PROBLEMS

*Plague in Europe and the Mediterranean Countries**

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It is hardly exaggerated to assert that without the plague pandemic of the fourteenth to nineteenth centuries the whole course of European history would have been radically different. For a period of 300 years plague probably exercised the single most effective check on the growth of western European population. Without plague, the kind of uninterrupted population growth that has characterised and transformed western Europe since the eighteenth century would have begun several centuries earlier. True, had plague not appeared in the mid-fourteenth century to cut back earlier population growth, some other killer disease, or at least starvation, might have been substituted to check the growth of population: but in the event it was plague that was cast in this role. There could have been few men or women in Europe, it has been remarked, who attained the age of twenty-five at any time between the mid-fourteenth and mid-seventeenth centuries without living through the scarifying experience of a least one plague epidemic. It is hard in the late twentieth century to place oneself in one's imagination in such a context. Plague was highly infectious, and the case fatality rate ranged from 60 to 100 per cent. When an epidemic struck a community — and between the mid-fourteenth and mid-seventeenth centuries many communities in Europe did not enjoy immunity from epidemics for more than a decade at a time — every person stood a high chance of catching it sooner or later, and an equally high chance of dying from it when caught. We all have to die sooner or later, but death from plague was

* I am grateful for helpful comments on an earlier draft of this article from my colleagues Rosalind Mitchison and Christopher Smout. They do not, of course, bear any responsibility for the interpretations suggested here.

mostly premature and always hideously unpleasant. If for no other reason, it is better to live in Europe in the twentieth century than at any time between the fourteenth and eighteenth centuries.

Because it has had such an influence on the course of late medieval and early modern historical development, and because it was such a major recurring element in the lives of people of all sorts and conditions, it has forced itself on the attention of historians. The developments in the techniques of demographic history of the last twenty years have, however, brought a new purpose to the study of plague in history, and the last ten years in particular have given birth to a number of studies which have set a new pitch of scholarliness and relevance for demographic history.[1] In particular, a group of articles in the 1960s [2] alerted interested scholars to the fact that a French historian, Jean-Noel Biraben, was studying plague history on an entirely new plane of scope and detail. These articles served as attractive appetisers for the *magnum opus* that was to come. His monumental study of the plague in France, Europe and the Mediterranean appeared in two volumes in 1975 and 1976.[3]

This endlessly fascinating and highly readable study achieves many objectives. It surveys and synthesises previous work in the field. Since the geographical area he embraces is enormous, the literature he surveys is prodigious. Ignoring the original manuscript sources he has also tapped, his bibliography of printed works, primary and secondary, covers 216 pages and embraces more than 3,000 items in almost every European language. Though no doubt other scholars may be able to find the odd omission here and there from this great alphabetical list of writings, it will clearly be many years if not decades before anybody else will undertake the daunting task of updating so comprehensive a listing.

Secondly, Biraben presents, as far as is humanly possible, the full details of the geography of the plague epidemics in the great pandemic launched by the Black Death of 1347-50. This is handled in several ways: chapter 2 of vo-

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[1] B. Bennassar, *Recherches sur les Grandes Epidémies dans le Nord de l'Espagne à la Fin du XVIe siécle* (Paris, 1969); P. Ziegler, *The Black Death* (London, 1969); J. F. D. Shrewsbury, *A History of Bubonic Plague in the British Isles* (Cambridge, 1970); M. F. & T. H. Hollingsworth, "Plague mortality rates by age and sex in the parish of St. Botolph's without Bishopsgate, London, 1603," *Population Studies*, 25 (1971); C. Morris, "Plague in Britain," *Historical Journal*, 14 (1971); I. Sutherland, "When was the Great Plague? Mortality in London, 1563 to 1665," in D. V. Glass & R. Revelle (eds.), *Population and Social Change* (London, 1972); J. Hatcher, *Plague, Population and the English Economy, 1348-1530* (London, 1977); T. C. Smout, "Part 3, The Seventeenth Century" in M. W. Flinn (ed.), *Scottish Population History from the Seventeenth Century to the 1930s* (Cambridge, 1977); *The Plague Reconsidered* (Local Population Studies, Matlock, 1977).

[2] In *Le Concours Medical* (1963); and *Daedalus* (1968).

[3] J. N. Biraben, *Les Hommes et la Peste* (Paris and The Hague, 2 vols. 1975 and 1976).

lume one presents a verbal account in which the European experience is surveyed under a range of headings; chapter 4 of the same volume studies, and attempts with considerable success to quantify, the human losses arising from the plague. This chapter concentrates heavily on France, and there is an important section, as Biraben's earlier publication would lead us to expect, on the epidemic of Provence in 1720-2. Then there is a series of fascinating maps illustrating the routes taken by advancing infection in each of the major epidemics in the two pandemics of the sixth to eighth centuries, and the fourteenth to eighteenth centuries. Finally, Biraben has recorded in tabular form chronologically under separate countries every known place or region where an outbreak of plague has been recorded. The combined effect of these presentations is to put the geography of plague epidemiology in Europe and the Mediterranean countries beyond further doubt.

