health Sciences University for information on other potential pathogens. See also David P Stevens, 'Giardiasis', in James B Wyngaarden and Lloyd H Smith, Jr (eds), *Cecil textbook of medicine*, 16th ed., Philadelphia, London, W B Saunders, 1982, pp. 1746–7. Amoebiasis causes what is today known as "dysentery", but Linnaeus

was not necessarily using the word with its modern meaning. Some contemporary microscopes had enough power to reveal such pathogens. On microscopes associated with Linnaeus see Brian J Ford, *Single lens: the story of the simple microscope*, New York, Harper and Row, 1985, pp. 113–18; O T Hult, *op. cit.*, note 7 above; and W J Holland, *Address presented to the New York Academy of Sciences*, 23 May, 1907. I would appreciate further comment from readers on this subject, as Rolander's case may mark an early instance of a potentially justified identification of a particular pathogenic micro-organism as a cause of acute internal illness.

world. It seems likely that they also transmitted the debate over the hypothesis of *contagium animatum*, but there have as yet been no studies of this subject, nor of the prevalence of theories of *contagium animatum* in other parts of Europe during the eighteenth century.

Between 1600 and 1750, proponents of the thesis of *contagium animatum* had generally been men of little standing in the medical community and had not gained a respectful audience for their proposals. Pringle, for example, was familiar with Kircher's views, but had never found them persuasive, perhaps because Kircher was notorious for his credulity. It was the Linnaean formulation that caught his attention and gave the theory of *contagium animatum* a renewed respectability in medical debates. Despite its deficiencies, the 'Exanthemata viva' did in fact combine the achievements of Kircher, Leeuwenhoek and Redi into a hypothesis of animalcular contagion, and probably contributed to the dissemination of the theory during the eighteenth century.

II

Annotated Translation of the Thesis

A J Cain

The following translation is strongly literal, both to give the meaning more exactly, and to demonstrate the style in which it was thought necessary to deliver a public academical oration. The text used is that printed in the *Amoenitates academicae*, vol. 5, pp. 92–105, at Leiden (*Lugdunum Batavorum*), published by Wetstenius, 1760. As usual, Linnaeus, considering these theses as intellectually his own, altered it from the original printing as a separate thesis, adding footnotes, expanding the text, correcting minor errors, and making some omissions. The only significant omission is from p. 101 (1760 edition); a paragraph is dropped after that on the Syphilitic Diseases, which says: "We cherish the same opinion of the origin of *Petechiae and Purple*, also of *Uredo*, which in the absence of observation we must dismiss [i.e., not discuss further]".

Uredo is translated by Pulteney (see note 43, above) as Nettle Fever, *Petechia* as Spotted Fever (apparently typhus, see p. 167 above), both in the Class Exanthematici. *Purpura* is not mentioned, nor is it in Linnaeus's *Genera morborum*, nor his *Clavis medicinae duplex*; it may be a synonym of *Erysipelas*, St Anthony's Fire, also an exanthematic, or just possibly of *Bacchea*, Ruby-face; *Gutta rosea* of authors, in Class II, *Vitia*, Order 4 *Scabies*, cutaneous diseases. Apparently, Linnaeus changed his mind on the origin of these afflictions, but see p. 169 above. Linnaeus's additions to the text bring no new principles.

The *Amoenitates* page numbers are given between slashes, in their approximate positions, word order being often different in Latin.

I am much indebted to Gina Douglas, librarian of the Linnean Society of London for a xerox of the original printing, and to Margaret DeLacy for commenting on the translation's style and contents.

LIVING EXANTHEMAS,⁶⁵ WHICH [THESIS], MR. DR. CAR[OLUS] LINNAEUS PRESIDING,
Was proposed by JOHANNES C. NYANDER of Kalmar at Uppsala 1757. June 23

Of diseases, hardly any have been for physicians so abstruse as to origin as the contagious: in that a body, until then in perfect health, observing the Dietetic rules to the very unit,⁶⁶ if he merely comes close to a sick person, should be infected just as much as [he who is] abandoned to voluptuousness. The wonder is increased when we see some native contagious diseases, such as the Itch,⁶⁷ [Dog-] Madness,⁶⁸ wild Cough,⁶⁹ and Dysenteries, spring up as though spontaneously, almost every year with us; but others never without conversation⁷⁰ with the sick, the first origins of which [are] foreign, to be summoned otherwhence than from the fatherland: I am speaking of the Plague, Measles, Smallpox, and Syphilis.

