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RINDERPEST AND MAINSTREAM INFECTIOUS DISEASE CONCEPTS IN THE EIGHTEENTH CENTURY

by

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Bacteriology became an established academic discipline with the discoveries and the subsequent classification of a number of pathogenic bacteria during the latter half of the nineteenth century. The first attempt to trace the historical background to this new development appeared as early as 1887 as a series of lectures given by Friedrich Löffler and dedicated to his mentor Robert Koch.[1] Löffler paid only scant attention to developments prior to 1800, although he was more appreciative of the work of Athanasius Kircher (1602–80) than many other commentators,[2] and devoted several pages to Leeuwenhoek's discoveries. His interest in the eighteenth century was limited to a brief mention of works inspired by the plague in Toulon, and an even briefer reference to Lancisi's views concerning malaria, in addition to a paragraph on Plenciz' theories of *contagium animatum* and their relationship to Leeuwenhoek's observations.[3]

Löffler's historical essays – and several later works[4] – contain no mention of the diseases specific to cattle which played havoc in Europe at a time when veterinary science hardly existed.[5] Medical authorities were forced to take a long, hard look at these diseases – bovine pleuropneumonia, foot-and-mouth disease, and, above all, rinderpest.[6] In the process, they learned much about the control of epizootics and

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[1] Friedrich Löffler, *Vorlesungen uber die geschichtliche Entwickelung der Lehre von den Bacterien*, Leipzig, Vogel, 1887. Isolation of individual pathogenic bacteria had become possible only with Koch's perfection of the pure culture method in 1881. Working in Koch's laboratories, Löffler was associated with many important early discoveries, and towards the end of the century with the first isolation of a filterable virus, that of foot-and-mouth disease.

[2] While it is unlikely that Kircher could have seen the plague bacillus in the blood samples he examined with his not very powerful microscope, he may have seen some protozoa and he almost certainly saw red blood corpuscles. If his interpretation of his observations was incorrect, he did introduce the concept of *contagium animatum* in infectious disease. For an objective evaluation of Kircher's *Scrutinium pestis*, see C. E. A. Winslow, *The conquest of epidemic disease*, Princeton University Press, 1943, reprinted University of Wisconsin Press, 1980, pp. 145–152.

[3] Löffler, op. cit., note 1 above, pp. 8–9; Lancisi's and Plenciz' theories, especially in relation to rinderpest, will be discussed in detail below.

[4] Cf. e.g. William Bulloch, *The history of bacteriology*, Oxford University Press, 1938. The impact of rinderpest and particularly of Cogrossi's work (see below) was, however, noted by Jean Théodoridès (*Un grand médecin et biologiste: Casimir-Joseph Davaine (1812–1882)*, Oxford, Pergamon Press, 1968, pp. 49–50).

[5] The first veterinary school was established at Lyons, France, in 1762, and others followed in rapid succession. Until then, the care of cattle had been the exclusive province of unenlightened shepherds and cow-leeches.

epidemics, and in a tentative way began to develop a comparative pathology of infectious diseases. Later ages were to find disadvantages as well as advantages when using animals for experimental work.[7] In the eighteenth century, the struggle against epizootics taught useful lessons in epidemiological control, especially with regard to isolation and quarantine, although one very important measure taken could never be used in human epidemics: the slaughter of all those affected or even suspected of being affected.

Rinderpest, then known as cattle plague, was introduced into the Venetian territories from Dalmatia in the summer of 1711,[8] from then on, its continued presence on the European continent throughout the eighteenth century had devastating results. The ever-present threat of rinderpest, and the efforts made to control it, are amply illustrated in the literature of the period. In addition, it inspired medical authors to reflect on the nature of the contagion and of the pathogenic process, frequently comparing the distemper[9] to that scourge of man in the same century, smallpox.[10] The more succinct of these observations clearly express the views on contagion and infection prevailing in the age of Enlightenment. In the case of infectious diseases it was an age perched, somewhat uneasily, between the philosophical approach to contagion of previous centuries,[11] and the radical changes which, backed by technical innovations, were to change the face of the study of infectious diseases irrevocably in the nineteenth century.[12]

During the latter half of the seventeenth century, two developments paved the way for more rational explanations of the hitherto inexplicable processes involved in the onset and development of contagious diseases, especially in epidemic and epizootic form. The importance of these two became obvious not just with hindsight; they were invoked at the time, again and again, in the relevant literature. One was

[6] The three diseases are not always easily distinguished in the early literature due to inadequate descriptions, but there can be little doubt that the particular outbreaks discussed below were of rinderpest. Contagious bovine pleuropneumonia is caused by *Mycoplasma mycoides*, while the other two are both virus diseases. The agent of foot-and-mouth disease is among the smallest of the known viruses, with a diameter of little more than 20 nm, while that of rinderpest is pleomorphic with an average diameter of between 120 and 300 nm.

[7] Animal experimentation was as controversial in the nineteenth century as it is today, culminating in this country in the debates concerning especially rabies experimentation at the end of the century, and the furious arguments surrounding the Royal Commission on vivisection, see e.g. *Br. med. J.*, 1908, **i**: 1183–1186.

[8] According to Gamgee, in 1709, the disease had begun to travel from Tartary westwards through Russia to Poland and Dalmatia, and from there spread throughout Europe. In the early years no fewer than 70,000 head of cattle were lost in Naples alone and 100,000 in Silesia, while the Netherlands reported a loss of 300,000 animals. (John Gamgee, *The cattle plague*, London, Robert Hardwicke, 1866, pp. 288–289.)

[9] Used freely and somewhat indiscriminately of a wide variety of febrile diseases of animals and man in the eighteenth century, "distemper" is now applied specifically to canine distemper, a virus disease of young dogs. Coincidentally, its virus is antigenically related to that of rinderpest.

[10] Analogies in the past were often selected because the physician was familiar with a particular disease prevalent at the time rather than because it bore any striking similarity to the entity with which it was being compared. (Lloyd G. Stevenson, 'Exemplary disease: the typhoid pattern', *J. Hist. Med.*, 1982, **37**: 159–181, p. 160.)

[11] V. Nutton, 'The seeds of disease: an explanation of contagion and infection from the Greeks to the Renaissance', *Med. Hist.*, 1983, **27**: 1–34.

[12] Improvements to microscopes assisted observation of micro-organisms after 1840. (Elizabeth F. Genung, 'The development of the compound microscope', *Bull. Hist. Med.*, 1942, **12**: 575–594.) See also note 7 above.

Leeuwenhoek's epoch-making microscopical observations which finally and firmly gave reality to a vast world of hitherto invisible *animalcules.*[13] The other was the discovery of the *acarus,*[14] the genus of mites causing scabies in man and mange in animals. Throughout the eighteenth and well into the nineteenth century, long after the existence of bacteria had become a well-documented fact, the *acarus* mite remained a basis for comparison, a point of reference among others in microbiology.[15] In the eighteenth century, it was the only one: if scabies were caused by a small creature, easily observed doing damage to the skin of the sufferers, would it not be reasonable to suppose that other contagious diseases with external or internal manifestations were caused by even smaller creatures, *animalcules,* invisible to the naked eye but eventually to be seen with Leeuwenhoek's microscopes, or with future even stronger versions of such instruments?[16]

When, in 1711, the virus of rinderpest[17] began its devastating progress among cattle herds around Padua, it found not only an abundance of suitable host animals, but also a human population in charge of these animals quite unprepared for outbreaks of epizootics on such a scale.[18] The sporadic occurrence of cattle plagues[19] in the seventeenth century had been overshadowed by the great epidemics of plague in man; and veterinary medicine as such was non-existent both in theory and practice apart from a few treatises on diseases of the horse, reflecting the preoccupation of the reading classes with this important transport animal. Within a few months of the onset of the cattle epizootic, which by October 1711 had enveloped the length and breadth of the Venetian territories and was raging with undiminished force, the Venetian Senate appealed to the supreme medical authority of the region, the medical faculty of the University of Padua.

[13] L. C. Palm and H. A. M. Snelders (editors), *Antoni van Leeuwenhoek (1632–1723),* Amsterdam, Rodopi, 1982.

[14] A number of attempts, of a more or less fantastic nature, were made to depict the *acarus* mite causing scabies in man between 1657 and 1687 when more realistic observations were published by Bonomo and Cestoni, associates of Francesco Redi (G. C. Bonomo, *Osservazioni intorno a'pellicelli del corpo umano,* Florence, 1687, English abstract in *Phil. Trans.,* 1704, **23**: 1296–1299). For details of this development, see Reuben Friedman, *The story of scabies,* vol. I, New York, Froben Press, 1947, pp. 191–292.

[15] As late as 1898, Emile Duclaux quoted at length Claude Bernard's observations concerning the lessons to be learnt from the exploration of the role of *acarus* as the agent of scabies for the far more difficult pursuit of agents of disease seated inside the body. (E. Duclaux, *Traité de microbiologie,* vol. I, Paris, 1898, pp. 33–35.)

[16] This was the theme of Cogrossi's letter to Vallisnieri, cf. note 37 below, but the suggestion had been made as early as 1683 in a postscript by F. Slare to 'An abstract of a letter from Dr Wincler chief physitian of the Prince Palatine, Dat. Dec. 22, 1683 to Dr Fred Slare, Fellow of the Royal Society, containing an account of a murren in Switzerland, and the method of its cure', *Phil. Trans.,* 1683. **13**: 93–95, see p. 95.

[17] Included with the viruses of measles and dog distemper in the morbillivirus subgroup of the paramyxoviridae, rinderpest virus is antigenically related to the other two, and there is in fact a certain amount of cross-protection among the three viruses.