Thirdly, Biraben brings to his study a breadth of approach which makes plague history virtually a new subject. This originality is largely concentrated in volume two sub-titled ' Les hommes face à la peste '. Here he examines men's reactions to plague, looking firstly at the explanations they have offered for its appearances and reappearances over the centuries. After the initial shock in the mid-fourteenth century and when it was recognised that the plague was not a once-for-all catastrophe but a recurring scourge, there began a long struggle to explain the phenomenon, at first in terms of supernatural manifestations, and later, one may imagine as advancing Renaissance explorations of scientific principles began to give a new edge to men's rationalising, in terms of the various possibilities of contagion. From these reactions, Biraben moves on to what is at once his most interesting and valuable contribution to plague studies — the examination of man's administrative reactions. These, as Biraben shows, were obviously constrained by his understanding of the etiology of the disease. Supernatural causation called for the usual responses of sacrifices, exorcisms and talismen; late medieval Christianity, on the other hand, invoked the whole gamut of prayers, processions, pilgrimages, candles, mystery plays, relics and saints. These in their turn proved minimally effective. To the more observant and pragmatic, however, the repeated experience of epidemics pointed to certain logical conclusions, and communal action in respect of the isolation of the infected and the control of movement began to evolve.

Finally, Biraben tackles the explanation of the etiology of plague in modern terms. Two aspects of plague history have always perplexed historians — how infection spread, and how to account for its ultimate disappearance from Europe. Hypotheses in both these areas have come and gone, but with the increasing interest in recent years in demographic history, satisfactory answers to these fundamental questions seem more urgent. On both these issues Biraben adopts a distinctive and unequivocal standpoint. Readers will react variously to his views: those brought up on English historical writing will find most to surprise them; those familiar with the work of German-language authors least.

It is the intention of this article to consider Biraben's conclusions in these two important areas in the light of other recent work in the field of plague history.

In the discussions that have ranged round these two problems of plague history there has been a tendency for scholars to have more regard to what has been written in their own language than to what has appeared in other languages. Thus in recent years at least the interpretations commonly accepted by anglophone readers in respect of the first of these two problems — the etiology of plague — have been heavily influenced by Shrewsbury's authoritative views,[4] particularly as they were modified by Morris's famous review article.[5] From these explanations English readers understood the role of what seemed to be effectively the sole vector of the plague bacillus, *Pasteurella pestis*, the rat flea *Xenopsylla cheopis*. Though *X. cheopis* normally lived on rats, and showed what now appeared to be only a marginal preference for the black rat (*Rattus rattus*) over the brown rat (*Rattus norvegicus*), it was also allowed that this flea from time to time adopted other rodents including squirrels and marmots. (It is mostly this group which, in the late twentieth century, provides the few surviving world reservoirs of enzootic plague infection in the Americas, Africa and Asia). Other rodent fleas, notably *Nosopsyllus fasciatus*, could serve as vectors, but all tended to be less efficient in this capacity than *X. cheopis*. It was clear that a plague epizootic was a necessary pre-condition for any plague epidemic, and an understanding of the ways in which infection spread among humans hinged on the study of the movement of rats. When Shrewsbury demonstrated that black rats rarely strayed far from their nests close to human habitations it was clear that the constraints on the diffusion of the disease were powerful, so powerful indeed as to raise questions about how it was possible for wave after wave of the disease to sweep across Europe with the inevitability and speed that has been demonstrated in many studies of the disease. Biraben, for example, is able to show (I, 90) that in the epidemic of 1348-9 infection moved across France at anything between 1 and 4 kms. per day.

Shrewsbury and other writers, however, insisted that rats were unlikely to move unaided over anything other than very short distances, so that it was apparent that man had to play some part in the diffusion of infection. Either rats were carried in the course of human commercial intercourse by ship or by wagon, or the fleas were similarly carried in goods, perhaps after the death from plague of their rat hosts. An infected flea could apparently remain alive and infectious for up to fifty days. This capacity of fleas to move (or rather be moved) independently of their normal host rats for substantial distances overcame the relative immobility of the rats and made it possible for infection to move at human speeds rather than at a rat's more restricted pace and limited distance.

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[4] SHREWSBURY, *op. cit.*, pp. 1-16.

[5] MORRIS, *loc. cit.*

The position, then, for anglophone students of plague history in the early 1970s, was that humans were infected by bacilli passed into the bloodstream by blocked rat fleas which moved either short distances on rat hosts, or longer distances either on rats carried on human transports, or in human baggage. The infected human could not directly infect another human, and the disease could spread geographically only to the extent that blocked fleas were moved by rat hosts or transports of goods.

It was this extremely restricted etiology that permitted Shrewsbury to assert that the incidence of, and mortality from, bubonic plague were more limited than had hitherto been assumed. With such an etiology, the disease was likely to be heavily restricted to urban environments,[6] while, knowing that *X. cheopis* hibernated in winter and could not survive at all in low temperatures, what had popularly been assumed to be plague incidence in winter must, of necessity, be another disease, probably typhus, wrongly diagnosed as plague.

The problems arising from Shrewsbury's analysis were explored by Morris, who showed that Shrewsbury had largely ignored the possibility that forms of plague other than bubonic had prevailed from time to time. Pneumonic plague, in particular, spread by droplet infection in the human breath, is extremely infectious as well as being almost invariably lethal. Moreover, since it did not depend on the independent movement of vector fleas, it was not restricted to the summer months. When, as in the Black Death, infection and mortality persisted between one summer and the next, or when mortality was exceptionally high, there is a high probability that it was pneumonic rather than bubonic plague. Shrewsbury was rightly criticised for taking no account of the pneumonic form of the disease, and an alternative etiology was established which allowed for the easy spread of an extremely lethal form of infection at as fast a pace as humans could travel.