It will help to report but only briefly the few conjectures of the Most Learned Physicians about the origin of these: some have cherished one opinion, others others: *Fracastorius*, *Rhodus*, *Mindererus* have deduced the cause of them from PUTREFACTION: from a sulphurous multiplied PUTREFACTION *Hoffmannus*: from a FERMENTING /93/ miasma *Junkerus*: from a certain CORROSIVE *Alpinus*: from an acrid VOLATILE SALT *Sylvius*: from an ARSENICAL VENOM *Sorbait*: from the IMAGINATION and terror *Rivinus*: others from the EXHALATIONS of the poisonous earth: others from the heavens and conjunction of the PLANETS: others from an abundance of CORPSES, infecting the air: from a putrefaction FULL OF WORMS *Kircherus*, [and]⁷¹ others. Of the causes alleged widely by authors, that one seems to us closest to the truth, in which it is declared that the contagion comes from living animalcules, as *Rivinus on the itching of exanthemas [caused] by mites*, [and] others. And so we shall enlarge [on this head], attempting to walk along this road no further than the guiding of our experience permits; but that which is believable [although] enjoyed by no eyes up till now, that we will bring forward in the fewest [words] as a probable conjecture, which we do not make our own [property], but will merely taste with the edges of our lips.⁷²

⁶⁵ Exanthemas. A Greek word, written in Greek by the Roman medical author Celsus, obviously as a technical term, and remaining as such thereafter in medicine. "Efflorescence" and "Eruption" were seventeenth- and eighteenth-century translations, "rash" was rather a newcomer in the early eighteenth century. It seems best to keep the word "exanthema" (with an Anglicized plural) as a technical term.

The thesis characterizes these exanthemas as alive because it propounds the new doctrine that all such are actually caused by living animalcules—in publicity-hungry circles today the title would be:

EXANTHEMAS ARE ALIVE!

The Latin is in fact ambiguous, *vivus* being equally "vivid", "brilliant" or even "ardent", but "living" is justified by the contents of the thesis.

⁶⁶ *ex asse* (*as*, a unit, coin). "Completely" (a legal term), "to the exact farthing".

⁶⁷ Itch. *Scabies*. Itching, as an irritation, is *pruritus*.

⁶⁸ [Dog-]Madness. *Rabies*.

⁶⁹ Wild Cough. *Tussis ferina*, whooping-cough (chincough in the eighteenth century).

⁷⁰ Conversation, *conversatio*. Frequenting of, meeting frequently with; also conversation in the modern sense.

⁷¹ The omission of the conjunctive particle "and" is deliberate, conforming to the classical figure of speech called *asyndeton* which was intended to make the movement of the sentence more rapid and vivid. The outstanding example is Caesar's "*veni, vidi, vici*". There are several occasions later on in the thesis where it is used to some effect; using it here is an unintelligent application, and it becomes a stylistic affectation.

⁷² edges of our lips. A reminiscence of Cicero's *De natura deorum*, I, 8, 20; in other words, "which we shall not state dogmatically, but merely dip into". A typical over-used literary adornment; also, a typical saving clause in propounding a thesis, so that if the opposition proves too strong, the proponent can claim that he said all along that his idea was only a conjecture. Mere thesis rhetoric here—Linnaeus would not have tolerated serious opposition.

CONTAGIOUS diseases, for the most part, agree amongst themselves in that (1) they blossom out into EXANTHEMAS internal or external, just as the Madness⁷³ shows itself by certain pustules beneath the tongue[s] of dogs; for in all contagious diseases the matter of the exanthemas is wont to be close at hand,⁷⁴ which breaking out mitigates the fever. *Hoffm. Syst.* IV. 122;⁷⁵ (2) that FEVERS also excite restlessness of the body,⁷⁶ either a minimum at a certain hour of the day, or they become worse towards the night; (3) that the violence of the disease is increased by SWEET, is expelled by BITTER, and is provoked by FATTY [SUBSTANCES]; (4) that it is repulsed to some degree⁷⁷ by COLD: and just as every living thing is revived by gentle warmth but is too much overthrown by heat; so by heat the Itch and the exanthema are intensified and, as the heat and fever increase, they are impelled to the lukewarm surface of the body; (5) that they are abolished by ANTHELMINTICS, and for that reason *Sulphurated* [medicines] remove the itch, and *Mercurials*, killing almost all insects, cure the Itch and Syphilis, preserve from the Plague /94/ and Smallpox: *Tobacco* kills the minutest insects, its fumigation averts infection; (6) ANALOGY and the itching of the pustules in contagious [diseases] intimate the same; (7) because in the Itch and Dysentery they [i.e. minute insects] have been manifested to the eyes: *Langius*⁷⁸ saw them in the MEASLES, *Kirkerus*⁷⁹ in the PLAGUE, in SYPHILIS (like Slugs) *Hauptmannus*, in PETECHIAE *Siglerus*, in SMALLPOX *Lusitanus & Porcellus*; this last saw little worms also in TETTERS and other skin diseases.