[18] All the contemporary treatises discussed here emphasize that the Italian outbreak was initiated by a single infected ox straying near Padua from a contingent in transit from Dalmatia, see Ramazzini (note 22), p. 13; Cogrossi (note 37), p. 22; and Lancisi (note 31), p. 1.

[19] Gamgee, op. cit., note 8 above, emphasized that the importation of rinderpest from Russia (he also called it "Russian disease" and "Steppe disease") through Hungary to northern Italy had taken place intermittently throughout the seventeenth century; he also pointed out that the disease had been frequently confused with other afflictions of cattle, including foot-and-mouth disease, bovine pleuropneumonia, and anthrax (cf. note 134 below).

The principal professor of medicine of that august institution[20] at the time was Bernardino Ramazzini (1633–1714). On 9 November 1711, he devoted his annual address to faculty and students to an examination of the outbreak, discussing its causes, the possibility of controlling it by the usual means of cleanliness, isolation of infected animals, and fumigation of stables, and adding a number of prescriptions for potential remedies.[21] In a short, but clearly written, paragraph, Ramazzini gave his views on the nature of the contagion. Having expressed his contempt for astrological explanations[22] and pointed out that there could be no general source of infection in the air or pastures since the disease was obviously specific to cattle and did not affect other animals, he wrote:

> It is an inherent characteristic of infections that the seeds of disease easily multiply and widely propagate themselves if, as they say, they find a lodging in a suitable and susceptible subject. The best example we have of this characteristic of infections is the French disease which, having been brought to us from the West Indies by a few sailors, has most rapidly infested, first Italy, later all Europe with a most abominable and inescapable disease. When we consider to what extent this present contagion has been spread through the secretions and the excreta of ailing and dead oxen, how stables and pastures have been contaminated to the injury of other oxen subsequently using them and how hides of dead oxen are polluted (for the infection can long persist in hair), I say that we most certainly must not be astonished that this infection has spread far and wide. When this most virulently poisoned breath in its travels enters the healthy body of an ox . . . .[23]

Ramazzini did not use the term "seeds" of disease as freely as had Fracastoro in the sixteenth century,[24] but otherwise his ideas of contagion were not dissimilar. They both had a firm idea of the multiplication and propagation of infectious disease entities; but neither suggested that the entities might be animate.

Ramazzini also set the pattern for the comparisons between smallpox and rinderpest[25] which were to be made with increasing frequency throughout the century, noting that affected animals developed pustules by the fifth and sixth day, that the majority died between the fifth and the seventh day, and that very few survived.[26] And, as Lancisi was to do in his book four years later, he directed a broadside at those medical men who found it beneath their dignity to discuss diseases of cattle.[27] McDonald noted with some astonishment that Ramazzini chose to conclude his lucid oration with an uncharacteristic and curiously incongruous apologia and plea for heavenly guidance rather than scientific common sense to deliver the region from the pestilence.[28] On the other hand, this was also a reminder of previous attitudes and

[20] Prominent among Italian medical schools since the thirteenth century, the faculty at Padua enjoyed enormous prestige both at home and abroad; for the lure of Padua for Ramazzini even at the age of nearly seventy, see Wilmer Cave Wright's introduction to her translation of Ramazzini's *Diseases of workers*, New York, Hafner, 1964, pp. xxiv–xxv.

[21] Wilmer Cave Wright has pointed out that it was contrary to Ramazzini's custom to suggest remedies and that this was most likely a concession to the Venetian authorities, ibid., p. xxxvi.

[22] Bernadino Ramazzini, *De contagiosa epidemia, quae in Patavino agro, & tota fere Veneta ditione in boves irrepsit*, Padua, Conzatti, 1712, p. 15.

[23] Ibid., p. 17. In his translation, McDonald (note 28 below) used "contagion" and "infection" indiscriminately and independently of the text; he also translated *morbosi seminii* simply as "contagion" and made no mention of the use of the "seeds of disease" metaphor.

[24] Cf. Nutton, op. cit., note 11 above, pp. 21–30.

[25] Cf. Stevenson on analogies, note 10 above.

[26] Ramazzini, op. cit., note 22 above, p. 13.

[27] Ibid., pp. 12–13.

[28] John M. McDonald, 'Ramazzini's dissertation on rinderpest'. *Bull. Hist. Med.*, 1942, **12**: 529–539, see p. 538.

perhaps not as unexpected as McDonald and other commentators would have it; it was quite possibly included as a precautionary measure and a token of respect to the established church, and as such was reflected in much later literature. In any case, his plea remained unanswered, and the plague continued to spread until, two years later, it had reached Naples and eventually even Rome, in spite of preventive measures taken by the Papal authorities, under the Pope's personal physician, Giovanni Maria Lancisi.[29]

It was not Lancisi's first encounter with the cattle plague. At the time of the initial outbreak at Padua, he had written an epistolary essay on the disease in the vernacular of the day and of the region.[30] But in August 1713, in spite of all caution, infected oxen reached not just Roman territory, but a market within the city itself.[31] Pope Clement XI and his Council and ministers acted with commendable speed, relying on the expert advice of Lancisi.[32] Measures of quarantine and isolation were enforced; Lancisi advocated the killing of all infected or suspected cattle, but ran into difficulties with those he described as of "milder opinion" among the officers of the court.[33] Eventually, the efforts of the Papal state to stem the tide of disaster succeeded, and Lancisi could publish his version of the sequence of events, as well as his reflections on the nature of the contagion responsible for the outbreak.[34]

Lancisi wrote at much greater length than Ramazzini, and also in more depth and with much wider terms of reference. He included not only classical authors such as Columella and Varro, whose remarks on disease-causing animalcules issuing from swamps he may have been the first, but certainly not the last, to quote;[35] among those whom Lancisi called "modern writers" were, in addition to Athanasius Kircher, some whose works were much closer to home, in every sense implied by that term. These authors were Cogrossi and Vallisnieri,[36] whose correspondence sparked off by the same epizootic of cattle plague was published in 1713.[37] When rinderpest came to Italy, Cogrossi was working as a physician in his home town of Crema in the Veneto and so had ample opportunity to observe the outbreak. In the works of Cogrossi, Vallisnieri, and Lancisi in the second decade of the eighteenth century we find for the first time explicit discussions of the problem which was to engage so many able minds

[29] G. M. Lancisi (1654–1720) is, like Ramazzini, better known for a number of other works than for his treatise on rinderpest. He had been physician to successive popes since appointed by Innocent IX in 1688.

[30] Published at Venice in 1712, translated into Latin from the original "Etruscan", it was also included as part of *De bovilla peste*, note 31 below, pp. 179–205.

[31] G. M. Lancisi, *Dissertationis historicae de bovilla peste*, Rome, J. M. Salvioni, 1715, pp. 2–3.

[32] Ibid., p. 3.

[33] Ibid., p. 6.

[34] Ibid., pp. 175–178.

[35] Ibid., pp. 173–174; Lancisi's own comments on swamp fevers were published two years later in *De noxiis paludum effluviis eorumque remediis*, Rome, Salvioni, 1717. Cf. also Saul Jarcho, 'A papal physician and the sanitation of New York City', *Bull. Hist. Med.*, 1978, **52**: 410–418, see pp. 411–414.

[36] Carlo Francesco Cogrossi (1682–1769) had attended the lectures of Antonio Vallisnieri (1662–1730) at the University of Padua; Ramazzini was also among his teachers.

[37] C. F. Cogrossi, *Nuova idea del male contagioso de'buoi*, Milan, 1714. This volume contained Cogrossi's original letter of *Pensieri filosofici* as well as Vallisnieri's reply and comments by Tomaso Piantanida and by Morando Morandi, see facsimile edition of the *Pensieri filosofici*, published with a translation by Dorothy M. Schullian and a foreword by Luigi Belloni by the Lombardy Microbiology Society for the sixth International Congress of Microbiology, Rome, 1953. This translation has been used in part, but not exclusively, for the present quotations.

until Koch and Pasteur delivered irrevocable proof in the following century: were infectious diseases caused by invisible living organisms, or by inanimate entities which could be vaguely described as being of a "poisonous" nature?

The transition from the concepts of Fracastoro, from "germs" or "seeds" of disease considered able to multiply but still regarded as inanimate substances, to the idea of living germs, able to multiply and spread *because* they were living organisms, was given full expression in Cogrossi's *Pensieri filosofici*, and in Vallisnieri's reply. More than 150 years were to elapse before these ideas were generally accepted; and over that period another, parallel, battle was being fought over the vexed question of spontaneous generation. But by 1713, all the necessary ideas were there; Cogrossi and Vallisnieri had linked transmission of contagious disease to Leeuwenhoek's *animalcules*. They had been helped by developments in neighbouring fields in Italy during the latter half of the seventeenth century. As Belloni has pointed out, there is indeed a straight line of development to be discerned from Harvey's *Omne animal ex ovo*, through Francesco Redi's elegant refutation of the theory of spontaneous generation and his definitive formulation of the concept of parasitism in 1687,[38] with the simultaneous microscopical observations of Leeuwenhoek, to the final, all-important link in the chain, the real turning-point to which Cogrossi refers at length in his first letter to Vallisnieri: the evidence, presented also in 1687, for the causal role played by *acarus* in scabies, by two Italians belonging to Redi's circle.[39]

In the *Pensieri*, Cogrossi gave a detailed description of the progress of the epizootic, noting that the measures taken due to "the splendid vigilance of our most diligent magistracy"[40] had managed for a while to contain the outbreak in isolated foci of infection, but that at the time of writing[41] it was out of control and spreading mercilessly in all directions. He helpfully appended a useful maxim which suggested that increasingly cynical attitudes were beginning to overtake the pious confidence in divine guidance of earlier centuries: "Do not fail to make use of divine measures as if human ones were lacking, and of human ones as if divine ones were lacking".[42]

But unlike Ramazzini, and later Lancisi, Cogrossi was not concerned with actual practical measures, be they human or divine. He was trying to provide a theoretical background by puzzling out the nature of the infection, his points of departure being the microscopical observations of Kircher[43] and of Leeuwenhoek,[44] and the recent

[38] Ibid., pp. ix–x.

