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Abstract

According to written sources, two memorable outbreaks of ‘plague’ occurred in Iceland in1402-4 and 1494-95. Here we argue that these were episodes of pneumonic plague, caused by Yersinia pestis, and that the likely mortality was no more than 25% of the population in both cases. This contrasts with the higher rates (50-60% and 30-50%) postulated elsewhere. Although it is recognised there are alternative explanations for plague in Iceland, greater caution needs to be taken in interpreting the direct and indirect evidence for its demographic effects. A lower mortality rate fits better with a less widespread and more fragmented epidemic. The numbers and types of Icelandic farms which might have been vacant during the fifteenth and early sixteenth centuries are given more detailed consideration than in previous accounts. ‘Farm abandonment’ in the fifteenth century was continually driven by of a series of environmental and economic factors and need not be interpreted as a demographic collapse caused solely by the plague. Greater attention is also given to undersanding how plague could have reached Iceland and the biological, ecological and sociological factors which might then have sustained it.

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Introduction

Icelandic annals record two severe plague epidemics for 1402-4 and 1494-95. This mortality, which was sufficient to merit special attention from the annalists, appears to have been of the same order as that reported elsewhere for the 'second plague pandemic' which spread through most of Europe from 1346 onwards. Some of the evidence for Iceland has been taken to suggest mortality rates of over 50%.[1]

Both Ole Benedictow and Gunnar Karlsson have summarised the information that can be gleaned from the documentary sources and work by Icelandic scholars writing in Icelandic.[2] It seems probable that annals were written shortly after the actual events took place but they are preserved in manuscripts which date from the fifteenth century onwards. The first epidemic appears to have started in the autumn of 1402 and that the disease had arrived by ship, possibly in the harbour of Hvalfjörður on the south west coast. The epidemic is generally considered to have spread to the north of Iceland before Christmas and continued until Easter 1404. The annals refer to a 'plague winter', 'plague year' and 'later autumn plague'.[3] For the second epidemic there is no contemporaneous annal but the four surviving accounts from the sixteenth and seventeenth centuries give the same general picture of it. A significant plague started in the south-west and spread everywhere except for Vestfirðir, the north-western peninsula. The epidemic seems to have been active in the north and north-west of the country in the winter of 1494-95.[4]

The nature of these epidemics is puzzling. Benedictow, in line with many scholars, has argued that they were primarily rat-based bubonic plagues, the aetiological agent being the bacterium *Yersinia pestis*. He rejected pneumonic plague as an explanation because it is not easily transmitted and those that are infected die on average within two to three days of the onset of symptoms, having little opportunity to pass the disease on.[5] Gunnar Karlsson, however, has maintained that there was no evidence that the Black Rat (*Rattus rattus*) had ever established itself in Iceland. Thus, because

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[1] Gunnar Karlsson, 'Plague without rats: the case of fifteenth-century Iceland', *Journal of Medieval History* 22, no. 3 (1996), 263–84; Gunnar Karlsson and Helgi Skúli Kjartansson, ''Plágurnar miklu á Íslandi', *Saga* 32 (1994), 11–74. Reference is made to the earlier Icelandic-language article when necessary to cite its more detailed discussion. Some of the criticisms of these articles were also made by Jón Ólafur Ísberg, 'Sóttir og samfélag', *Saga* 34 (1996), 177–216. His points, particularly on the weaknesses of the written evidence in relation to the early fifteenth century, were elaborated on in idem, 'Annálar og heimildir um Svarta dauða', *Ritmennt* 2 (1997), 55-75. The scientific evidence for the case made by Jón Ólafur Ísberg has since grown and this article adds to his arguments about the nature of the textual evidence. The following abbreviation is used in this paper: DI: *Diplomatarium Islandicum. Íslenzkt Fornbréfasafn* I–XVI (Copenhagen, 1857–1972).

[2] Gunnar Karlsson, 'Plague without rats', 263–5; O. J. Benedictow, *Plague in the late medieval Nordic countries. Epidemiological studies* (Oslo: Middelalderforlaget, 1992).