Biraben does not accept such a restricted etiology. The plague bacillus, he argues, did not reserve its favours exclusively for the rat flea *X. cheopis*, but was ingested also by the human flea *Pulex irritans*. Morris had allowed for the role of *P. irritans* only in the extremely rare case of septicaemic plague: Biraben, however (I, 13), asserts categorically that *P. irritans*, which thrives only on humans, 'transmits the infection very effectively'. A vector role for the human flea would, of course, allow for direct human-to-human infection. This is a point of great importance, since it radically alters existing etiological understanding, and is directly contrary to the most explicit statements hitherto made by many English-language historians on plague etiology. 'The transmission

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[6] In this view Shrewsbury is not without supporters, and it may be that it conforms most clearly to the English experience. But it is not confirmed by experience in northern Spain in the late sixteenth century (Bennassar, *op. cit.*, pp. 15–21) nor in Provence in the early eighteenth century (Biraben, *Daedalus* (Spring, 1968), 541–4.

of bubonic plague is from rats to humans', Appleby has stated recently, 'not from humans to humans'. In support of this view, he argued that 'the human almost invariably dies before the plague bacteria-count in the blood reaches the level commonly found in rats with plague. The chance of a flea ingesting bacteria from a human is therefore much smaller than from a rat'.[7]

Biraben's explanation of this etiology is brief, one supposes, because, in spite of the confidence of English-language historians to the contrary, he accepts that it has long been established beyond doubt. Readers familiar only with the English-language historical literature may find this assumption curious,[8] and for this reason it might have been advantageous had he reinforced his explanation with wider bibliographical references to the relevant medical literature. This literature is comprehensively and critically surveyed by Pollitzer[9] and Hirst,[10] the leading modern medical authorities on plague, both with extensive personal experience of twentieth-century plague epidemics in non-European areas. Of the two, Hirst takes up a position least favourable to the role of the human flea. He found that the correlation between *X. cheopis* density and human plague was so close that in most cases there was no need to look further for explanations of epidemics. He did not deny, however, that human ectoparasites could be carriers of *P. pestis* and quoted many medical authorities who had argued that the human flea could be, and had been in certain epidemics, a principal vehicle for the dissemination of infection. What seems to emerge from Hirst's examination of this problem is that the relative importance of *X. cheopis* and *P. irritans* may vary from one part of the world to another (presumably for climatic reasons), and has varied from time to time in the past (perhaps because of mutation). The human flea is undoubtedly a less efficient carrier of *P. pestis* than are rat fleas, but it seems that, where human populations are concerned, it is capable of making up in numbers what it lacks in efficiency. 'Let it be admitted that human fleas present *en masse* are capable of carrying plague between human beings in homes where a man has died acutely of plague of rat origin', concluded Hirst. 'The belief that *P. irritans* was an important auxiliary in transmitting the infection', he went on, 'is most plausible for the period of the Black Death'.[11]

Pollitzer, who similarly favoured the primacy of rat fleas as vectors, also accepted the subsidiary role of *P. irritans* in the transmission of infection.[12] He,

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[7] A. B. Appleby, "Famine, mortality and epidemic disease: a comment," *Economic History Review*, 2nd ser. 30 (1977), 509, n. 5.

[8] Ladurie differentiates quite simply between the "English, rat school" and the "French, human flea school." (E. Le Roy Ladurie, "Un concept: l'unification microbienne du monde (XIVe-XVIIe siècles)," *Revue Suisse d'Histoire*, 23 (1973), 632).

[9] R. Pollitzer, *Plague* (WHO, Geneva, 1954).

[10] L. F. Hirst, *The Conquest of Plague* (Oxford, 1953).

[11] Hirst, *op. cit.*, pp. 236-46.

[12] Shrewsbury seems not to have consulted Pollitzer.

too, recognised the geographical variability of the parts played respectively by human and rat fleas. He referred to Girard's hypothesis of 1943 that one chain of infection might be from rodent to man, man to man, and back from man to the rodent, noting that ' each link of this chain was apt to assume greater or less importance according to the conditions peculiar to each plague area '. ' Therefore ', he argued, ' without refuting claims made in regard to each plague area, one ought not to consider them generally valid .... One must certainly agree with this conclusion. It is conceivable that, in areas like Morocco, where *P. irritans* occurs abundantly, it might play an important role in the transmission of plague, the high incidence of this species compensating for what it lacks in vector capacity. However, it is certain that, in other plague areas, e.g., China, India, and Madagascar, the role of the flea is negligible, the transmission of the infection depending upon the rat fleas, particularly *X. cheopis* '.[13]

So far as historians are concerned, the role of *P. irritans* was also accepted as far back as 1953 by Rodenwaldt in a study of the etiology of the plague in the 1575-77 epidemic in Venice.[14] His analysis was confirmed by Lesky's careful study of 1957 of the operation of anti-plague measures embodied in the Austrian *cordon sanitaire* of the eighteenth and nineteenth centuries (of which more later).[15] Further support to this view was given by Mattmüller in his 1973 study of Swiss precautions in the seventeenth century.[16] Biraben is thus treading a path which, if unfamiliar to English and possibly some French readers, is nonetheless firmly established.