[39] *Osservazioni intorno a'pellicelli del corpo umano fatte dal Dottor Gio: Cosimo Bonomo, e da lui con altre osservazioni scritte in una lettera all'Illustriss. Sig. Francesco Redi, Firenze 1687.* The pharmacist Giacinto Cestoni (1637–1718) had collaborated with Bonomo (d. 1696) in making the observations. See also, Richard Mead, 'An abstract of part of a letter from Dr Bonomo to Signor Redi, containing some observations concerning the worms of humane bodies', *Phil. Trans.*, 1702/1703, **23**: 1296–1299.

[40] Cogrossi, op. cit., note 37 above, p. 2; presumably, the magistrates had been acting on the recommendations made by Ramazzini, op. cit., note 22 above, pp. 38–41.

[41] Cogrossi's letter is dated 3 September 1713, nearly two years after Ramazzini's address on cattle plague given in November 1711.

[42] Cogrossi, op. cit., note 37 above, p. 2; Cogrossi does not pretend to originality here but refers to a popular collection of maxims, *L'homme de cour.*

[43] Ibid., p. 16: "Padre Kircher claims, if I recall correctly, to have observed very tiny worms in the blood", cf. note 2 above.

[44] Cogrossi, op. cit., note 37 above, p. 14: "The micrographs of Lochio, the famous Dutchman. . . ."

Italian observations concerning the role of the *acarus* in scabies.[45] There are copious references to all of these throughout the letter, as well as protestations of admiration for and inspiration drawn from Vallisnieri's own work on worms and the cycle of development from flies to eggs, to maggots, and back to flies. His main source of inspiration was quite clearly the analogy it had occurred to him to draw between the *acarus* as the agent in scabies, and even smaller organisms such as Leeuwenhoek's microscopes might one day render visible as agents in the present plague of oxen. Cogrossi wrote:

> If therefore such tiny living creatures are so readily met with everywhere, and if they can penetrate into the most hidden recesses of animals, may it not be permissible to suspect that in the epidemic among oxen the poisonous insects [*i venefici insette*] can pass from one animal to another of similar kind and through the fauces, the nose, and even the passages in the skin creep into the blood and introduce there irreparable and fatal disorders?[46]

Cogrossi also told Vallisnieri that he had no illusions as to the response his theories might elicit from the philosophers of the day, remarking that:

> In so prodigious an effect of nature as an epidemic among oxen, because our senses are blind, philosophy allows us to proceed tentatively .... But alas for me if I spoke with some of our long-robed peripatetics. The reply would be laughter .... They would find it inconceivable that there could be in nature animalcules so tenuous.[47]

Perhaps this outburst may also have been designed as subtle flattery for Vallisnieri's more perceptive biological outlook before his correspondent hopefully submitted his own revolutionary thoughts on contagion by confronting "the accepted hypothesis of poison or fermenting substance with the theory of the insects in order to see which of these two presents the clearer idea of so prodigious a pest".[48] In any case, having prepared the ground for whatever reason, Cogrossi launched his hypothesis of the aetiology of cattle plague with a flourish of metaphor and a reference to the potential for spreading infectious disease offered by rapid multiplication of "insects", i.e. micro-organisms,[49] which brings to mind the more sophisticated calculations employed by Löffler and Frosch at the end of the nineteenth century to support their theory of even smaller living organisms as the agents of foot-and-mouth disease.[50] Cogrossi wrote:

> The one Hungarian ox which manifestly arrived in Sermeola was the true origin of this disease. In the manner of the Trojan horse this animal brought in its viscera the malignant ferments of such a destructive intestinal war. This animal infected with its effluvia the countless herds of densely populated Lom-

[45] Ibid., pp. 4–12: Cogrossi refers to evidence available from Redi and from Cestoni, Bonomo having died in 1696, cf. note 39 above.

[46] Cogrossi, op. cit., note 37 above, p. 17.

[47] Ibid., p. 13: Cogrossi also adds, with some relish, that "... they would make game of me with that heroic sentence of mighty Aristotle *(Imbecillitas est intellectus relinquere sensum, et quaerere rationem)*".

[48] Ibid., p. 21; he also appends the pious thought that 'In all nature there is perhaps no theme more fruitful of marvels than that of contagious disease".

[49] In this text Cogrossi makes no distinction between the use of *"insetti"* and of *"vermiculi"* to denote the invisible organisms he was postulating as instigators of the contagion, cf. notes 65 and 66 below.

[50] Löffler and Frosch were using the need for multiplication of the agent in order to infect successive series of calves in inoculation and re-inoculation experiments to disprove the idea that a toxin might be responsible. F. Löffler and P. Frosch, 'Berichte der Kommission zur Erforschung der Maul- und Klauenseuche bei dem Institut für Infektionskrankheiten in Berlin', *Zentbl. Bakt. ParasitKde*, Abt. 1, 1898, **23**: 371–391.

> bardy. If then those very effluvia were nothing but a mass of very agile and very tenuous poisonous atoms, it should be realised how very numerous they must necessarily have been in order to afflict and infect so many animals . . . But . . . suppose we concede that the effluvia of the first infected ox also themselves did nothing more than release in the blood of others principles through the excitation of which were created new contagious effluvia identical to the original ones. The thought is reasonable, and may be upheld by many chemical experiments, and I also can support it. Nevertheless, if on the other hand I consider the manifest and visible propagation of the insects I cannot but admit that this offers quite a clear and ready pattern for understanding the spread of the contagious disease among oxen. Just two of these insects carried into Italy by the Hungarian ox could in successive generations have produced an innumerable army of others to damage so many herds and so many flocks.[51]

In the following pages of his letter Cogrossi brought into play other diseases of man, animals, and plants, ranging from syphilis to corn rust, to support his thesis. For all his self-deprecation – and there is more than one outburst of "Who would believe it?" and references to "my . . . poorly conceived and more poorly expressed thoughts" – Cogrossi obviously had faith in the message he finally summarized, presenting his case and at the same time disposing of the theories of the past, in the following passage:

> Let us now apply our reasoning . . . to the tiny animals which I postulate are infecting the oxen, and it becomes apparent that it is perfectly possible to consider the mere introduction of these insects carried by the ox, and their multiplication, favoured by the special conditions at the time of the air and of the bovine humours, the reason for a calamity so great and so general, *without* incriminating poor innocent Saturn, *without* blaming unseasonal weather or corruption of water or pasture, and *without* having to assume that such a malignant ferment could result from an accidental amassing of fluids in the first ox.[52]

From Vallisnieri's reply it is evident that he found a responsive attitude in his old teacher;[53] but others were more cautious in the absence of experimental evidence that could not and would not be produced until the nineteenth century. How cautious, may be seen from the writings of Lancisi. Lancisi, in a commanding position within the medical establishment of Italy in the early eighteenth century, had written his epistolary essay on cattle plague in 1711.[54] In 1713, as we have seen from Cogrossi's *Pensieri,* the disease was spreading alarmingly, with the consequences for Rome referred to above and reflected in Lancisi's *De bovilla peste* of 1715. This is a comprehensive account of the outbreak and especially its Roman ramifications, its history and background, and the secular and ecclesiastical measures taken by the Papal state.[55] It also includes a chapter containing Lancisi's own closely reasoned opinion of the nature of the disease.[56] Although Lancisi and the full machinery of the powerful Papal state had ultimately failed to prevent the epizootic reaching their territory, swift and radical action[57] did minimize the effects and avert total disaster. By the time

[51] Cogrossi, op. cit., note 37 above, pp. 22–23.

[52] Ibid., p. 30.

[53] Belloni, op. cit., note 37 above, pp. xx–xxiii.

[54] G. M. Lancisi, *Epistolaris dissertatio ad Doctissimum virum Antonium Mariam Borromaeum . . . De bovilla peste, Veneta Armenta depopulante, ab Hetrusco in Latinum sermonem conversa;* see *De bovilla peste,* note 31 above, pp. 179–205, cf. note 30 above.

[55] The enlightened attitude of Clement XI in choosing Lancisi to handle the outbreak instead of relying exclusively on prayers and appeals for divine mercy has been admired by a number of later commentators, see e.g. J. F. Smithcors, 'The history of some current problems in animal disease: III. Rinderpest', *Vet. Med.,* 1956, **51**: 249–256.

[56] Lancisi, op. cit., note 31 above, chap. VIII.

[57] Movement of cattle was banned, frank cases were ordered to be destroyed, and suspected ones isolated. Lancisi ominously adds: "Meanwhile, the merchants who had by evil deceit broken the ban on business

Lancisi wrote his final report, in 1715, he was able to review the outbreak from a position of strength, and to reflect with calm objectivity on its causes.