[3] Ibid., 267. Karlsson appears to translate 'mandeyda haustit seinnara', DI, vol. 3, 739, as 'posterior plague autumn' but the translation here is preferred.

[4] Gunnar Karlsson, 'Plague without rats', 267.

[5] O. Benedictow, *Plague in the Late Medieval Nordic Countries: Epidemiological Studies* (Oslo: Middelalderforlaget, 1996), 221.

the epidemics persisted throughout the winter months when fleas are unlikely to operate as vectors, the outbreaks were most likely the pneumonic rather than the bubonic form of this disease. Although Karlsson[1] has pointed out that the mortality rates implied by the annals might have been exaggerated, and that other diseases such as typhus fever might also have been prevalent,[6] he sees evidence of unoccupied farms in the fifteenth century as supporting the idea of high mortality rates. We argue, however, that empty farms are continually caused by a number of economic and environmental factors as well as intermittent outbreaks of disease. This phenomenon can be explained, therefore, by a series of processes rather than simply the result of outbreaks of plague.

Difficulties with the reconciliation of historical and modern plagues have led some authors to argue that the first and second plague pandemics were not caused by *Y. pestis* but by a completely different organism, as yet unidentified, whilst others have suggested anthrax and typhus as possible causes.[7] Nevertheless, this paper argues that, despite Iceland's apparent isolation and the delay in the 'Black Death' reaching it, a strong case can still be made for identifying *Y. pestis* as the disease organism which ravaged Iceland on the two occasions mentioned above. However, the way in which the disease epidemic progressed is likely to have been more complex than has been previously understood and it needs to be recognised that aspects of human social behaviour in a rural community – medieval Iceland was entirely rural – served to facilitate the spread of the disease, particularly in its pneumonic form. Such a view is based on an understanding of how modern plague epidemics progress and a reassessment of the significance of the written evidence for the plague in Iceland.

Descriptions of the plague in Iceland

Before the details of mortality are discussed it is necessary to explain just why debates about the Black Death in Iceland have been so heated: more so than for other regions, there is little detail on the actual symptoms of the disease which is assumed to have been the bubonic or pneumonic plague. Harsh words have been exchanged between Benedictow and other scholars on the way a single account of the symptoms has been described by the single Icelandic annal entry which describes it. That entry, an account in *Lögmannsannáll* for 1349, describes the symptoms of the disease that was ravaging the rest of the world. The disease killed people within two or three days and was accompanied by 'hörðum stinga' (hard/sharp pains) and the vomiting of blood.[8] The ambiguity here concerns the meaning of the Old Norse-Icelandic word *stingi*, variously interpreted as being an internal pain such as might be associated with pleurisy, or else the kind of glandular swellings (bubos) which are often associated with bubonic plague. It is extremely hard to tell which is intended and is possibly not relevant to the Icelandic situation of 1402 or later. Most likely, the phrase shows awareness on the part of the author that victims suffered severe, focussed pain in

[6] J.F.D. Shrewsbury, *A History of Bubonic Plague in the British Isles* (Cambridge: Cambridge University Press, 1971) suggested something similar for the fourteenth-century epidemics in the British Isles.
[7] S. K. Cohn, 'The Black Death: end of a paradigm', *American Historical Review* 107 (2002), 703–38 and C.J. Duncan and S. Scott, 'What caused the Black Death?', *Postgraduate Medical Journal* 81 (2005), 315–20 for scepticism; G. Twigg, *The Black Death: A Biological Reappraisal* (London: Batsford, 1984) suggested anthrax; Shrewsbury, *A History of Bubonic Plague in the British isles*, 142 suggested that typhus could account for some episodes routinely attributed to *Y. pestis*.
[8] Gunnar Karlsson, 'Plague without rats', 281-2; *Islandske Annaler*, 275-6.

particular parts of their body. It seems unsafe to speculate as to the author's meaning beyond this rather than accept Gunnar Karlsson's suggested analogy with the Modern Icelandic usage of the word noun *stingur* for a sharp chest pain.