That it may be an easy matter for very minute insects of that sort, perhaps MITES of diverse species, to be the causes of diverse contagious disease, we shall believe, from analogy and experience so far acquired, nor are their structure and magnitude in opposition; for they are the tiniest animalcules which the human eye has been able to perceive so far. [That] Lynceus⁸⁰ *Leuwenhoekius* has observed, in such humours as the naked eye sees as wholly pure, thousands of insects which, taken together, hardly equal the hundredth part of a sand-grain. The most acute *Reaumurius* believed that in summer, what in the air is perceived to be denser, and glitters like a clear vapour, is none other than insects, that vanish from sight.⁸¹ The same [man] showed them to live not only separately, but also in a society, in the manner of Bees and Ants, each one preserving [an] order among themselves. One cannot conclude, therefore, from the [evidence of the] senses to the impossibility of the thing; the ingenuity of the Most Wise Founder shines out as much in the least as in the greatest [things].⁸² When we look into a living Mite, we shall be

⁷³ See note 68, above.

⁷⁴ close at hand. The verb *subesse, subsum* means both close at hand and concealed. The meaning here seems to be that the matter is both concealed and superficial; it can easily break out onto the very surface.

⁷⁵ Friedrich Hoffmann, *Medicinae rationalis systematicae*. Several editions of this were published 1718–1740 including one at Halle.

⁷⁶ restlessness. The same word, *inquietudo*, is used for feverishness in Marcus Empiricus, according to Charleton T Lewis and Charles Short, *A Latin dictionary*, Oxford, Clarendon Press, 1879, rept. 1980.

⁷⁷ to some degree. Or, to a noticeable degree.

⁷⁸ *Langius*, not *Longius* as in Hedlund's notes—no wonder he could not identify him. Dr DeLacy

points out to me that this is most likely to be Christian Lange the elder (1619–1662), author of *Miscellanea curiosa medica, annexa disputatione de morbillis, quam prodromum esse voluit novae suae pathologiae animatae, itemque de elixir proprietatis*. . . edita a Johanne Centurione Macasio, Leipzig, 1669. (For Hedlund, see note 6 above.)

⁷⁹ Kirkerus. Nyander's misprint for Kircherus.

⁸⁰ Lynceus. The Argonaut with a sight as keen as that of a lynx; hence, "the keensighted Leeuwenhoek".

⁸¹ that vanish from sight—presumably perceptible only in sunbeams.

⁸² as in the greatest. A very often repeated sentiment from Seneca.

persuaded that it is provided with muscles, nerves, veins, arteries and viscera: the belief is right that these, although minute to the last degree, contain nevertheless liquors suitably subtle, as to be able to pervade their vessels. Whence these animalcules come, we marvel, but the marvelling becomes inappropriate, when at the same time we consider that they, [being] of such small proportions as to be almost nothing, flit about in the air just like atoms, /95/ penetrate into the smallest chinks, no otherwise than Mould is born from seeds wherever there is [anything] putrid.

The ITCH

The ITCH is the commonest of all [contagious diseases], and most evident to the senses. It is clear to everyone that this is excited by the *Acarus Siro*,⁸³ which, lurking at the sides of the pustules, under a sort of spot, hardly to be perceived with the naked eye, when pushed out with a needle we can lay upon the finger-nail**.⁸⁴ Now if we cherish it with warm breath, and place it on the skin, it creeps most easily distinguished by its structure, for a long time it surveys the wrinkles, until, gnawing, like a Mole,⁸⁵ it goes beneath the skin, where it constructs burrows. The *Acarus F[auna] Sv[ecica]* 1195, although more coloured, is extremely like this. Not uncommonly we see cheese or flour, unmoved from its place for a long time, nourish many thousands of them; hence it happens, that when nurses in place of the Pollen⁸⁶ of Lycopodium, the Flowers of Zinc,⁸⁷ etc. sprinkle children suffering from chafing, with flour of grain, the groins and armpits so sprinkled, bloom into the Itch; which evil, however often cured, continually returns, as often as the sprinkling with flour is repeated, and infects other children. Hence we conclude that the mites of Flour and the Itch are one and the same species. Mr. *Zweib* has well observed these Mites to lay eggs, multiply themselves extremely quickly, and live many days away from the body*. For that reason we daily ascertain the Itch to be propagated by clothing, conversation,⁸⁸ and contact. As insects love warmth, will not suffer cold, so thus /96/ the itch is vehemently inflamed by heat, and restrained by cold. We⁸⁹ have personal experience that those infected with the Itch, if anointed with Sulphurated [ointments], are seized with either internal pain or fever, so that the same cause which externally [excites] the Itch, internally excites Fever also, which does not cease before the eruption reappears. By reason of this phenomenon, therefore, how [very] much is it permissible to

** The Mite is not to be sought under the pustule itself, it retires further, by following the wrinkle of the cuticle it can be observed; it lays its young in the pustule itself, which by scratching, nature thus impelling [us], we break open and sow [them] abroad.