While Cogrossi, the layman physician, made only fleeting reference to the possibility of employing "divine measures", Lancisi, the Papal physician, for obvious reasons, devoted several pages to "What the Holy Pontiff ordained for the beseeching of Divine Help while the cattle epidemic was raging";[58] although it may be noted that the practical measures taken were described first, and at considerably greater length.[59] Lancisi also devoted the middle part of his three-part account to a comprehensive list, with Latin titles and commentary in the Italian vernacular,[60] of all the edicts issued by the Papal authorities in their efforts to stem the rising tide of the epizootic. The seventy pages of these practical prescriptions contrast sharply with the mere eighteen pages sufficing for the edicts to ecclesiastics, and instructions to the general public, concerning suitable prayers. The last prayers mentioned were to be recited "at the time of the Sacred Advent, at the sound of a trumpet... to the same end of extinguishing the plague".[61] It may also be noted that while Lancisi's text is in Latin, the edicts, with the exception of two issued in the name of the pontiff himself, are all in the Italian vernacular, presumably to facilitate understanding by the public at whom they were directed.[62]

It was in the third and last part of the book that Lancisi came to consider the theoretical background and discussed his own and others' opinions concerning the aetiology of the disease.[63] Cogrossi had paid his respects to Lancisi and his early essay[64] in his first letter to Vallisnieri; writing two years later, Lancisi was able to consider in some detail the suggestions made in that correspondence. In fact, Lancisi used the ideas expressed by Cogrossi and seconded by Vallisnieri as his point of departure for his chapters on the possible causes of the disease:

> ... some (Carolus Franciscus Cogrossi and Antonius Valisnerius, the one of Crema, the other the celebrated professor of medicine at Padua) have recorded certain swarms of particular little grubs from which among those infected arises a sickness like scabies; so they think by plausible conjecture that the cattle plague is passed on from the sick to healthy herds by contagion at close quarters or from a distance by infection. And indeed I would add my own vote to this opinion as wholly indisputable, as it should be if I had specifically looked for or at least chanced to come upon worms in their blood on the evidence of my own eyes. But since it has been granted me to see them only in the skin, nostrils, mouth and fauces, which have a surface next to the air about them; for that reason we rightly hesitate as dealing with a matter very probable but not yet certain, not doubting however that a hypothesis of that kind should be listed with those which, though not perceptible by the bare senses, yet may be true and by diligence may

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were thrown into prison to be suitably punished in due course (deceitful merchants put in chains)", ibid., p. 4.

[58] Ibid., chap. V, pp. 10–13.

[59] Ibid., chaps. III–IV, pp. 3–9.

[60] Throughout the eighteenth century, the use of the vernacular was increasingly replacing the exclusive use of Latin in medical and scientific texts everywhere, perhaps slightly later in Italy than elsewhere; in the case of the present edicts a contributory cause was undoubtedly the need for them to be understood by the general public.

[61] Ibid., p. 12.

[62] Cf. note 60 above.

[63] Ibid., part iii, chap. VII, pp. 172–174, where he refers to Kircher, Langius, Cogrossi, and Vallisnieri, and quotes Varro and Columella on "obscure and pestilential diseases which arise in the neighbourhood of marshes", p. 174.

[64] Cogrossi, op. cit., note 37 above, pp. 21–22.

> be brought into a clearer light. The claims by some people that worms have been seen, with the naked eye or the aid of a microscope, in the shed blood of affected oxen, this we think needs further attention; for after the blood settles and turns into a lump, it can be so infected by air-borne insects that it is very quickly seen to abound with a close-packed but wholly alien company. So we are torn in mind because we can reach no certain decision about what otherwise, as being highly probable, we have seen put forward, approved and carefully committed to writing not only by modern writers, especially Kirckerius, Langius, Cogrossius and Valisnerius, but also by important ancient writers. For Marcus Varro and Columella, when dealing with obscure and pestilential diseases which arise in the neighbourhood of marshes make mention of such little grubs. . . .[65]

Like Lancisi's other, better known, treatises, his dissertation on rinderpest is presented in clear and persuasive language. There is just one point on which today's reader might wish to seek clarification. In the above passages, was he or was he not making a conscious distinction between "little worms or grubs" *(vermiculi)* and worms *(vermes),* and was he deliberately ignoring the finer points of Cogrossi's argument? For it would seem reasonably clear from Cogrossi's discussions quoted above that he used the idea of visible worms only as a paradigm for a world of much smaller invisible beings posited as agents of the cattle plague.

On the other hand, it must be admitted that Lancisi presented the more realistic and comprehensive picture of the facts as known and of the conclusions it appeared to be permissible to draw in the contemporary context. Although he did not mention Redi by name, he must have had in mind the latter's recent experiments disproving spontaneous generation when he warned of the rapid invasion of blood samples by "airborne insects":[66] and his reasonable distrust of Kircher's conclusions[67] obviously added to his frustration at the impossibility of obtaining reliable experimental evidence in an age which had neither sufficiently powerful microscopes nor the general experimental techniques needed to obtain unequivocal results. It is perhaps also significant that Lancisi was the first writer in this era to draw attention to the classic works of Varro and of Columella on swamp fevers;[68] two years later, he published his own thoughts on the subject in *De noxiis paludum effluviis eorumque remediis.*[69] In three famous passages, first translated by Gorgas and Garrison[70] and often quoted since as suggesting Lancisi's awareness of the involvement of mosquitoes in the transmission of swamp fevers, Lancisi mentioned both the harmful effects of mosquito bites and the breeding of these insects in the swamps around which malaria flourished. He also emphasized the lack of experimental evidence, and his conclusion hardly amounted to a firm implication of mosquitoes as carriers of malaria when he wrote:

> . . . it would be necessary that the blood of those suffering from marsh fevers should be let, which medical reason seldom admits; and to carefully examine the blood with a microscope for insects of this kind, if such there be. But, although worms might be seen in the drawn blood, it would still be doubtful that these insects should be considered as the cause of the evil; or whether, which I consider more probable, it is

[65] Lancisi, op. cit., note 31 above, pp. 172–173; in this passage, "grubs" has been used when Lancisi used "*vermiculi*" in the text and "worms" when he used "*vermes*".

[66] Ibid., p. 173; here he used the term "*insecti*".

[67] Cf. note 2 above.

[68] Cf. note 63 above.

[69] Cf. note 35 above.

[70] W. C. Gorgas and F. H. Garrison, 'Ronald Ross and the prevention of malarial fever', *Sci. Mthly.* 1916, 3: 133–150, see p. 135.

> the product of the breaking down of the fluids; whence all the minute ovules, after they have been wrapped up in particles of the blood, are set free or are supplied from the external air. I can therefore form no opinion from autopsies whether these diseases are carried by insects into the blood.[71]

As in the case of writings on cattle plague of this period, such passages must be read with circumspection and subjected to semantic considerations. Certainly the need for a microscope to look for "insects of this kind" suggests a somewhat indiscriminate use of the terms "insects" and "worms", as elsewhere in the contemporary literature, and makes it highly improbable that Lancisi's ideas concerning "diseases carried by insects" had much in common with modern concepts of disease-carrying arthropods.[72] Even if we accept that the possibility that fevers might be carried by mosquitoes had occurred to Lancisi,[73] it is clear that his caution and objectivity combined with the restrictions imposed by contemporary medical knowledge and his recognition of the total lack of experimental evidence to make him resist unwarranted further and firmer conclusions. The same circumspect common-sensical reasoning is uppermost in his discussions of the cattle plague.

In the chapter in which Lancisi presented his final opinion concerning the "manner and means by which the cattle plague insinuates itself into the bodies of the beasts by contagion or infection", he carefully avoided any suggestion that the agent of the disease might be a living entity as suggested by Cogrossi, and to do so turned to the terminology used earlier by Fracastoro. Recognizing that the illness never occurs spontaneously, but is always transmitted from beast to beast or, frequently, from beast via herdsmen, other attendants, or dogs to another beast,[74] Lancisi ascribed the spread of the disease to the transmission of the seeds *(semina)* of cattle plague.[75] Describing these "seeds" as a poison or, for "greater clarity" a "pestiferous ferment",[76] Lancisi presented his ideas on the aetiology of cattle plague in particular and of contagious and infectious diseases in general. In the eighteenth-century context, this proved to be so much more acceptable to the medical community at large than the ideas of Cogrossi that Lancisi's concept of causes was reflected, when not copied outright, in almost all of the subsequent literature until the rise of a proper science of microbiology based on sound experimental evidence in the nineteenth century.[77] Lancisi wrote:

> ... the cause of the cattle plague is nothing other than an entity, or the particles of an entity, which endowed with extreme tenuity, speed and force of action move over a very short gap in such a way that by their proximity they are transported and cross from one body to another either by contact or at least by fomites ... it may be inferred that these tenuous corpuscles [are of the] character of a particular poison whose property is that, while it weakens and destroys the animal's processes, it also has the innate quality of immediately creating and releasing in great quantity corpuscles like to itself in the healthy body which it invades, and multiplying them by its contact. Wherefore not only those molecules which at

[71] Lancisi, op. cit., note 35 above, chap. XIX, III, p. 72.

[72] He may have meant only to suggest that the blood was actually invaded by such "insects", i.e. micro-organisms, again using the mosquito as a paradigm of much smaller invisible beings, i.e. "insects". In this same paragraph, he again seems to use "worms" and "insects" indiscriminately.

[73] Gorgas and Garrison see the above passage as "suggesting [the mosquitoes] a possible agency in inoculating disease", op. cit., note 70 above, p. 135.

[74] Lancisi, op. cit., note 31 above, p. 175.

[75] Ibid., "... *ipsius pestis semina, hoc est fomitem* ..."; cf. Nutton, op. cit., note 11 above.

[76] Lancisi, op. cit., note 31 above, p. 177.

[77] Although the work of M. A. Plenciz discussed below formed a notable exception.