Shrewsbury's etiology via *X. cheopis* is in no way denied by Biraben's hypothesis, merely augmented by it. In Biraben's etiology bubonic plague could be disseminated both by the movement of *X. cheopis* from a dead rat, and by *P. irritans* moving from human to human by physical contact, by exchange of clothes, or common use of bedclothes. Though in Europe summer conditions are likely to be more favourable to the spread of infection by either means, winter conditions do not necessarily rule it out by the latter. A plague epizootic in rats or other rodents may still be the most common origin of an epidemic among humans: but once transmitted to humans via *X. cheopis* the infection may seemingly be diffused from human to human via *P. irritans*.

If this is the position established by both medical and historical writers on the subject, it still, however, leaves some puzzles, some unanswered questions. Bubonic plague seems most commonly to have followed the seasonality and

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[13] POLLITZER, *op. cit.*, pp. 380-1.

[14] E. RODENWALDT, *Pest in Venedig, 1575-1577. Ein Beitrag zur Frage der Infektkette bei den Pestepidemien West-Europas* (Heidelberg, 1953), quoted by Lesky (see note 15), pp. 102-3.

[15] E. LESKY, " Die österreichische Pestfront an der k. k. Militärgrenze," *Saeculum*, 8 (1957).

[16] M. MATTMULLER, *Einführung in die Bevölkerungsgeschichte an Hand von Problemen aus dem Schweizerischen 18 Jahrhundert* (mimeograph, Basle, 1973), pp. 128-140.

geography of rat flea infection; but, if human fleas are capable of transmitting infection, why was the ebbing of epidemics in winter so commonly the pattern, particularly in northern Europe? Could an epidemic be sustained only by a massive reservoir of infection such as would exist in a rat (or other rodent) population? Why did the northerly parts of Europe — northern Scotland and northern Scandinavia — enjoy almost complete immunity from the disease throughout the later stages of the pandemic? The human flea was evidently not so selective of the range of temperatures necessary for survival as was the rat flea. Merely to assert that the human flea was a less efficient carrier of infection than the rat flea is scarcely an answer to these questions.

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The second major problem on which Biraben also throws new light for anglophone readers is the important question of why plague died out in Europe, a slow process stretching from the mid-seventeenth century to the mid-nineteenth. On the western and northern European periphery the last appearances on a serious scale of bubonic plague were in the 1640s in Scotland, the 1660s in England, the 1710s in Scandinavia; on the continental mainland the last appearances were the 1650s in Italy (apart from the isolated epidemic in Sicily of 1742-4), the 1680s in Spain, the 1710s in the Low Countries and Central Europe, the 1720s in France, the 1810s in Russia, and the 1840s in the Turkish empire. Various hypotheses have been advanced from time to time to explain the withdrawal, only to be subsequently rejected — the replacement of the black rat by the brown,[17] the cutting of land routes between Asia and Europe by the Turkish advances into south-east Europe, the substitution of brick for lath-and-plaster in house construction, and the emergence of breeds of rats with an inherited immunity to infection.[18] But as late as 1968 it was still possible for a distinguished French demographic historian to assert that ' we are still basically unable to explain the retreat of plague in the seventeenth century '.[19]

Biraben obviously does not have much use for any of these possible explanations since he devotes little if any attention ot them. He does, however, review, with a wealth of local illustrative detail, the various measures taken by individuals, by communities and by states to protect themselves against the plague. His study of these measures is an essential part of his argument since, in his view, western Europe was cleared of plague by these corporate human actions.

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[17] Most authoritatively argued by Macfarlane Burnet, *Natural History of Infectious Disease* (Cambridge, 3rd edit., 1962, pp. 326-7).

[18] This case was supported by Morris (*loc. cit.*, 212-3) and has recently been cogently re-asserted by A. B. Appleby, *loc. cit.*

[19] R. Reinhard & A. Armengaud, *Histoire Générale de la Population Mondiale* (1968 edition, Paris), p. 199.

Most of the actions taken by individuals, such as the use of perfumes, fumigation by tobacco smoke, cleaning of streets, etc., were probably totally ineffective. In the absence of an understanding of plague etiology, one form of action by individuals only was any use — what was neatly expressed by an Italian writer of the sixteenth century as ' pills made of three ingredients called *cito*, *longe* and *tarde* (swiftly, far and tardily), namely run swiftly, go far and return tardily '.[20] On the whole, this expedient was available only to the wealthy. Moreover it tended to be denied increasingly by local and national authorities during the sixteenth and seventeenth centuries. It was a different matter, however, with communal action. Responses, of course, were conditioned by assumptions about etiology. So long as people thought that plague was a manifestation of God's anger, or that it was a direct product of astrological conjunctions, eclipses, phases of the moon, comets or earthquakes, then their reaction could be none other than acceptance. But when, from the sixteenth century, theories of contagion began to be developed, there also emerged the possibility that human action could hinder, if not prevent, the spread of the disease. It was thought at first that the infection was passed from one person to another by supernatural beings ranging from the devil himself to witch-like women. Alternatively, it was possible for the air itself to be corrupted, particularly by corpses, and become capable of engendering the disease. More realistically, there was a belief that corpses of those who had died of the plague or their bedclothes and clothing contaminated the surrounding air, and thus passed the disease on to others. Others thought that merely a look from an infected person could communicate the disease. There was, more remarkably, a theory of *contagium vivium* evolved in the mid-seventeenth century by a small number of doctors in Holland and Italy and in Marseilles during the great epidemic of 1720-2, which postulated the spread of the disease by invisible animalcules in the air, even parasitic ones.