Some accounts of the 1494-5 outbreak of what is called 'the great disease' (*mikla sótt*) say that it might have spread from some blue cloth or clothes (*hafi komið upp úr bláu klæði*). This speculation comes only for this outbreak in what is generally a rather tentative account and is followed by a sentence saying that the disease had a physical form, resembling a bird and, in one account, giving off smoke.[9] Whether the story only reflects folk beliefs about disease in general, or derives from real knowledge of the source of the disease is unclear but it may be significant that there is a connection with shipments of clothing as we shall see.

With this rather limited sense of what the two plagues' symptoms and causes were, the scale of deaths and the more extended narration of the diseases' course make them distinct from other recorded epidemics, as discussed below.[10]

The course of the epidemics

The picture we get of the arrival and spread of the 1402-4 plague is vague at times but generally coherent, as Gunnar Karlsson made clear. However, the annal accounts of the arrival of the disease have a sparseness and simplicity to them reminiscent of myths. Some historians have chosen to read them as providing a straightforward sense of the spread of the disease from the south-west of the country out to other regions but really each annal has its own interests. What Gunnar Karlsson identifies as 'the fragment' (*Vestfjarðarannáll*) has an interest in the geography of the disease's spread, while the other account, *New Annal*, has almost no detail on the movement of the disease. The former identifies Hvalfjörður in the south-west as *the* point of entry of the disease but the latter provides no precise origin.[11] The former also records that the cleric Áli Svarthöfði and his servants died *en route* back to the south from Hvalfjörður, while the latter merely says that he was the first cleric to die in the autumn of 1402.[12] *New Annal* is also short on detail for the north of Iceland and far more concerned with the southern of Iceland's two sees, Skálholt, hence its recording of the deaths of priests at Skálholt and then of clergy in the south, west and east of the country but not in the north. It is also noteworthy that it says nothing about widespread depopulation or its consequences, such as a reduction of rents that landowners might have had to suffer as their tenants died or disappeared.[13]

The evidence for the north of Iceland is in a different form and unambiguously contemporary. For the north-east of Iceland two documents record vows of atonement which were taken at two of the most significant religious centres, Grenjaðarstaður in Þingeyjarsýsla and Munkaþverá in Eyjafjörður, written in December 1402 and January 1403 respectively. The former document, from the more north-easterly location, records that the plague had travelled eastwards (*for vestan epter landinv*) and

[9] Benedictow, 'Plague', 212; Annaler 1400-1800, vol. 1, 27-8; *Safn til sögu Íslands*, vol. 1 (Copenhagen, 1856), 43-4.
[10] See below, p.10.
[11] Jón Ólafur Ísberg, 'Sóttir', 194, accepts this for all his scepticism about the value of the annals.
[12] Cf. Gunnar Karlsson, 'Plague without rats', 265–67.
[13] Jón Ólafur Ísberg, 'Sóttir', 187.

affected the south of Iceland, and the northern regions of Húnavatnssýsla and Skagafjörður, before it arrived in the north-east. Thus it appears not to have affected either the large, relatively densely populated valley of Eyjafjörður or the sparsely-populated north-eastern corner of the country before this point. The disease seems to have persisted for over a year, only being remembered as disappearing after Easter 1404.[14] As will become clear, it is frustrating that annals do not give a clearer picture of the first epidemic's spread in the north when other evidence exists for possible mortality rates there.