> the beginning of the contagion have passed from sick beasts to altogether healthy ones cause so much and so great destruction, but also those which have most recently joined them and when set in motion acquire the same poisonous force. Now we observe the same increase in fermentation, evident and before our very eyes, in breadmaking, where a small lump of yeast corrupts the whole mass and converts it to its own nature. In a cask of sweet wine too, which with the addition of a little vinegar immediately turns sour. Again, in bodies wounded with a viper's bite, which in a moment of time swell all over with the same corruption and are weakened. And so it seems to me that nothing remains to prevent a clear understanding that the cause of the cattle pestilence must be attributed to that sort of body which for greater clarity we call a pestiferous ferment. . . .[78]

It will be observed that nowhere in this final summing-up of his opinion does Lancisi suggest that these tenuous corpuscles, although able to multiply, might be living organisms. By a curious twist of history, his description of the causative agent as "tenuous corpuscles with the innate quality of immediately creating and releasing in great quantity corpuscles like to itself in the body which it invades, and multiplying them by its contact" would make a not entirely inept definition in the light of present-day knowledge of the nature of virus infections.[79] His very caution, which did not allow him wholeheartedly to embrace Cogrossi's flight of the imagination, made his explanation closer to the truth in the case of virus diseases, while it made him reject what might be called the bacteriologically sounder explanation. And, in view of the facts known to him, his conclusion cannot be faulted.[80]

The first wave of the eighteenth-century European epizootic of rinderpest reached England in 1714. Where Clement XI had turned to his personal physician, George I used his court surgeon, Thomas Bates.[81] Bates handled the outbreak with the same kind of efficient common sense displayed by Lancisi, and the London epizootic was quickly contained; but he was no Lancisi, and made no attempt to come to terms with the underlying problems of aetiology. He wrote, sensibly if diffidently:

> The Providence of God has so disposed of the matter of Animal Bodies, as to render Contagious Diseases very seldom infectious to different Species; but Experience demonstrates, that Contagions may be communicated to the same Species, by touching the Woolen, Linnen, etc. to which the Infectious *Effluvia* of the Diseased had adhered, tho' the two Bodies should be at a very great distance; and I verily believe that more Hundreds died from the Infection, which was carried by the Intercourse that the Cow keepers had with each other, than single ones by the original Putrifaction.
>
> The Nature of Contagious Diseases are but little understood, and it would neither be agreeable to my Design, nor useful to the Publick, to say more of this than what was evident. . . .[82]

[78] Lancisi, op. cit., note 31 above, chap. VIII, pp. 176–177.

[79] The question of whether viruses should be classified as live organisms or dead molecules belongs to the twentieth century. It attracted much attention in the 1920s and 1930s, see e.g. H. H. Dale, 'The biological nature of viruses', *Nature, Lond.*, 1931, **128**: 599–602; and A. E. Boycott, 'The transition from live to dead: the nature of filterable viruses', *Proc. Roy. Soc. Med.*, 1928, **22**, (i): 55–69; until it became apparent, when viruses were identified as nucleoproteins and their manner of replication was recognized, that the question was pointless.

[80] Winslow has discussed the reasons why the concept of contagion was accepted by the unquestioning layman long before such theories could be accepted wholeheartedly by the better informed medical profession who could not reconcile the apparent contradictions and inadequacies before a fully developed germ theory supplied the necessary explanations. Winslow, op. cit., note 2 above, see p. 182.

[81] Little is known of the life of Thomas Bates. He had been surgeon to Queen Anne and served with a regiment before becoming surgeon to George I on his accession in 1714, the year of the London outbreak of cattle plague. He was elected FRS in 1718, and died in 1760.

[82] Thomas Bates, 'A brief account of the contagious disease which raged among the milch cowes near London, in the year 1714. And of the methods that were taken for suppressing it', *Phil. Trans.*, 1718, **30**: 872–885, p. 884.

After 1714, both England and Italy were spared further ravages of the cattle plague for about thirty years, although at no time was the European continent entirely free from the disease. In the Netherlands, where the tendency was to rely on spurious "cures", the disease became endemic at an early stage, and the low countries remained a convenient reservoir for re-introduction of rinderpest to the rest of Europe. The threat of re-introduction was ever present, as is reflected in much of the literature on other infectious diseases of the period.[83] In 1745, rinderpest reappeared in England. Although Bates was still around to give advice,[84] this outbreak proved considerably harder to control than the previous one. It could have been a more virulent strain of the virus;[85] it is also possible that climatic factors and political difficulties[86] added to the seriousness of the situation and to the complexities faced by the authorities. Although the 1714 outbreak in and around London was under control in a matter of three months, the 1745 epizootic was to drag on for more than ten years, until it finally receded in 1757. And, because the disease this time was present in the country for a protracted period, the harvest in terms of literature on the subject was proportionately richer.

A number of serious discussions concerning the nature and origin of the outbreak were found in the *Philosophical Transactions* and elsewhere during the crucial years. Although the majority ignored the ideas of Cogrossi regarding living animalcules as agents of the disease, they frequently perpetuated both the sound public health measures suggested by Ramazzini, Lancisi, and Bates on the previous occasion, and also Lancisi's ideas on the nature of the infection. One such characteristic rendering in English of views which most probably had their origin in Lancisi's texts was published in the first year of the epizootic, in a volume otherwise concerned mainly with plague in man.[87] Its author, Theophilus Lobb, appears to have been an engaging mixture of a non-conformist minister and a concerned physician enthusiastically pursuing the study of a number of diverse subjects. His 1745 volume comprises a collection of letters written to friends and colleagues on aspects of contagious diseases, especially the plague.[88] The sincerity of his faith lent a special flavour to Lobb's letters, and Biblical quotes and Christian moralizing form as much a part of the writings as do the

[83] Before the 1745 outbreak, the *Gentleman's Magazine* and the *London Magazine* had warned of the impending threat, see e.g. *Gent. Mag.*, 1744, **14**: 567, 585–588. Other contributions in these magazines during this outbreak have been chronicled by C. R. Mullett, 'The cattle distemper in mid-eighteenth-century England', *Agric. Hist.*, 1946, **20**: 144–165 *passim*.

[84] *Gent. Mag.*, 1745, **15**: 528: 'Directions recommended to be observed in the present incurable, and contagious distemper among the cows'. By Thomas Bates, Esq. of Alton in Hampshire. This is largely a re-statement of his previous account (op. cit., note 82 above), to which he refers.

[85] Not only do different strains vary in virulence, but different breeds of cattle vary in susceptibility.

[86] The British army was engaged in pursuit of the Young Pretender until his defeat at Culloden in April 1746 and his final departure for France. Another protracted problem during these years was the War of the Austrian Succession.

[87] Theophilus Lobb, *Letters relating to the plague, and other contagious distempers*. London, Buckland, 1745. Lobb's ancestry was rich in non-conformist divines, and he was educated for the ministry. During a long life (1678–1763), he acquired enough medical knowledge from friends and neighbours eventually to become a member of the College of Physicians and a Fellow of the Royal Society.

[88] Part I of the book consists of eight letters, all on the plague, and all addressed to the then President of the Royal Society, Martin Folkes. The letters in part II are all to an anonymous recipient, with the exception of the letter on cattle plague (Relating to *contagious* sicknesses among *Cattle*) which is addressed to John Milner, cf. note 100 below.

author's equally sincere attempts to come to terms with the nature and causes of contagion, and possible preventive measures. Apart from the natural preoccupation of the clergyman with sin and vanity as triggers for the wrath of God and hence as ultimate causes of pestilence of any kind, Lobb was as vague as most of his contemporaries as to the origin of the primary cases of prevailing epidemics and epizootics. Regarding the proliferation and further spread of the infection, he did, however, have firm views. If they were not wholly original but rather a reflection and distillation of prevailing ideas expressed by other authors, Lobb's version was succinct and presented with confidence. In his letter on sickness in cattle he wrote:

> ... the *dissolvent Particles* (however they came into the Body of the first sick Beast) like the *variolous* Humour in Persons who have the *Small-Pox;* and like the pestilential *Infection* in *People, assimilates*, or *transmutes* Part of the animal Fluids into their own Kind; which is a Generation, and *Multiplication* of infectious *Particles*, some of which, emitted from the Body of the first sick Beast is conveyed by Contact, or somehow, thro' the Air, into the Bodies of the Cattle infected by it, and so on.[89]

Innocent of all knowledge of the nature of cells and micro-organisms, let alone viruses, this passage from Lobb's letters, as well as Lancisi's earlier conclusions on which it may have been modelled, remains as a reminder that even in the field of infectious disease, eighteenth-century medical philosophy could sometimes quite unwittingly outstrip an empiricism which was struggling against heavy odds.[90]

The year 1745 also saw the introduction of rinderpest to Scandinavia, brought to Denmark with raw hides of cattle dead of the disease in Flanders.[91] The English outbreak may have proved hard to control. In Denmark the situation deteriorated rapidly and irreversibly. Thirty-five years later, when Layard (see below) could write of preventive measures against the disease from a comfortable distance, he mentioned "... Denmark, where the contagious distemper is become naturalized and general...."[92] Neither was this due to indifference and inaction. Shortly after the appearance of the first cases in 1745, the *Transactions of the Copenhagen Academy of Science*[93] published the results of an official inquiry by three professors at the University of Copenhagen into the nature of the disease and the possibilities for control.[94] Their observations followed the overall pattern of the contemporary English ones; the pathologist among the three, J. B. Buchwald,[95] believed that the cattle plague

[89] Lobb, op. cit., note 87 above, pp. 376–377.