Whatever the means, common observation taught that the disease spread with the movement of people or goods: it arrived in a new location from outside. At a very early stage of the pandemic some ports initiated the restriction of movement of people and goods. Ragusa imposed a 30-day quarantine on incoming travellers as early as 1377, with Genoa and Venice following suit in 1380. From the sixteenth century, as theories of contagion encouraged people to envisage the possibility of effective action, a whole series of measures began to be taken by more and more communities until, in the seventeenth century, they were common features of public health regulation of towns, villages and ports in all parts of western Europe. The measures included the formation of a corps of officials with specific anti-plague duties in the event of the appearance of the disease. These sanitary police were required to seek intelligence about the existence of the disease in the neighbourhood of places from which tra-

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[20] Quoted by C. CIPOLLA, *Cristofano and the Plague* (London, 1973), p. 23.

vellers came. When judged necessary, which in the case of Mediterranean seaports might be at all times, certificates of clean bills of health from the place of last call were demanded. Clearance certificates of this kind became a normal feature of sea trade, particularly in the Mediterranean, from the early seventeenth century. The appointment of plague doctors was often made mandatory, and provisions for caring for plague victims were to be prepared, to be brought into action in the event of the appearance of the disease.

Most important, however, was the spread, again from the sixteenth century, but more effectively in the seventeenth century, of regulations against movement either out of an infected area, or into a clear area. In the former class, Biraben quoted (II, 167) some terrifying examples of the forcible prevention of flight from infected places by the frightened population of the surrounding uninfected places. When the plague appeared in the small French town of Cordes in 1628, for example, the authorities of the surrounding villages prohibited any movement out of the town, while the peasants refused to take their produce to the town for sale at its market. The epidemic in Cordes lasted for three years. In 1644, the population, which had been around 2,000 before the plague, was no more than 304. In 1586 the Parlement of Aix in Provence had issued a general order prohibiting flight in the event of an outbreak of plague, and when in 1629 the epidemic hit the town of Digne, the order was renewed. The inhabitants of the surrounding communities, out of fear of the disease spreading, encircled the town with guards which Biraben describes as having more the appearance of a siege than a *cordon sanitaire*. There were even, he reported, those among the people of the surrounding villages who went so far as to advocate the destruction of the town with all its inhabitants by means of an incendiary bombardment. Here, too, the epidemic led to extremely high mortality.

Biraben's accounts of the rigour with which the restrictions on movement were enforced in particular localities during epidemics may be multiplied endlessly. At the time of the Great Plague of London in 1665–6, for example, the inhabitants of the small Gloucestershire town of Tetbury met together at a general meeting and promulgated a set of regulations for meeting the emergency. A council of eight principal householders was set up to execute the agreed rules which ordered

> 'that noe traveller from the citty of London, or any other place, nor any goods whatsoever, shall be recd into the sayd towne, unless good satisfaction by certifficate, or otherwise, shall be first given, to two or more of the sd eight persons, that the said traveller, or goods, have not for the space of a month before, at the least, been in any place infected. That if any householder shall permit any such traveller, or other stranger, or other person whatsoever, or any

goods, to come or bee brought into his or her house, before such satisfaction soe as aforesd given, to the sayd eight persons or two of them then the sd householder and all of his or her family, together with the said stranger, or other person or such goods shall be removed to some place out of the towne, there to remaine for a month, and for such longer time, if there shall be apparent cause for it, as shall be thought fitt by the greater number of the sd eight persons.
That noe carrier, by horse, waggon or waine, or other carriage, shall bring or drive their hourses, waggons, or waines, or other carriages into, or through the sd towne, or to baite, or lodge in the sd towne, but be directed to passe some back way by the sayd towne according as his or her journey lieth, being furnished with such necessaries, at reasonable rates, as the sayd towne will afford to be delievered to them out of the towne... '[21]

Tetbury avoided the fate of Eyam.

Another example to add to those cited by Biraben comes from the village of Lourmarin in Provence during the last great epidemic of 1720–2. When news of the outbreak in Marseilles, thirty miles to the south, reached Lourmarin in August 1720, the village Council immediately established a Bureau of Health which operated independently of the Council, met three times weekly for the 18-month duration of the emergency, and was endowed with extensive power. It was responsible for the issue of *billets de santé*, and established a mechanism by which nobody was allowed to enter the village from outside without a similar *billet* issued by comparable authorities in other communities. Because the village was not walled, a makeshift defensible perimeter had to be created by the boarding up of all windows and doors facing the outside of the village, and the establishment of guarded gates at the entrances to the village. A *garde bourgeoise* was created from among the residents to maintain the guards. A suitably isolated house was appointed as a quarantine station to be used for the 40-day quarantine isolation of any arrival lacking a *billet de santé* or for any family within the village suspected of infection. There is ample evidence of the most determined enforcement of these regulations despite consequential economic hardship and the tensions generated within a small, tightknit community during the period over which the precautions endured. Though plague entered and decimated neighbouring villages less than ten miles away, Lour-