For the 1494-95 outbreak, despite its being more recent in time, there are almost no contemporary or near-contemporary sources for its perceived cause or course. Almost all that can be safely said about this outbreak is that it did happen in the 1490s. Authors of annals written in the seventeenth century give various dates for its arrival ranging from 1492 to 1495 and all give the impression that it lasted for no more than a year but at least one will fixes it to no later than 1495.[15] It is said to have reached Iceland via a port in the south-west, much as for the first outbreak. Four different points of entry for the disease are given, however, all of them equally credible (Vestmannaeyjar, Hafnarfjörður, Seltjarnarnes and Hvalfjörður), in much the same way as medieval English writers had disagreed over the plague's point of entry to England in 1348.[16] This time a source says that one region of the country was spared, the West Fjords peninsular, home to perhaps 12% of the population.[17]

Mortality

Mortality rates are notoriously difficult to calculate on the basis of the kinds of anecdotes and random samples of general economic data which are typically used for other regions affected by the Black Death. For Iceland it is much the same but it has been tempting for some historians to place too much confidence on the written sources' veracity without considering their form and origins. For example, one might choose to stress that mortality rates were low on the basis that several key figures in the stories about the plague's arrival survive, figures like Einarr Herjólfsson himself, for instance, and other members of the elite.[18] However, much more has been made of other statements or apparent evidence of high mortality rates. These latter anecdotes were heavily shaped by oral transmission, a point that will be made for all of the stories commonly used to indicate mortality rates. The mixture of brief statements and then outline narratives about particular mortalities should not fill us with any great confidence beyond accepting the general impression that mortality was remembered as shockingly high.

Good illustrations with which to begin are the stories of Jón Egilsson (1548-1636?), compiler of the Bishops' Annals. Jón Egilsson clearly had a genuine interest in the past but he was keen to retell the few stories which came his way without doing much

[14] Gunnar Karlsson, 'Plague without rats', 266-7; DI, vol. 3, 680, 682.
[15] Gunnar Karlsson, 'Plague without rats', 267; Gunnar Karlsson and Helgi Skúli Kjartansson, 'Plágurnar miklu', 18–19.
[16] *The Black Death*, ed. R. Horrox (Manchester, 1994), 10.
[17] Gunnar Karlsson and Helgi Skúli Kjartansson, 'Plágurnar miklu', 28. Based on earlier and later data which, respectively, suggest 9% and 15% of the population lived here.
[18] Jón Ólafur Ísberg, 'Sóttir', 199.

in the way of his own research. Other scholars have noted this propensity in his account of the history of the Icelandic church in the 1520s, for example, again a time before he was born and where he does not seem to have had access to anything other than hearsay.[19]

The first of Jón's plague stories Gunnar Karlsson cites as 'a moving picture of the mortality in the second plague in the central South, where he [Jón] was brought up and later served as a priest'. Jón was writing his account in 1605 which needs to be given in full to appreciate its anecdotal nature:

> In this plague the mortality was so great, that no-one remembered or had heard of anything like it, because so many farms were devastated, and on most farms only three or two survived, sometimes children, usually two or mostly three, and some of them yearlings, and some suckling their dead mothers. Of these I saw one, who was called Tungufell's-Manga, she was three winters here in Galtafell, she died when I was thirty years old. Another I saw was called Halldóra, she was the mother of *síra* Grímr, who lived at Hruni, she had been one winter old in that plague. She died when I was 34. Where there had been nine children, two or three were left alive.[20]

In this first story we are given what appears to have been the received wisdom about what had happened but Jón does not even go so far as to claim that the elderly women identified here were his informants. In the second story a man who was 'fourteen years in that plague', so seemingly born in 1480, supposedly told Jón, born in 1548 and writing in 1605, that there were four farms, two on each side of the river Hvítá, where the plague 'might not have reached'. The account uses the subjunctive mood three times in setting out what Jón was told, indicating that even Jón seems uncertain about what he was recording including even the location of his informant's childhood home. The geographical distribution, size and status of these farms also follow a pattern which might have been real or else shaped by the memory of the informant or interviewer.[21] We have no idea when Jón spoke to his witness, who would have been 68 years old when Jón was born. It is difficult to imagine at what time this conversation might have happened such that the informant had a clear enough memory of what had happened such a long time before, Jón was old enough to appreciate what he was being told, and then that Jón could remember what he was told clearly to write it down in 1605. We might postulate that, at best, perhaps, Jón

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[19] e.g. M. Kalinke, *The Book of Reykjahólar. The Last of the Great Medieval Legendaries* (Toronto: University of Toronto Press, 1996), 25–26.