* *Leuvenhoekius* not long ago taught and figured mites as viviparous.

⁸³ *Acarus Siro*. This is the correct nominative for Linnaeus's species.

⁸⁴ upon the finger-nail**—a convenient platform for observation, into which it cannot burrow. The printer has transposed the signs for the two footnotes, putting the double asterisks first instead of second.

⁸⁵ gnawing like a Mole; eroding the skin by burrowing like a Mole, a bad figure of speech since moles do not gnaw and are not rodents, as Linnaeus knew perfectly well. Poor quality thesis rhetoric.

⁸⁶ Pollen of Lycopodium. "Dust", or "fine flour". (The powder of clubmosses is actually of spores.)

⁸⁷ Flowers of Zinc. Zinc oxide as a powder.

⁸⁸ See note 70, above.

⁸⁹ We. The plural for the singular I is a very frequent Ciceronianism, now somewhat irritating because it is ambiguous—is the writer speaking generally, or of himself personally?

cherish [this] opinion of the causes of *exacerbating Fevers*,⁹⁰ which of themselves generate a vehement and very often contagious itch? For the like effect is wont to have the like cause, and nature, always like herself, does not easily make jumps;⁹¹ but we can hardly maintain our way further through such dense shadows.⁹²

In the *wild Itch* the Mites are discovered with difficulty, but those extracted [successfully] we perceive very clearly with armed eyes⁹³ to be a different species (*Acarus exulcerans*) distinct with four hind feet twice as long as the body: so that it cannot be that any should doubt the cause of it. We preserve clothing and Museums of insects from mites with Ambrosiacs,⁹⁴ for example, Musk, Civet, *Holcus odoratus*,⁹⁵ Camphor, oil of Birch-bark, which are also successfully given internally for the bringing-out of these exanthemas when they have been repressed. Children with musk hung around their necks are preserved from smallpox rioting around according to the experience of the Norlanders and Mr. President [Linnaeus]; the eastern Russians keep themselves from contagious diseases with musk sown into the borders [fringes?] of their garments, as is shown in the recent [St] Petersburg Transactions.

The same arguments apply to the *senile Itch* and experience of it also confirms Mites. When once it has overwhelmed the body, [after] having ceased spontaneously, it does not readily return.

A few years ago, we saw two Kids infected with Itch, especially in the mouth: one of them died, but the other, to which Musk had been presented, became healthy in the space of three days.

/97/

The DYSENTERY

Epidemic DYSENTERY is an internal Itch of the intestines, as appears from dissections of corpses that died of Dysentery; nor does anyone skilled in medicine hesitate whether it is propagated by privies and common sewers. *Bartholinus* tells of a Danish Physician, in the previous century at Helsingburg, very often attacked by Dysentery, who observed his bowel excreta to be full of living insects, moving themselves about by a hardly observable motion. At which point we must not omit an observation, greatly illuminating the matter.

⁹⁰ *exacerbating Fevers*. These, in Linnaeus's classification of disease, *Genera morborum* (1763), are remitting fevers, mainly (or all?) forms of malaria, which make the third Order of his second Class, Critical Fevers. In his scheme, extensively discussed in Pulteney's *General view* (note 43, above), the first class is the *Exanthematici*, elevated by Linnaeus to this dignity from being only an Order in Boissier de Sauvages' classification. The Itch, with which the *Exanthemata viva* begins its review of exanthematic diseases, is in Class II, Vitia, Order 4, *Scabies*, cutaneous diseases. Although Linnaeus expanded the thesis in several places when publishing it in 1760, he could hardly re-write it completely, which bringing it into conformation with his 1763 classification would require. Moreover, as that classification was especially by symptoms (on the grounds that the causation of diseases was too uncertain to afford a reliable basis, compare

Pulteney, note 43 above, pp. 139–41) it is possible that Linnaeus saw no contradiction; he was quite happy, and with good reason, to put out both an artificial and a natural classification of plants.

⁹¹ "Nature is always self-consistent, makes no jumps" is a classic tag, repeated *ad nauseam* and going back at least to Aristotle.

⁹² dense shadows—a reminiscence of Petronius, or perhaps a re-wording of a Virgilian phrase.

⁹³ armed eyes, i.e. the eye aided by a lens.

⁹⁴ Ambrosiacs. "Perfumes" is not correct.

Linnaeus, in the thesis *Odores medicamentorum*, distinguishes seven classes of odours of medicinal substances, of which the Aromatics and Fragrants are both sweet-smelling, the Ambrosiacs and Goatish pleasant to some persons, unpleasant to others. He gives no definitions, only (as here) examples.

⁹⁵ *Holcus odoratus*. *Anthoxanthum odoratum* L., sweet vernal grass, smelling strongly of coumarin.