[90] At the same time, this passage neatly demonstrates the prevailing confusion and all the reasons for Winslow's apologia for the physicians who "knowing the facts more intimately, realized that no *existing* theory of contagion taken by itself could possibly explain those facts", cf. note 80 above.

[91] The same source responsible for the simultaneous outbreak in England.

[92] D. P. Layard, 'A letter to Joseph Banks, Esq. President of the Royal Society, etc. from Daniel-Peter Layard, M.D. Fellow of the Royal Societies of London, Antiquaries and Gottingen, etc. relative to the distemper among the horned cattle', *Phil. Trans. R. Soc. Lond.*, 1780. **70:** 536–545.

[93] The Copenhagen Academy (Videnskabernes Selskab) had been founded in November 1742 (Royal Charter January 1743) and the first volume of its Transactions (*Skrifter*) was published in 1745.

[94] Of the three, only one was concerned with pathological anatomy, another was professor of forensic medicine, and the third had neither medical experience nor pretensions, but had some experience of farming and was a distinguished philosopher who was to go down in history for his literary merits. See Hans Rieck, 'Studien zu Betrachtungen der Kopenhagener Professoren J. B. von Buchwald, Georg Detharding und Ludwig von Holberg zur Rinderpest 1745 in Dänemark', Inaugural–Dissertation, Justus Liebig–Universität zu Giessen, 1979; abstract in *Hist. med. vet.*, 1980, **5** (3): 69–70.

[95] Buchwald (1697–1763) was an early exponent within the medical profession of interest in veterinary science, and even had a not inconsiderable knowledge of the functioning of the ruminants' digestive system

was caused by "insects". In spite of their efforts, theoretical and practical, the Danish outbreak continued on an alarming scale until the king and government, and the French-trained authorities at the recently established veterinary school in Copenhagen (which, in part, owed its existence to the threat of cattle plague),[96] sought assistance in London to establish a practice of inoculation.[97]

Their appeal went to Daniel Peter Layard, who became involved in the problems of rinderpest only in the later stages of the mid-eighteenth-century epizootic.[98] Like Ramazzini, he was much preoccupied with its possible analogy with smallpox, and, writing at a time when smallpox inoculation had become a well-established practice, he not unnaturally turned to considerations of the efficacy of inoculation[99] in the supposedly parallel case of cattle plague. His directions for inoculation of cattle at risk were closely modelled on contemporary sources dealing with smallpox inoculation. His verdict on the prophylactic value of the procedure was wholly favourable, although he pointed out that it should be used only with extreme caution and forethought. His thoughts on the uselessness of cures, and of the need for killing and deep burial of infected cattle as a means of control, differed little from those of Ramazzini and of Lancisi, to whom he referred at length, and of Thomas Bates whom he ignored.[100]

Unlike some of his more philosophically inclined colleagues, Layard was essentially a practical man, who saw little reason for speculating on the aetiology of the disease. He wrote:

> What particles constitute the pestilential *fomes*, how, and in what manner it acts, I shall not take upon me to determine *a priori;* but leave to others to philosophize on, and refer them to the authors already mentioned. Thus far I will venture to affirm, that either by inspiration, or deglutition, *effluvia* of a very

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(ibid., p. 70). On the whole he appears to have concerned himself less with the aetiology of the disease than with symptoms and pathological–anatomical details.

[96] H. C. Bendixen, 'The Royal Veterinary School in Copenhagen. Highlights from the time of Peter Chr. Abildgaard and Erik Nissen Viborg', ibid., 1976, **1**: 70–77.

[97] Layard wrote in 1780: "Count Bernsdorff and Dr Struensee had all the necessary instructions, books, and papers delivered to them by me, when the King of Denmark was in England" (Layard, op. cit., note 92 above, p. 540). This must have been before 1772 (probably 1768–69) when Struensee was beheaded for high treason, i.e. his intrigues, political and otherwise, with the unhappy young queen of the insane Christian VII.

[98] D. P. Layard (1721–1802), MD Rheims 1742; FRS. After a short period at Middlesex Hospital, he settled in Huntingdon and practised there for twelve years. "... this calamitous sickness, which, from my situation in Huntingdonshire in 1756, it fell to my lot to investigate". (Layard op. cit., note 92 above, p. 544).

[99] D. P. Layard, *An essay on the nature, causes and cure of the contagious distemper among the horned cattle in these kingdoms,* London, Rivington, 1757, see chap. VII, pp. 100–110. Layard referred to the experiences of a number of livestock owners at home and abroad who had inoculated their cattle. He stated that "In Holland inoculation has both failed and succeeded". This would indicate that the practice of inoculating for cattle plague was quite well established by 1754, more than two decades before the appearance of work on the subject by Geert Reinders, who inoculated for Pieter Camper (cf. A. van der Schaaf, 'Geert Reinders (1737–1815)', *Hist. med. vet.*, 1978, **3**: 89–98). The claim here that "Reinders was the first to recognise the acquired immunity of a cow after her recovery from rinderpest and the practical use of this immunity for the safe vaccination [*sic*] of her calves . . ." would seem exaggerated, in view of the fact that Reinders did not become involved in inoculation experiments until the late 1760s.

[100] Lobb dedicated his letter on the cattle plague to the justice of the peace (cf. note 88 above) administratively in charge of control of the 1714 outbreak without mentioning Bates – was this a conspiracy of silence directed at a mere surgeon?

subtle and active nature are drawn in, which first vitiate the fluids, then relax, and destroy the solids of the cattle.[101]

It may also be noted that with Layard in mid-century there was a progressive advance towards a more outspoken pragmatic approach and an increasing tendency to rely on one's own medical conscience and capabilities rather than admit the necessity of appeasing divine wrath as the underlying cause of any calamity. In his closing paragraph in 1757, Layard summed up the changing attitudes, with only a final protestation, almost as an afterthought on the last page, of his own intention to secure divine approbation of his practical recommendations (modestly referred to as "second causes"):

The Heathens made sacrifices, and oblations to their gods, that they might appease their wraths and avert the disease. The Christians have called upon their creator in such times of visitation, according to the rites and ceremonies of the church they belonged to. Our prelates have drawn up a form of prayer to be used all over these kingdoms at the time of such distress, to implore the Divine Assistance and Protection. A method on all accounts expedient, towards the success of our own endeavours; since we cannot reasonably expect a good event in the use and application of *second* causes, 'till we have secured the concurrent favour of HIM whom we justly acknowledge to be the *first*.[102]

By 1780, when it had become clear that the policy adopted in England had been effective in combating the cattle plague, Layard summarized his experience in a letter to the President of the Royal Society,[103] taking the opportunity to compare the control measures used in different European countries.[104] He also firmly pointed out that the development of an effective system of control by killing infected cattle originated in England and not, as suggested by certain French authors,[105] in the "Austrian Low Countries", i.e. Belgium. But he failed to remind his readers that very similar measures had been recommended originally by Ramazzini, Lancisi, and Thomas Bates during the early outbreaks in the second decade of the century.

Although the system of control by killing and quarantine was beginning to work in the British Isles towards the end of the 1750s, rinderpest continued to affect the European continent with undiminished intensity until almost the end of the century. In 1762, M. A. Plenciz wrote:

There is no sickness such as to cause greater loss among the herds, and the State to suffer more damage therefrom, than the infection which has lasted already some thirty years; for we see and deplore that such a dread plague wanders all around Europe seizing on the herds.[106]

Plenciz' *Opera* is an ambitious work, dealing in four tracts first with contagion in general, and subsequently with smallpox and scarlatina in particular, and finally with earthquakes and their consequences,[107] apparently inspired by a series of extensive

[101] Layard, op. cit., note 99 above, p. 22.

[102] Ibid., pp. 133–134.

[103] Layard, op. cit., note 92 above, p. 538. He reported with some satisfaction that re-introductions of the disease between 1757 and 1780 had been quickly quelled by the measures taken.

[104] Ibid., p. 541. He expressed sympathy for Pieter Camper whose attempts to introduce an inoculation policy in Holland had been thwarted by "the obstinacy and interruption of the peasants" and by inclement weather.

[105] E.g. Felix Vicq d'Azyr, *Exposé des moyens curatifs et préservatifs qui peuvent être employés contre les maladies pestilentielles des bêtes à cornes*, Paris, Merigot, 1776, see p. 577.

[106] M. A. Plenciz, *Opera medico physica*, Vienna, J. T. Trattner, 1762, pp. 142–143.