[21] A. T. Lee, *The History of the Town and Parish of Tetbury* (London, 1857), pp. 21–25.

marin's vigilance and self-imposed hardship were rewarded by freedom from infection. It is worth noting that, apart from the steps taken by individual localities like Lourmarin, and by the provincial Parlement, the central government contributed to the effort to contain the infection: one-third of the infantry of the entire French army and one-quarter of the cavalry was employed in the *cordon sanitaire* around Provence, and though the devastation and suffering within the province were terrible, the infection did not penetrate further into France on this occasion.[22]

Mattmüller, too, has shown how, from the early seventeenth century, the principal commercial centres of Switzerland had all established intelligence networks to inform themselves of the possible existence of plague in any of the towns of other countries with which they traded. By the late seventeenth century they had set up sanitary authorities with wide powers in the event of an epidemic. Berne, it seems, was successful in creating a particularly effective body, partly, Mattmüller suggests, because Berne, being an administrative rather than a commercial centre, had few merchants to oppose the enactment of stringent regulations, and partly also because, for the same reason, there was in the town an ample supply of experienced administrators capable of devising effective regulations and making them work. In the last serious Swiss plague epidemic of 1669, the disease raged in the Bernese Oberland, but the town itself escaped. In April 1665 two travellers were found to have died of plague in a suburb of Baden. Immediately the suburb was isolated from the town, food was left at a distance for the inhabitants to pick up, and no entry was permitted to the suburb. The infection did not enter the town.[23]

Biraben observes that although actions of this kind were multiplied almost universally in western Europe during the seventeenth century, they still did not defeat the disease entirely, though they sometimes limited the spread of infection and protected individual communities. They constituted merely the first phase of the two-stage battle against plague. So long as the measures were local, and left to the discretion or energy of individual communities to take or not to take, there were bound to be loopholes and gaps large enough for the infection to be carried through by man or goods. This was certain to be so when the most influential in towns and villages were the tradesmen, and the immediate effect of any prohibition of the movement of men or goods must be the diminution if not total destruction of trade. In the face of the threat of an epidemic, the businessmen, in whose hands more often than not lay the decisions about plague precautions, were faced with the agonising decision as to whether to risk their fortunes or their lives. Probably it took many epidemics, human cupidity being what it is, to drive home the wisdom of preferring life

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[22] T. F. Sheppard, *Lourmarin in the Eighteenth Century. A Study of a French Village* (Baltimore and London, 1971), pp. 116–127.

[23] Mattmüller, *op. cit.*, p. 140.

to property. But in consequence, it tended to be later rather than sooner that local action became vigorous enough to be effective.

The final conquest of plague in western Europe, according to Biraben, was dependent upon action at the national and international level rather than merely at the local level. This second phase in the struggle against the disease had to wait for the lessons of local action to sink in — that local action vigorously prosecuted *could* be effective in protecting individual communities, but that it could never rid larger areas of infection. Furthermore, one form of human movement that was beyond the control of individual communities was certainly the movement of armies: there is no shortage of evidence, above all from the first two-thirds of the seventeenth century, that armies were a principal vehicle for the dissemination of plague. While the Thirty Years' War and the wars between France and Spain persisted, there could be little hope that even the most determined local regulation could do more than protect individual communities some, but not all, of the time.

The emergence during the sixteenth and seventeenth centuries of more powerful nation and city states began to make more effective action possible. Not surprisingly it was the Italian city states, among the earliest examples of effective government to emerge, that paved the way for the control of *international* movement. Significantly, the word 'quarantine' is of Italian origin. The use of 30-day quarantines, extended to longer periods during an epidemic or for travellers known or suspected to have come from an infected area, spread steadily to most western European countries. The effectiveness of quarantines varied, of course. They interfered with trade, and governements were too frequently willing to sanction exemptions, while port officials were susceptible to bribes. But as time went on, and the effectiveness of a properly enforced quarantine in keeping plague out of a country was demonstrated, the execution of quarantine laws was tightened up. The disasters of Marseilles in 1720 and Messina in 1743 drove home the lesson. At the time of the Baltic epidemic of 1709, Britain imposed a strict quarantine on all vessels coming from the Baltic ports. This measure was re-imposed with greater vigour in 1721 following the outbreak of the Marseilles epidemic.[24] With the good fortune of a sea frontier relatively easily defended, there is little doubt that this eighteenth-century quarantine was a principal means of preventing the re-entry of plague into Britain after the 1660s.

Most of the successive waves of plague in western Europe during the whole pandemic from the fourteenth century originated in the east, generally the south-east, and moved westwards and northwards along the main lines of commercial communications. By the sixteenth and seventeenth centuries this tended to mean that infection moved into western Europe from the Turkish empire.

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[24] C. F. MULLETT, "A century of English quarantine, 1709-1825," *Bullettin of the History of Medicine*, 23 (1949), 528-31.