Four years ago, Mr. *Rolander*, boarding in the house of O[ur] M[r.] President, was infested with Dysentery; he was cured with [preparations of] Rhubarb and Peregories in the accepted manner. Eight days later he fell into the same disease, and was similarly healed; but another eight days elapsing, he was seized by Dysentery a third time: the cause was sought by every endeavour, but not discovered; since the patient used the same table, and mode of life, as the healthy cohabitants. And so O[ur] Mr. President [advised] the patient, [who was] especially studious in Entomology, to investigate his excreta, by which it would appear more definitely, whether the observation referred to of Bartholinus held good, or not. This done, the patient declared that myriads of animalcules were seen by him in them, which [when] accurately described, were Mites, and like Mites of flour; several guessed the cause to be in a night-drink; which did not seem sufficient to others. He was unaccustomed to drinking between meals; so at night urged by thirst, from a cup made out of juniper wood, he took a very weak drink; looking into this vessel, he found a thin whitish line, hardly visible to the naked eye, in the chinks of the sides; with the armed [eyes],⁹⁶ he observed all this whiteness to be nothing other than innumerable Mites, and of the same species with those, which he had observed in the excreta; he discovered by repeated investigations, that when the drink was poured into the vessel, they did not change [position], but quitting [their] abodes, in the middle of the night, they sought the surface of the drink, where they looked for their food right up to the hour of ten /98/ a.m., when they sought again their previous places. When extracted Mites were placed on a dampened disc, he paid attention to how little they were irritated by various liquors sprinkled on, and that they went through oil itself in safety; they were harmed by spirit of wine, but most greatly by T[incture] of Rhubarb, which is worthy of note in the highest degree; for since Rhubarb is a specific for the Dysentery, and *Lapathum acutum*⁹⁷ [is] truly akin to it, and a daily used Medicine for the itch, we have discovered an affinity and analogy. They [the mites] would stick to a vessel, although thrice washed with hot water; he looked for them also in other places, and often found them in vessels of acid drink, and beneath the bungs of casks.

The Dysentery, which afflicts *Gyinge*, a district of Scania almost every year, at the time of harvest, equally with that common in [army] camps, perhaps draws its origin, from those very Mites, lurking in acid drinks, which are propagated from thence by privies, and engender contagion. Therefore in the O[ld] Test[ament], it is prescribed that every one of the soldiers should immediately bury in the ground his bowel excreta. Surely then it would help the Scanians and soldiers in camp to use drinking vessels of shell or metal, to which these insects stick with more difficulty? The common people are wont to declare, not knowing the cause, that it is wrong on a fasting stomach to take in a drink enfeebled by the night air, unless first the topmost liquor of the draught is poured away; the observation stated above shows that these Mites are then in motion, and hence was born the origin of this custom. By drinking flat liquids, colic of the stomach is excited, which, often, is curbed by spirits of Wine, which comes next to Rhubarb in power of damaging Mites.

⁹⁶ See note 93, above.

⁹⁷ *Lapathum acutum* is sharp-leaved dock. Linnaeus is arguing here by analogy within a *natural* Order; *Rumex* (docks) and *Rheum* (rhubarbs) are adjacent in his twelfth natural Order, Holoraceae, in the list of natural orders appended to the 6th edition

of his *Genera plantarum*, 1764. In several of his writings he insists that plants in the same natural group have the same or closely related medical properties. (They are only within the same Class, separated by several Orders, in his artificial system.)

The 'Exanthemata Viva' of John Nyander

The Wild COUGH

The disease wild COUGH was less known to our ancestors, by which [(the disease)] children are principally affected. /99/ Therefore, as far as it is epidemic*, the miasma of it must be such as is easily evaporated from the sick, propagated, and multiplied; but this we judge cannot be premised unless it is assigned to something living. The Domestic Medicine from an Infusion of the Herba Ledi**⁹⁸ which the Westrogoths employ against this evil, the power of which is narcotic, virulent and hateful to insects, intimates that insects are here as causes also. We may borrow, in relation to this, the excellent observation of Mr. Dr. Ass[essor] Wahlbom, who gave to a child in Kalmar, afflicted with this evil, Julep. Musk. Lond[on], by which, after two days, the whole body of the patient burst out in a light sort of Itch, and the wild Cough ceased; after some time, the same evil having returned, and the itch going inward, he administered the same remedy, from which he again obtained the same effect, and the child became completely healthy. Surely therefore we can derive the wild Cough from Mites of some species, devoted to the organs of the lung, to seek [there] principally [their] nourishment? since it is removed or alleviated by remedies of this sort. But why this, just as the Smallpox and Measles, infests our life a single time, and does not return, we are as ignorant as of why the senile Itch most often does not return.