[20] Gunnar Karlsson and Helgi Skúli Kjartansson, 'Plágurnar miklu', 25–26; *Safn til sögu Íslands*, vol. 1 (Copenhagen, 1856), 43.

[21] Árni Magnússon and Páll Vídalín, *Jarðabók*, vol. 2 (Copenhagen 1925–7), 258, 272, 326–27, 357. Each farm is from a different district on either side of the river Hvítá. If later data is anything to go by, each farm was slightly below the average size for this region and the three other than the former home of the informant, were arguably on the real geographical map and the informant's mental map of the region. Kaldárhöfði and Efri-Þórisstaðir (modern Þóroddsstaðir in Grímsnes) were most likely independent farms in the possession of the see of Skálholt and both to the west of the river, Kaldárhöfði being on the southern shore of the lake Þingvallavatn. Ás and Hamarsholt were both on the eastern side of the river and Hamarsholt was the last settled farm as one headed towards the uninhabited interior. Both Ás and Hamarsholt were dependent properties owned by a church (Tungufell and Hruni respectively).

had interviewed his informant in 1568, when they were 20 and 88 respectively, and Jón had written down what he was told immediately rather than at some later time by which his memory of what he had been told had weakened.

These accounts certainly show how carefully Jón intended to record the stories of his elderly informants about the plague of 1494-95 but they are far from convincing as accurate accounts of mortality. Jón was in essence getting oral accounts from two generations before his own, those who had been children during the plague but who survived and were able to retell the stories to Jón in their old age. While we can certainly conclude that mortality rates were remembered as being high, nothing much else can be inferred. The first story almost certainly recounts what had become a cliché in popular memory, namely that no more than 'two or three' people/children survived on each farm. The second story simply lacks any context: we do not know what question prompted the informant to name four farms unaffected by the plague, one of which was their own home. Gunnar Karlsson infers from the latter story that it was 'exceptional' for farms to have escaped the 1494-95 plague entirely. He suggests on the basis of this account, which relates to a district of about 120 farms, that 'not more than 10% of all farms in localities where the plague raged could have escaped the epidemic.' What seems more significant is that it was exceptional for a member of the clergy in the later sixteenth century to find anyone old enough to talk about the plague and provide any kind of detail about it.

Most other annal entries giving numbers of deaths in the 1402-4 epidemic follow the same kinds of patterns as those provided by Jón Egilsson, even if they are not quite so obviously anecdotal. From *New Annal* the first one is perhaps the least certain but its account that only the bishop and two lay servants remained at the see of Skálholt is cited as credible. However, in this case it is not even clear that the people who had left had actually died or whether the occupants of the emptied or abandoned (*aleyddi*) estate had simply left to avoid the plague.[22] It does say that every priest remaining there died but one might expect priests to stay longer than lay servants.[23]

Other accounts for the 1402-4 outbreak in *New Annal* include stories where the numbers of living and dead appear to be of the kind which get retold in myths, folktales or oral stories. For example, there are two competing stories for the same year's entry at the monastery of Þykkvibær. In one story, the abbot and six brothers died while another six survived. In the second story, only two brethren and one servant survive after Þykkvibær had been emptied of people a third time. It is not clear which of these stories is correct but they do conform to patterns we see elsewhere in these accounts. For the nunnery at Kirkjubær it is said that the abbess and seven others died while six survived, making it look like a variation on the first Þykkvibær story. Other statements in the sources that only six priests survived in the diocese of Hólar and 'barely 50' in the southern see might also be the product of tales passed down and exaggerated rather than on first-hand knowledge. If we assume that the Icelandic church was operating efficiently, and had a full complement of priests, then with only 56 survivors over 300 had died.[24] There seems little reason to be