In the early 1680s the Turkish bridgeheads pushed far into central Europe. The threat to Vienna at last provoked the great counter-offensive by which the Austrians, with the aid of Poland, Russia and other allies, drove the Turks back towards the Balkans. The Peace of Passarowitz of 1718 ultimately stabilised a frontier between the Turkish and Austrian empires which held firm for many decades. It was along this frontier that the Habsburgs set up the *cordon sanitaire* that may have stopped the last and greatest gap in the defences against invading plague.[25] The Austrian 'military frontier' was created in 1728 and the same year produced the imperial 'plague order' (*Pestpatent*). This was reinforced by further orders of 1737 and 1770. These orders placed the sanitary control of the frontier in military hands, and based its regulation on an Italian-type quarantine system. To enforce the *cordon sanitaire* along the immense land frontier of 1,900 kms, a permanent force of 4,000 men was stationed in normal times. To provide this force, Slav 'soldier peasants' (*Soldatenbauern*) were liable to up to five months' military service each year. A system of spies gave warning of the appearance of plague in any part of the Turkish empire, and on receipt of such information the manning of the cordon was immediately stepped up to 7,000 men. When it was known that plague was present in any part of the southern Balkans, on the Moldau, in Wallachia, Serbia, or Bosnia, the manning was further increased to 11,000, at which level each watch-post was within sight by day and within hearing by night of its neighbours along the whole 1,900 kms. Quarantine periods for any person, transport animal or goods wishing to pass the frontier were 21 days in normal (plague-free) times, 42 days when there were rumours of plague in the Turkish empire, and 84 days when plague was known to be present in the Balkans. There were to be no exemptions, not even for Imperial or Porte envoys. The guards were under orders to shoot to kill anyone attempting to evade the control.

Quarantines were passed in specially-built *Kontumazstationen*, within which separation of non-cleared suspects from cleared persons (mostly the permanent staff) was achieved by a cell-like division of the whole structure by 10 feet high walls. On arrival from the Turkish side of the military frontier, travellers, transport animals and goods were separated and subjected to separate treatment. Travellers were stripped of all clothes which were then subjected to an elaborate disinfestation process of fumigation, heating and washing before being returned to the owner while he sat out his lengthy quarantine in a detached cell to keep him out of contact with other quarantiners or cleared personnel. Packs of raw wool and cotton, the principal imports from the Turkish empire, which were believed (probably quite rightly) to be highly suspect as carriers of plague

[25] The following account is based on E. Lesky, *loc. cit.*, pp. 83-102. G. E. Rothenburg's more recent account ("The Austrian sanitary cordon and the control of the bubonic plague: 1710-1871," *Journal of the History of Medicine and Allied Sciences*, 28 (1973) is very brief and adds little of significance to Lesky's article.

infection, were elaborately and exhaustively disinfested. Each *Kontumazstation* was manned by a staff of soldiers, doctors, quartermasters and disinfestation process workers. One of the principal ' stations ', at Semlin on the Austrian side of the border from Belgrade on the main route from Constantinople, had a staff of 54 in 1823. When Kinglake passed through Semlin in 1835 he was deeply impressed by the divisiveness of the border. The worlds on either side, he wrote, ' are as much asunder as though there were fifty broad provinces that lay in the path between them '. ' It is the plague ', he explained, ' and the dread of the plague, that divide the one people from the other. All coming and going stands forbidden by the terrors of the yellow flag. If you dare to break the laws of the quarantine, you will be tried with military haste; — the court will scream out your sentence to you from a tribunal some fifty yards off; the priest, instead of gently whispering to you the sweet hopes of religion, will console you at duelling distance, and after that you will find yourself carefully shot and carelessly buried in the ground of the Lazaretto '.[26]

Some of the more severe aspects of this sustem, like the 84-day quarantine, were modified in 1770, but in essence the system was maintained until 1873. It was not 100 per cent successful in preventing infection from spreading to Austrian territory, but very nearly so. On the few occasions when infection crossed the frontier it was quickly contained and fatalities were few.

Though plague continued to ravage most parts of the Turkish empire for another century after the establishment of the Austrian *cordon sanitaire*, there was no more serious infection in any part of Europe north and west of the Turkish and Polish frontiers. It is Mattmüllers's opinion that the Habsburg measures of 1728 and 1737, which he describes as ' draconian ', played an important part in preventing the recurrence of the disease in western Europe.[27] Curiously enough, Biraben, although (II, 183) referring in his brief conclusion to ' active and massive ' participation by goveernments in the struggle against plague, makes no explicit reference to the Austrian *cordon sanitaire*, though Lesky's important article is listed in his immense bibliography.

In the last resort, of course, there can be no proof that the Austrian *cordon sanitaire* of the second quarter of the eighteenth century was finally instrumental in banishing plague from western Europe. The Austrian cordon, for all its length, was only a part of the great wall separating western Europe from the east and the Turkish empire. To the south and west the cordon was extended by the strict quarantines of the European Mediterranean and even Atlantic ports. But to the north of the Austrian cordon stretched the land frontier between Russia and Poland, or Russia and Prussia as it became after the Polish partitions. The nature and effectiveness of control on this shifting frontier

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[26] A. W. KINGLAKE, *Eothen* (edition London, 1963), p. 15. I would like to thank my colleague Dr. M. R. M. Palairet for drawing my attention to this quotation.

[27] MATTMÜLLER, *op. cit.*, p. 138.

remains almost a closed book to western historians. Biraben lists outbreaks of plague in Poland in every decade of the eighteenth century, though an epidemic of 1797-8 was the last there.