The SMALLPOX

There is a close relationship between the SMALLPOX and the preceding. It is born from a cause in no way to be sought in Europe or America, for never is it transferred to us without contagion. It is most likely to be excited in the same manner, especially since children, who are fed too much on sweet [things], /100/ and endowed with tenderer humours⁹⁹ are more greatly infested with this evil, no otherwise, than with the Itch. A few years ago, when the Smallpox was most calamitous at Uppsala, almost all the children in the neighbourhood were carried off by it, O[ur] M[r.] President hung Musk about the necks of his children, as has been a long time the custom in Norrland: by which he brought it to pass, that they remained untouched by this pestiferous evil. It is an accepted custom, to keep warm the thighs of those recently infected with Smallpox, with a fomentation of sugared milk, by which the insects are as it were allured thither, for the thighs become as though roasted, with pustules, but the remaining part of the body is affected by a lesser multitude

* There are observations that healthy children going into a house and meeting with a sick one once, have also been infected with the wild Cough shortly thereafter. All Physicians assert that in visiting one patient after another, they have taken the miasma with them, and infected others with it.

** By the same plant Pigs & cattle, infected with lice, are cured.

⁹⁸ Herba Ledi is *Ledum palustre*, an ericaceous plant growing in acid bogs, looking (vegetatively) like rosemary although no relation to it—rosemary is a labiate. Linnaeus in the *Species plantarum* (1753) gives as a synonym: *Rosmarinum sylvestre* Cam. Epit. 546; i.e., Camerarius's augmented edition of Petrus

Andreas Matthioli, *De plantis epitome utilissima . . . aucta et locupletata*, a D. Ioachimo Camerario, Francofurti ad Moenum, 1586, p. 52. Presumably the name was still employed by apothecaries.

⁹⁹ Humours. Constituent body-fluids, not states of mind.

of Pocks. To prepare children against the Smallpox Mercurials and Sulphurated [medicaments] are administered, and Crocus¹⁰⁰ is applied to their eyes, lest they should be hurt, with which sailors colour their undergarments, against being more than usually tormented by lice. The Chinese, inoculating with Smallpox, add Musk to the pus, applied to the nostrils, in order to drive out by it the Mites, from the sanies,¹⁰¹ in the sinus of the forehead. Any wound in one labouring under Smallpox, will not be made firm before the evil quits the body, and the exantheas will be accumulated thither in the highest degree, by some live thing which also seems to be produced, which, impelled by internal injury, seeks it [the wound] as though an outlet and refuge.¹⁰²

The MEASLES

The MEASLES, by [their] nature and foreign origin, are truly related to the Smallpox. The seeds of *Aquilegia*, poisonous and resembling the seeds of *Staphisagria*,¹⁰³ are commonly administered against the Itch of children: and the little old women, ignorant [that they are] children, giving them the correct larger dose of it,¹⁰⁴ often kill [them]. It is extremely common with us, to administer to those labouring under Measles or Smallpox powdered seeds of *Aquilegia* or Musk, that the exantheas may be expelled to the surface of the body, lest the internal viscera should be too much contaminated with them. In the obstinate Cough and /101/ oppression of the chest of those labouring under measles, when almost nothing is of help, small and repeated doses of *Flo[wers]* of *Sulphur*, given at the same time as febrifuges, are named as a specific. In a word: all remedies, stealing up on Smallpox and Measles whether preventives or cures have the virtue of killing insects or at least not conveying their exhalation,¹⁰⁵ they [the insects] are forced to the outside of the body, and forced to flee.

The PLAGUE

That the PLAGUE with us never arises without contagion from foreign lands, above all in maritime cities: hence¹⁰⁶ the same is multiplied and propagated: that it rages worst in

¹⁰⁰ Crocus. Saffron from *Crocus sativus*, not a mineral such as crocus antimonii, so called from its similar colour.

¹⁰¹ *Sanies* is lymph, serum, pus or any other white or colourless animal or human fluid, in opposition to blood, but including "corrupted" blood. It was commonly used in Latin translations of Aristotle for the colourless or white liquid in invertebrate animals corresponding in function to the (red) blood of mammals, birds etc.

¹⁰² The Latin is slightly obscure in the last half of this sentence, perhaps intentionally if the proponent is only "touching the matter with the edges of the lips". The general sense, however, seems clear.

¹⁰³ *Staphisagria*. A species of *Delphinium*, Anglicised as Stavesacre, with very poisonous seeds, which, however, have been used "as an insecticide and a cure for neuralgia and toothache" according to A Huxley and W Taylor, *Flowers of Greece and the Aegean*, London, Chatto & Windus, 1977, p. 79.

John Ray, in his *Historia plantarum* (1686), p. 705, adds to these "it purges, however, with danger of strangulation, wherefore its use is discontinued".