[22] Gunnar Karlsson and Helgi Skúli Kjartansson, 'Plágurnar miklu', 25
[23] *Annálar 1400–1800*, vol. 1 (Reykjavík, 1922-7), 10.
[24] Cf Gunnar Karlsson and Helgi Skúli Kjartansson, 'Plágurnar miklu', 15–16, 23. An eighteenth-century annal recalls a story for the later outbreak in which only a priest and a girl survive in a region

certain how many priests typically operated at any one time or about the reality behind these stories of priests' deaths although Gunnar Karlsson suggests 430 in total. The figure for the south has more of a ring of truth about it than yet another story of a 'lucky six or seven', but we might perhaps expect numbers of active priests to decline in an epidemic as (a) some were more regularly exposed to disease when blessing the dying and (b) others chose to abandon their calling to avoid exposure to disease. Either way, these are rather bland statements shaped heavily by oral tradition which when retelling numbers is unlikely to have preserved meaningful ones on which to base estimates of mortality. As Gunnar Karlsson has noted, a story of 795 people being brought for burial in *New Annal* for 1403 is not credible.[25] It would have been hard to find 795 people anywhere near isolated Kirkjubær at any point in Iceland's history so this figure looks like a complete exaggeration or scribal error.

Alongside the issue of numbers of dead, one might also question the claim that the West Fjords escaped the 1494-95 epidemic; both *Gottskálk's Annal* and the seventeenth-century *Skarðsárannáll* record this. As we shall see, records for the West Fjords for the 1440s suggest that there were almost no vacant farms in this region. If we can make anything of these accounts of the distinctiveness of the West Fjords then it seems equally likely that the authors of annals, Gottskálk Jónsson (1524-90) and Björn Jónsson (1574-1655) had confused the anecdotes they had been told about the two outbreaks.[26] This is a better reading of the evidence than to suggest that the West Fjords was so disconnected from the rest of Iceland that it should have somehow escaped the disease twice.

Farm abandonment

Beyond the annals, the other obvious texts that might relate to Iceland's demography in the fifteenth century are surveys of church properties and a handful of other documents which record property transactions including one list of the properties of a significant secular landowner. For both epidemics there are documents which relate largely to the north of Iceland and properties owned by the church.

Gunnar Karlsson dedicates considerable space to analysing these texts.[27] His analysis of these documents seems sometimes to make a number of assumptions which our analysis questions:

1. that the Icelandic farming landscape was in excellent health in 1402 when the first epidemic arrived with all known farms being occupied
2. that farms in Iceland were of relatively uniform quality, size and had the same numbers of inhabitants
3. that all signs of under-utilisation recorded in later decades were due solely to the 1402 and 1494-5 plague epidemics

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of eastern Iceland. They travel south to find seven people in one place and eleven people elsewhere. These motifs are barely different to those in stories from earlier centuries.

[25] Gunnar Karlsson and Helgi Skúli Kjartansson, 'Plágurnar miklu', 27. The figure might only be 675 but it still not credible.

[26] See further, Jón Ólafur Ísberg, 'Sóttir', 188.

[27] Ibid., 'Plágurnar miklu', 35–47; Gunnar Karlsson, 'Plague without rats', 271–73.

If these assumptions are put to one side then a more nuanced picture of the farming landscape appears in the fifteenth and early sixteenth century such has been attempted for Norway recently.[28] We will now look at the texts he considers as evidence for the scale of the demographic impact in the same order as he does.

The first document is a record of a visitation of the bishop of Hólar, Iceland's northern diocese, to his churches, seemingly first in 1429 and then again in 1431-2.[29] It is clearly a document(s) by which the bishop is trying to assess the wealth and assets of his see, just as Icelandic bishops had done periodically before him. Gunnar Karlsson makes the point that this kind of survey is inconsistent but the entries for most churches within it are generally shorter and patchier than any similar survey before it, rarely recording all the things one would otherwise expect for each church (its properties, ownership of rights to driftage on the coast (*reki*), church contents, and the duties of priests).[30] It only records the number of farms or number of empty farms for some parishes. This is why Gunnar produces both a maximal level of deserted farms of 35%, and minimal figure of 19%, dependent on whether or not one includes those parishes where total numbers of farms are given but the number of vacant farms is not. Certainly our preference would be to consider only those nine parishes where vacant farms are actually recorded, i.e. those parishes which suggest that 35% of farms appear to have been vacant in 1431-2.