In spite of confining his detailed consideration of anti-plague measures to the activities of towns and villages, Biraben nonetheless believes that the intervention of governments at the national level was crucial in the battle against the disease. The steps taken in the fourteenth and fifteenth centuries, he argues, were infrequent, isolated and, apart from the humanitarian benefits of provision for care of the sick, of little positive effect. In the late fifteenth and sixteenth centuries, counter-plague activity was stepped up generally. From the late sixteenth century, aided by contagionist theories, more emphasis was placed on isolation and the prohibition of flight from infected areas. Procedures were codified and the execution of regulations put in the hands of various forms of health police with dictatorial powers. But, in Biraben's view, it was not until the development in the seventeenth century of the international exchange of information on the movement of infection and the establishment of a regular system of maritime bills of health on international shipping routes backed by effective quarantine regulations that plague really began to retreat in western Europe.

For Biraben, the proof of the efficacy of rigorous government action in banning plague lay in the manner of its ending in the Turkish empire. In 1841, for whatever reasons, the Porte decide to begin to apply vigorously and severely throughout the length and breadth of the Turkish empire anti-plague regulations of the kind elaborated in western Europe over the preceding three centuries. Within twelve months plague was virtually eliminated from its last major reservoir in the European and Mediterranean world. After 1842 plague made only brief appearances in the Turkish sphere of influence which were also quickly extinguished.

It is worth enquiring about the links between Biraben's belief in the efficacy of controls on the movement of humans and goods in eliminating plague from Europe and his acceptance, examined in the first part of this article, of the etiological role of the human flea. If infection could be passed from human to human by means of *P. irritans*, humans could spread the disease as fast as they could move. Restrictions on human movement by quarantines and *cordons sanitaires* could, therefore, impose a check to at least one chain of infection. Two other chains remained, however. First, rats and other rodents transmitted infection with their fleas, and neither quarantines nor *cordons sanitaires* were barriers to their movements. Movements of rodents, however, were normally extremely localised, and river and mountain barriers might well have been effective in restricting long-distance movement except in the cases, no doubt frequent, of the use by rats of human transport — ships, carts, and even saddles. There is little doubt that in the early stages of the pandemic this form of rodent movement played an important role in the diffusion of infection. Later in the

pandemic, the vigorous local actions described above probably did much to confine new outbreaks initiated in this way. Many of the later outbreaks of plague, particularly those on the Austrian side of the *Militärgrenze* in the later eighteenth century, seem to have fallen into this category of episodes relatively easily contained without the enormous casualties of earlier times.[28] The remaining chain of infection was through the movement of goods which might harbour infected rat or human fleas. The quarantines and *cordons sanitaires* were probably quite good at breaking this chain. The Austrian *cordon sanitaire*, as we have seen, was particularly comprehensive in its disinfestation procedures.

In short, the acceptance of the possibility that *P. irritans* as well as *X. cheopis* was a vector of *P. pestis* carries with it the corollary that infection chains would continue to operate so long as human movement out of infected zones was permitted. If *X. cheopis* were the sole vector, checks on human movement would have been of far less relevance. The worrying thing about Shrewsbury's etiology was that it made the sacrifice of the people of Eyam unnecessary:[29] without a human-to-human infection link, the migration of the villagers out of Eyam could not have spread infection and might have removed some people from the probability of infection via *X. cheopis* in the village. If, on the other hand, it was possible for Eyam villagers to carry infection elsewhere with their own *P. irritans*, then the Rev. Mompesson's action in persuading the villagers to stay where they were was sound as well as heroic. And if the villagers did not move, nor probably would their infected goods.

It is worth noting that, apart from the use of modern chemicals and prophylaxis, the measures suggested by both Pollitzer and Hirst for the control of plague in the twentieth century involve fundamentally the same kind of controls of movement of humans and goods as was gradually built up in Europe between the sixteenth and nineteenth centuries.[30]

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Many questions about the rise and fall of the plague remain to be answered, but the studies reviewed in this article, above all Biraben's massive contribution, have gone a long way in the last ten years towards indicating the lines along which answers must be sought. There is room still for closer collaboration between bacteriologists, epidemiologists and historians. The suggestive work

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[28] These are described, for example, in G. STICKER, *Abhandlungen aus der Seuchengeschichte und Seuchenlehre* (Giessen, 1910), I, 327-8.

[29] A recent study of the Eyam outbreak shown that the mortality there, while heavy, was not so catastrophic as has hitherto been thought. (L. BRADLEY, "The most famous of all English plagues: a detalied analysis of the plague at Eyam, 1665-6," in *The Plague Reconsidered* (Local Population Studies, Matlock, 1977).

[30] HIRST, *op. cit.*, pp. 378-439; POLLITZER, *op. cit.*, pp. 611-13.

on communal anti-plague measures in France and Switzerland by Biraben, Mattmüller and their colleagues must clearly be extended, above all to Germany, Poland and Russia. The beginnings, which western historians are at last making, of original work on Turkish sources, must be exploited and, with Turkish collaboration, extended to embrace the kind of social, medical and demographic areas involved in the study of plague history. Above all, the international dimensions of this field of history must be recognised and retained in all future study: many of the limitations of earlier work in this field have persisted to a considerable extent because of the unwillingness or inability of historians to familiarise themselves with parallel work in countries other than their own.