Aquilega (columbine) must be poisonous indeed, if worse than *D. staphisagria*.

¹⁰⁴ The herb women presumably produce an adult's dose for the parents to administer to their children (otherwise they would see them and know they were children). The Latin is slightly obscure; I read *juste* for *justo*, but it may be that the herb women were ignorant of the correct size of dose for a child.

¹⁰⁵ Here, thesis rhetoric becomes very obscure. Literally the sense is "or at least not carrying their halitus", but this may be either "not conveying their effluvia" as in the eighteenth-century translation, or "not bearing their breath" implying that the insects are in danger of suffocation and so compelled to move.

¹⁰⁶ Hence, reading *hinc* for *hanc*. Perhaps not justified; it may be simply one more in the series of dependent clauses, but if so, the sense seems incomplete.

warm, putrid and low-lying places: that it gets worse by the aid of corrupt states of the air, is repressed towards winter, but breaks out again at the approach of spring and summer: that it does not enter houses thoroughly cleaned witnesses Mr. Timolus¹⁰⁷: that its approach, commonly, heralds other contagious diseases: that it always preserves the same exanthemas: that its miasma can as well be swallowed down in saliva as it can creep in through the pores of the skin: that it is weakened, by acid, in the air, prevented by acids, according to the experiments of Mr. Sylvius: lastly that it, having recently entered the kingdom of Muscovy, was successfully cured, according to Schreiberus, with Mercurials, famous remedies against the Acarine Itch: that Musk & Camphor serve as cures of the Plague, as the same [authority] reports, the observations are most true, which are seen to indicate insects, namely, as the cause of it.

We think the *Syphilitic* Diseases, especially since they never are born without a contact of fluids, by the mouth, cups, copulation and sweats, [having] an intimate analogy with the Plague, take their origin also from animalcules, but aquatic ones.

Hence it seems certain that [these] Fevers are excited in the same manner by Mites, since they are expelled by medicaments appropriate [to them].

/102/ From what has been said, we know wherefore the Itch and Dysentery, with us, often arise without contagion; for we find the cause of the former in flour of grain, of the latter in drinking vessels. In contrast, it is very likely, that the insects producing Plague, Smallpox, etc. are exotic, and cannot exist with us, unless imprisoned in an infected body, or in some other housing suitable to them. Perhaps the cause of this is to be sought in the severity of our skies, or in the tenderness of the nature of the Mites; for if it is right to infer the more subtle from the coarser, we observe some insects to be native with us, some to live [only] in the summer, some lastly not to live except in our dwellings, where the heat of the hearth sustains them: these last we judge to have journeyed from lands bordering more on the South; of them, not all have the same susceptibility; for we have taken here insects that have migrated hither from hottest India, and have taken out citizenship. The same happens also with those animals, of which the greatest multitude was found formerly in certain [restricted] places, whence they have spread themselves far and wide and put forth their dire effect. That some of these have become domestic with us, may perhaps explain; why certain diseases before unknown have arisen in certain places and [become] common. From this we may be led into a byroad, whereby the reason may be discerned, why certain diseases flourish at certain times of the year, beyond [the fact that] most contagious diseases bear sway in the summer, the three-month period most favourable for all insects taken together: [this] most tangled knot, arising from the ingenious economy of these animalcules, may perhaps be solved, by the wonderful analogy of exanthemas which we know: for by what means can a dead corpse break out into exanthemas always like itself,¹⁰⁸ [and] by the diversity of circumstances [they] seek the insides and outsides of the body in a definite manner? What more likely, than that organized animals should perform this? What more fit for this, /103/ than very subtle insects; the extraordinary mode of life which seized with admiration the greatest Priests of Nature?¹⁰⁹ What prevents these tiny

¹⁰⁷ Timolus. According to Hedlund, (see note 6, above), Timoni.

¹⁰⁸ always like itself. I am unable to explain this extraordinary statement.

¹⁰⁹ Priests of Nature, *Naturae Mystas*, a reminiscence of Ovid's *Fasti*; here, merely naturalists.

insects from having a residence in our body, since we cannot assert that we are exempt from larger ones: For we detect in the belly of one and the same man, insects of various kinds, in metamorphosis,¹¹⁰ complete insects and adults: see *O[ur] Mr. Chief-Physician and Knight Rosen in the [Stock]holm [trans]actions*. 1751. And furthermore¹¹¹ the very *Muscae carnariae*,¹¹² and others like them, insert their eggs to be hatched in our belly: the Medium-sized *Ascarides*, large *Lumbrici*, and long *Taenias*, enter into us, not with trivial harm.

The *Pulex penetrans* enters feet in america, excites malignant ulcers.

The *Gordius medinensis* enters the body in the indies, produces bone-wracking pains.