[107] The association of earthquakes with infectious diseases goes back at least as far as Seneca, cf. Nutton, op. cit., note 11 above, p. 11. It was still invoked at the end of the eighteenth century in Webster's *A brief history of epidemic and pestilential diseases*, 1799, cf. Winslow, op. cit., note 2 above, pp. 221–222.

earthquakes which had occurred simultaneously in Europe, Africa, and America in 1755.[108] The opening tract, on contagious diseases in general, offers perhaps a more closely reasoned discussion of the subject than any other contemporary treatise.[109] Although he did not, indeed could not within the limitations of the framework in which he was operating, present any experimental evidence, he went much farther than anybody else in using what circumstantial evidence there was, and what analogies he could find, to build up his case and to justify what still remained, undeniably, armchair reasoning. Like Cogrossi, he made good use of Leeuwenhoek's discoveries. Indeed, Plenciz discussed Leeuwenhoek's results in a far more detailed and trenchant manner than had so far been done by anybody, and in so doing arrived at a more convincingly formed conclusion. Above all, he based his reasoning on extensive calculations involving Leeuwenhoek's figure for the number of animalcules contained in a single drop of water,[110] and took the argument further by speculating on the eggs which might be produced by such *animalcules,* and (echoing Pliny) on the space containing whatever organs life on such a minute scale could be expected to support. He wrote:

> Animalcules . . . the smaller their size, the more they increase in number, for just as their size is barely perceptible, so their multiplication is unbelievable. What I have said may be found *passim* in the works of the illustrious Loewenh . . . and other famous authors; where a single small drop of water is said to contain at least 2,750,000 animalcules. But what, I ask, must we say about the eggs of these animalcules, and of the prospective fetuses developing from these little eggs? and taking it a step further, what finally about their organs? For the same celebrated author stated that myriads of millions of such animalcules do not equal a single grain of sand. These things are indeed inconceivable to our weak understanding, and yet if we admit the infinite divisibility of matter, we must judge them consistent with such infinite potential and therefore possible.[111]

Plenciz also seized this opportunity to connect his reasoning on contagion and infection to the never-ending arguments for and against spontaneous generation, and, by a further extension of this line of reasoning, to justify firmly his points of view by anchoring them in his Christian philosophy:

> But for the greater clarity in these matters we will linger awhile in considering the origin of all animals and plants: many natural philosophers are not ashamed to assert that certain animalcules and certain plants originate not from seeds but from decay, that is, from the internal mingling movement observed in putrefaction.
>
> However, besides the arguments which are commonly opposed to this opinion, I find one which is remarked by few, but which is incontestable: namely this. Everyone knows that both plants and animals, and their seeds, consist of discrete parts and organs set skilfully and in due order in their proper places; so that it appears not only inconceivable but also impossible that such organs could emerge and develop from a disorderly and fortuitous movement of particles. Otherwise, arguing from equal and indeed even stronger basic principles, I shall be able to say that larger bodies also are assembled from such fortuitous movement and meeting of particles; and I shall even be able to assert that the sun, the moon, the earth, the planets, the stars and other major bodies have been compounded by such chance movement and con-

[108] "De terraemotu, sed praecipue illo horribili agit, qui prima Novembris Anno 1755 Europam, Africam et Americam conquassabat", Plenciz, op. cit., note 106 above, tract 4, pp. 1–128.

[109] Cf. M. D. Grmek, 'Marko Anton Plenčič – A predecessor of medical microbiology', *'Int. Congr. Hist. Sci. VII* (Jerusalem, 1953), pp. 659–660.

[110] Plenciz explained the seed metaphor in more detail than earlier authors, asserting that "just as the multiplication of vegetables must take place through the successive evolution of seeds, so equally will the multitude of countless animalcules observed in liquids depend on the same process", op. cit., note 106 above, p. 42.

[111] Ibid., p. 36.

currence of particles; and this assertion savours not only of the damnable heresy of Epicurus and Democritus but of *actual atheism.*[112]

This led him straight to his basic premiss:

If therefore we posit as an infallible tenet, that all animals and vegetables take their origin from their own proper seeds, then it should equally be admitted that no seeds are produced *de novo*. . . . So, if no seed in Nature is produced *de novo*, it must necessarily be allowed that they were all created when the world began and now are evolved one from another; and so it follows that all seeds which were in existence from the beginning of the world right up to the present times, and which in the nature of things will exist in time to come, were already physically contained in those original seeds and that the rest can only be evolved from them. It equally follows that all mankind, those who have existed from the creation of the world and those who shall be in future, already *existed in rough outline in Eve's ovary.*[113]

Summing up, Plenciz returns to the principle of the infinite divisibility of matter, and to Leeuwenhoek's calculations of the number of animalcules in one drop of water, comparing them with figures quoted in contemporary sources for the number of seeds developed in one generation from single seeds of named species.[114]

Towards the end of his general tract on contagious diseases, Plenciz touched briefly on the "dread plague" of cattle, which by then had lasted "already some thirty years . . . around all Europe". By the time he finished writing, the cattle disease had taken on such alarming proportions[115] that he felt obliged to add a special section devoted exclusively to the *Lues bovinae*. He was no doubt spurred on by his earlier observation that:

. . . such things . . . are widely committed for direction to stupid and untaught veterinarians who know nothing of anatomy nor of animal economy, much less of illnesses, as though it were unbecoming for a doctor or surgeon to handle things of a kind which the great Hippocrates himself was not ashamed to deal with. . . . Whence, in order that we may the better confront similar diseases, I judge that it would be a very good thing if certain doctors and surgeons experienced in their profession, should be appointed to undertake a comparative study of animals that have died of the contagion, and duly assess it; so that remedies may be applied more appropriate to the disease.[116]

Although in the context of his times Plenciz had no access to direct experimentation, he assured his readers that he tried always to confirm his theoretical ideas by "reasoning and observation". He was also careful to familiarize himself with all the more important recent developments resulting from the European epizootic, quoting freely from the Cogrossi–Bonomo–Vallisnieri correspondence of 1713[117] and comparing the view expressed there with current observations of grubs[118] in mouth and throat ulcers of diseased animals.[119] Interested also in smallpox, he invoked the recent inoculation experiments to prove his point, concluding that "we must affirm that, like

[112] Ibid., p. 38; my italics.

[113] Ibid., pp. 39–41; my italics. The reasoning here seems to be a natural progression from Aristotle, cf. M. Delbrück's 'Aristotle-totle-totle', in Jacques Monod and Ernest Borck (editors), *Of microbes and life*, New York, Columbia University Press, 1971, pp. 50–55.

[114] He quotes the figures of "4000 from one seed of *Helianthus;* 32,000 from one poppy seed; 40,320 from one seed of the tobacco plant", and invokes "Linnaei foundations of botany". Plenciz, op. cit., note 106 above, p. 42. Here the seed analogy may be seen to have finally arrived in truly realistic terms, cf. Nutton, op. cit., note 11 above.

[115] ". . . as the year 1761 draws towards its close, this dreadful plague seems to be getting worse from day to day . . .", Plenciz, op. cit., note 106 above, p. 200.

[116] Ibid., pp. 140–142.

[117] Ibid., p. 145.

[118] Plenciz used the terms *"animalculae"* and *"vermiculi"*.

[119] Ibid., pp. 144–145.

smallpox and measles, cattle plague is infectious and communicable".[120]

Plenciz discussed in some detail the possible causes of the cattle disease. Unlike Lancisi, but like Cogrossi and Vallisnieri, he was willing ultimately to accept a link between the small "worms and grubs" seen with the aid of the microscope in the ulcerated areas of the mucous membranes of dead and dying cattle and the aetiology of the disease.[121] He believed that "seeds", or eggs, of such small creatures could lie dormant at the base of grass in the pastures, or be brought by rain and prevailing winds, to be ingested by healthy cattle, ". . . so thereby they infect the whole sum of their humours and dispose them to putrescence and the hatching of the eggs at the first given opportunity".[122] He finally summed up his ideas on the aetiology of cattle plague in the following words:

> If the matter of the little ulcers which attack the nose and throat, or of those abscesses, be examined with a microscope, always innumerable little grubs[123] are observed; which we can also see if the liquids which are contained in the vessels are subjected to such an examination, as has already been shown in the treatise on infection. Whence it comes about that, if a portion of such material is applied in other healthy oxen as a grafting or inoculation of the same disease, that same disease is spontaneously generated; as customarily happens in the case of smallpox and measles.
>
> From this again the material cause of cattle plague becomes clear. Because, if it can be communicated, spread and multiplied in the same way as smallpox, it necessarily follows that it likewise has the same elements, that is grub-bearing seeds.[124]

As Löffler was to note many years later, in the absence of any kind of tangible proof there was no possibility of Plenciz' work becoming seminal, and it attracted little more attention than had the correspondence of Cogrossi and Vallisnieri earlier in the century.[125] His observations on cattle plague were noted, albeit briefly and without much enthusiasm, in 1766, by Claude Bourgelat who, in the meantime, had established the first schools devoted to the training of veterinary practitioners, in Lyons (1762) and in Alfort outside Paris (1765). Bourgelat's remarks were published as notes and comments to an essay on cattle disease which in 1765 won for its author, Denis Barberet, a prize offered by the French Royal Society of Agriculture.[126] Barberet was no bright young student full of revolutionary ideas, but a middle-aged physician who seems to have made a practice of writing competitive essays on a variety of subjects whenever prizes were offered.[127] His essay on cattle diseases is unremarkable and more a compilation of the views of other authors than a work bursting with original ideas. He made a distinction, as was the custom at the time,

[120] Ibid., p. 202; the inclusion of measles here is interesting and indicates Plenciz' awareness of recent inoculation experiments claimed to have been successful by Francis Home; see L. Hektoen, 'Experimental measles', *J. infect. Dis.*, 1905, **2**: 238–255. Plenciz had mentioned Home's experiments in his tract on smallpox, ibid., tract II, *De variolus*, p. 22.

[121] Ibid., pp. 144–145.

[122] Ibid., p. 208.

[123] "*vermiculi*".

[124] Ibid., pp. 214–215; Plenciz' term for grub-bearing seeds is *seminia verminosa*.

[125] Cf. Löffler, op. cit., note 1 above, p. 11.

[126] The Société d'Agriculture de la Généralité de Paris was established by royal decree on 1 March 1761. W. A. Smeaton, 'Lavoisier's membership of the Société Royale d'Agriculture and the Comité d'Agriculture', *Ann. Sci.*, 1956, **12**: 267–277, p. 267.

[127] In 1750, he had been awarded a prize for an essay on the relationship between thunder and electricity; in 1762, he gained another for a tract on spoiling of wine; and the essay here discussed was his third success. N. F. J. Eloy, *Dictionnaire historique de la médecine*, Mons, H. Hoyois, 1778, p. 255.

between epidemic diseases and contagious ones. He believed, also true to prevailing ideas, that epidemic diseases of livestock were caused in most cases by contaminated feedstuffs, especially rust-infested grasses and grain,[128] while contagious ones originated in epidemic constitutions of the air[129] when it was charged with contagious miasmas. As with many another author before and since, this wretched question led him into difficulties he was unable to resolve with any clarity. Barberet made no mention of Plenciz. Bourgelat, in a note, did little more than draw cursory attention to his work, summarizing his ideas on cattle plague in one sentence: ". . . he attributes its cause to putrid, verminous miasmas, a viewpoint based on what, with the aid of the microscope, he has observed in the various ulcers extending from the mouth and throat of the diseased animals to their lungs and stomachs".[130]

Although Barberet had few thoughts of a constructive nature to contribute to the search for an aetiology, his directions for preventive measures, although also derivative, were less vague and more to the point. He wrote in his conclusion:

> Because the virus, the contagious miasmas, even when long dormant, conserve their full force, can one be too careful when cleaning the stables? It is not enough to clean them and keep the doors and windows open, it is necessary to wash the floor and the walls with vinegar and quicklime, and to fumigate them with scented herbs, boiling vinegar, and spirit of saltpetre . . . .[131]

The vague and conservative views of aetiology expressed by Barberet and by Bourgelat[132] were reflected during the following decades in three major works appearing in France in the wake of the opening of her first veterinary schools,[133] and influenced by the continuing threat to livestock of the cattle plague as well as other epizootics.[134] The authors were all physicians, and their works were published certainly with royal approval, and in some cases even by direct royal command.[135] If they all shared a reluctance to discuss in any detail initial causes of the prevailing epizootics, they did not hesitate to refer casually to the cause of the cattle plague as a "pestilential virus" communicated by means of infected saliva.[136] They also shared an ability to give sound epidemiological advice. Bost has remarked that this was the great

[128] D. Barberet, *Mémoire sur les maladies épidemiques des bestiaux*, Paris, Veuve d'Houry, 1766, p. 27.

[129] Ibid., p. 35.

[130] Ibid., note 6, p. 70.

[131] Ibid., pp. 63–64. In the 1760s, this had become standard practice. George Fleming, *Animal plagues: their history, nature, and prevention*, London, Chapman Hall, 1871, pp. 409–411, described very similar measures taken in the kennels of Louis XV's hunting dogs during the same decade.

[132] Although Bourgelat owed, in part, the political support he received to establish his schools to the need for qualified practitioners to deal with the threat of cattle plagues, his main interests lay in the care and diseases of the horse, and even there his concern was primarily pragmatic and his interest in aetiology cursory.

[133] Other schools opened in rapid succession throughout Europe, in nearly all cases in particular response to the need to combat the scourge of cattle plagues.

[134] As before and since, foot-and-mouth disease and bovine pleuropneumonia (cf. note 6 above) were frequently present alongside rinderpest at this time. The Swiss outbreak which spurred Albrecht von Haller into writing on "cattle plague" in 1773 is thought to have almost certainly been bovine pleuropneumonia, see Fleming, op. cit., note 131 above, pp. 446–460.

[135] They were: Felix Vicq d'Azyr, op. cit., note 105 above; Jean-Jacques Paulet, *Recherches historiques et physiques sur les maladies épizootiques*, Paris, Ruault, 1775; and Louis Vitet, *Médecine vétérinaire*, Lyons, Frères Perisse, 1771. The works of Vicq d'Azyr and of Paulet both proclaim themselves to have been "Publiées par ordre du Roi".

[136] Cf. Vicq d'Azyr p. 7 and Vitet p. 275 (vol. II).

advance made in the early years of the veterinary schools in France; making their staff and students available to help in areas affected by epizootics,[137] they laid a basis in epidemiological and clinical terms for the bacteriological revolution which was to take place in the following century. Further, Bost suggested that the practical success of this new generation of practitioners may have been due to a freshness of approach lacking in those more hidebound by a traditional medical outlook, however distinguished, pointing to Bourgelat's links with the encyclopaedists and his consequent refusal to indulge in "useless dissertations on primary causes of disease".[138]

Whatever freshness of approach these early veterinarians may have shown in the field of epidemiology was absent from their scanty considerations of the nature of the infective agent. Perhaps it was indeed the influence of the encyclopaedists, perhaps they were simply expending all their energies on the more immediate problems of controlling the severe epizootics. Certainly, during the last quarter of the eighteenth century the several distinguished treatises published on rinderpest paid remarkably little attention to questions of aetiology.[139]

At the end of the century, Italy, where the European outbreak had begun in 1711, was again in the grip of a major epizootic of rinderpest, this time centred on Piedmont.[140] It was described in detail in two tracts, published in 1793 and 1798 by M. F. Buniva.[141] Buniva's emphasis was also on practical matters of control, and he devoted a chapter to examples of the many pathways of transmission,[142] using his examples as proof of the contagious nature of the disease without in any way committing himself concerning the nature of the agent. In a paper read to the Société de Médecine in Paris two years later on an epizootic among cats, he made his feelings known regarding the arguments for and against the idea of a *contagium vivum*. He wrote:

> The partisans of a *pathologia animata* claim that this epizootic, like all other similar ones in domestic animals and in man, is due to infinitely small animalcules. My learned colleague Vassali and myself have made certain experiments and observations with regard to this question. The results, laid before the Société Philomatique, would appear to confute this system.[143]

[137] J. Bost, 'Les écoles vétérinaires françaises (Lyon et Alfort) face aux épizooties du XVIIIème siècle', in *Histoire des grandes maladies infectieuses*, Institut d'Histoire de la Médecine, Université Claude Bernard, Lyon I, Cycle de conferences, 1979–1980, pp. 135–162, see p. 149.

[138] Ibid., p. 160.

[139] This applied also to the works of Camper, cf. note 99 above, and of Haller, cf. note 134 above. Camper recognized the general contagiousness of the disease but dismissed what he called Vallisneri's theory of the worms, and added that not only was he himself ignorant of the actual origin of the infection, but that such matters were "beyond my comprehension and that of all mankind". P. Camper, 'Leçons sur l'épizootie qui règna dans la province de Groningen en 1769', in: *Oeuvres de Pierre Camper qui ont pour objet l'histoire naturelle, la physiologie et l'anatomie comparée*, vol. 3, Paris, H. J. Jansen, 1803, see p. 120.

[140] The 1711 outbreak in the Venetian territories had reached Piedmont in 1712, and there had been later epizootics there in 1735 and again in 1743 (M. F. Buniva, 'Mémoire. Contenant les plus remarquables notices historiques, et les résultats les plus intéressans de ses observations et expériences, relatives a l'épizootie bos-hongroise qui fait des ravages en Piemont depuis la fin de l'an 1793', in *Recueil de mémoires et observations-pratiques sur l'épizootie*, Lyons, Reymann, 1808, pp. 149–202, see pp. 149–157).

[141] Michele Francesco Buniva (1761–1834) was a native of Piedmont, qualifying in medicine at Turin in 1781, and subsequently becoming interested in animal epizootics, and later in Jenner's vaccination procedures. Bredin, *Notice biographique sur le professeur Buniva, de Turin*, Paris, Mme. Huzard, 1835; see also, Leon Moule, 'Du rôle des médecins dans la lutte contre les épizooties au XVIII^{e} siècle', *2nd Int. Congr. Hist. Med.* Paris, 1921, pp. 237–274.

[142] Buniva, op. cit., note 140 above, pp. 185–202.

Buniva did not elaborate concerning these experiments, nor did he disclose which "partisans of a *pathologia animata*" he had in mind. He may have been referring to followers of Linnaeus and of Plenciz, who were both dead by the time he wrote;[144] Reimarus made a cautious and fleeting reference to tiny organisms smaller than infusoria in 1794.[145] Very few authors speculated along such lines during the years linking the eighteenth century to the nineteenth, and none made any great impact. Certainly, rinderpest was not again to hold centre stage in the history of infectious diseases until the disastrous English outbreak of 1865–66.[146] In the meantime, the stage was set for the more realistic and often acrimonious arguments accompanying the debate between the anticontagionists and the new breed of experimentalists, both physicians and veterinarians and some who had taken advantage of the opportunities offered by the new veterinary schools to obtain both qualifications, who were to establish the science of microbiology on a firm basis from the middle of the nineteenth century onwards.

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[143] M. F. Buniva, *Observations et expériences sur la maladie épizootique des chats qui règne depuis quelques années en France, en Allemagne, en Italie, et en Angleterre*, Paris, Société de Médecine, 1800, p. 11.

[144] The ideas expressed by Linnaeus in this area were not very clear as he recognized himself when he placed the world of animalcules in the genus "Chaos". Cf. Löffler, op. cit., note 1 above, p. 9; L. Munster, 'Ein vergessener Vorkämpfer der Parasitenlehre: Agostino Bassi aus Lodi', *Janus*, 1933, **37**: 221–246, p. 237.

Plenciz had died in 1786, but his work could still have been fresh in the mind of Buniva in 1793; presumably Plenciz is meant by Munster's reference to "Placiz", (ibid., p. 238).

[145] J. A. H. Reimarus (1729–1814) qualified in medicine at Leiden in 1757 and later held a chair of physics and natural history in Hamburg. His sole contribution to the literature on infectious diseases appears to have been 'Vorrede über die allgemeinen Eigenschaften ansteckender Seuchen', in the German translation, published in Hamburg in 1794, of d'Antrechau's volume on the plague in Toulon in 1721.

[146] See Sherwin A. Hall, 'The cattle plague of 1865', *Med. Hist.*, 1962, **6**: 45–58.