The *Furia infernalis*¹¹³ falls from the ether in Bothnica, on to the naked body of men, kills swiftly in a moment, as established by the observations of D. C. Solander. That *Minute worms* of the appearance of *Gordii* excite Elephantiasis and malignant Ulcers in Norway, Martinus observed recently.

It appears that these animals have principally two methods of insinuating themselves into the body, when we consider diseases springing up in internal parts of the body, e.g., when the contagion infects first the neck and stomach, they descend, without doubt, by breathing in: but it seems certain, on considering diseases, appearing first on the surface of the body, that they intruded themselves by way of the inequality and pores of the skin.

It is equally right to allot a definite time to these animalcules and others of their kinds, at which they eat, make love, are multiplied, sleep and rest; Whence the periodical paroxysms of these diseases can be /104/ explained; add [also], that the *Ascarides*, taking their food by gnawing, excite itching in the rectum at a certain hour of the day.

From the laws of nature it is established, that the smaller the animal, the more abundant is the progeny it bears, and since a single Bee, in a few weeks, can be increased to 20,000, it follows that these insects also, far smaller than they [Bees], will be increased in a most abundant proportion: and hence we may believe, that a mere single one or two of these insects will quickly produce so abundant a progeny, that it will blossom out and flood the whole body. We are taught also in this matter by inoculation, that a greater or lesser amount of pus has little significance for the number of Pocks.

Authors report, that the Plague, Smallpox, etc. reigning frightfully, have laid hidden for a long time, and at length, dispersed themselves by infected articles made of skin, linen and wool; nor is this a wonder: for what hinders us from believing that the eggs of these insects, may be preserved many years, entirely fresh, and thereafter, act in contagious manner, when opportunity is given them to engage with our body, if they have enjoyed only a moderate degree of heat or cold and a suitable dwelling-place? For it is known that the eggs of the Bombyx¹¹⁴ may be kept, in such a way, three whole years.

Having finished all their work, in the body, and exhausted their harmful labours, they must all die, or be dispersed from it; we would rather believe the latter, calling to mind indeed that the contagion, at the termination of the disease, becomes most intense, and the

¹¹⁰ Insects in metamorphosis, i.e. eggs, larvae and pupae, as against adults.

¹¹¹ And furthermore, *quid, quod*, a Ciceronian cliché.

¹¹² *Muscae carnariae*, unitalicized by the proponent, is probably to be translated literally as "flesh flies", but it is also a scientific name of a

species of fly, *Musca carnaria*, in Linnaeus's *Systema naturae*, and that is perhaps what is meant here.

¹¹³ A mythical worm invented by Linnaeus to account for a sudden inflaming of his arm, yet attested by others later.

¹¹⁴ Bombyx. Silk-moth.

insects signal, by the itching, and seeking the periphery of the body, a desire for as it were a better dwelling-place; for inasmuch as in their abodes near the surface, before departure,¹¹⁵ they have recently laid their eggs, the harmful progeny of them, whether under metamorphosis,¹¹⁶ or as perfect insects, is able perhaps to issue forth easily by transpiration. But why they do not frequently seek again the [same] body, and do not exert the same violent /105/ economy, is among [those] wonderful phenomena, which are evident to our eyes, [but] not to reason. It often turns out by experience, that nurses previously vexed by the Smallpox, handle children labouring under Smallpox, and apply them to their breasts, where the contagion appears, breaking out in them, as one or two spots: this indeed, so utterly devoid of all power, that it cannot excite any fever. There is a Difficulty in such ambiguous¹¹⁷ diseases, [one should] institute more accurate observations, equally in the accepted hypothesis of Physicians, of the general causes of all diseases, which some have asserted that they have found in a corrupted mass of blood, some in an evil predominating in the air, carried by we know not what wind, from one region to another, [the Difficulty] has been an obstruction by which these living causes of all exanthematic diseases, have not so far been detected: however, we will dare to assert awhile that [our] more accurate posterity will make acquaintance far more ingeniously with these animalcules than we have reckoned, or have dared to disentangle; for the judgment stands most firmly [based]: *nature is nowhere more perfect, more skilful, than in the tiniest things.*¹¹⁸

Last of all we beg that the G[ood] R[eaders] will wish to subject to further investigation these sketches such as they are, so that ways till now trodden by few, shall be disclosed.

¹¹⁵ *ante-obitum*; an ambiguous phrase meaning either "before an approaching" (the surface of the body?) or "before dying"; perhaps intentionally so, in view of what has just been said. There seems to be an ambiguity in the proponent's mind as to whether what disperse from the body are the adult insects (having laid eggs) or the progeny (the adults having died).

¹¹⁶ See note 110, above.

¹¹⁷ *anceps*. Ambiguous, uncertain, two-fold—perhaps the latter as occurring in a virulent and mild form.

¹¹⁸ A reference to Pliny.