The most important point is that this list contains *only* 17 churches, a fraction of all the churches in the diocese. The previous comprehensive survey of churches in the diocese in 1394 had recorded 81 churches. Almost all of those churches continued to exist throughout the fifteenth century and beyond. Not all of them were necessarily parish churches, but nonetheless only about half the diocese is covered by the surviving parts of the 1429-32 survey. Indeed there are gaps in the manuscript where it might have been intended for further accounts to have been included. Thus the large and relatively densely populated valley of Eyjafjörður is absent and so is the much larger but sparsely populated region of Þingeyjarsýsla. The absence of these parts of the survey fits with the idea that there was no longer a need to monitor every church's income because most, if not all, farms were occupied and paying tithe just as they had been doing since 1394 and before. It could be that there were significant problems in certain parishes where church attendance and tithe income must have seemed low but,on the other hand, the church may have recognised that for most parishes, the many tens of them surveyed for which no empty farms were recorded, there was either no real need to carry out further surveys or else no need to transcribe the results of those surveys in 1431-2. It is impossible to say which of the latter possibilities was the case but either situation is probably indicative of contemporaries' perceptions that life was going on as usual in most parishes.

If we turn now to the detail of the surviving Hólar diocese, then we can see that there are certain districts with high numbers of vacant farms and that there are some small clusters of adjacent or near adjacent parishes.

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[28] A. Dybdahl, 'Climate and demographic crises in Norway in medieval and early modern times', *The Holocene* 22 (2012), 1159–67.
[29] DI, vol. 4, 464–68; 510–14. Gunnar Karlsson and Helgi Skúli Kjartansson, 'Plágurnar miklu', 35–8.
[30] Even the drawn-out and incomplete survey of Hólar's churches carried out by Bishop Jón skalli in the latter half of the fourteenth century gave a full account of each church according to conventional practice. DI, vol. 3, 155–79. Thank you to Erika Sigurdson for pointing this out.

Table 1
Deserted farms recorded in the visitations of the bishop of Hólar, 1431-2[31]

Parish	**Total no. Farms**	**Occupied**	**Vacant**
Urðir, Svarfaðardalur	17	7	10
Ufsir, Svarfaðardalur	7	4	3
Árskógur, Árskógarströnd	16	11	5
Þóroddsstaður, Reykjadalur	14	11	3
Helgastaðir, Reykjadalur	7	4	3
Auðkúla, Húnavatnssýsla	9	7	2
Breiðabólstaður, Vatnsdalur	14	8	6
Másstaðir, Vatnsdalur	5	4	1
Spákonufell, Skagaströnd	8	7	1
Totals	**97**	**63**	**34**

The first three parishes listed here were close to one another, on the western, outer part of outer Eyjafjörður while the list features two adjacent parishes in Reykjadalur and two at the mouth of Vatnsdalur. The remaining two are outliers. What best explains this high level of vacant farms and their particular distribution on the map? Certainly there is no particular sign that these areas were especially prone to periodic farm abandonment in the way that has often been identified for some parts of Iceland which are typically further inland and at relatively high altitudes. Equally, however, none of these districts stand out as being particularly wealthy or densely-populated. The worst affected parish here, Urðir in upper Svarfaðardalur, was an area where in later centuries smaller, dependent farms were periodically abandoned. There were 13 such unoccupied farms among 83 farms in the survey of the valley undertaken in 1712 (15% of households; for the survey see below), which, is higher than average but not by much – 39 other farms in the *sýsla* of Eyjafjörður were abandoned in 1712 (11% of all remaining farms) – but this is nowhere near what we see in the table. Considering this valley in isolation, then, it could be argued that fluctuations in Iceland's climate might have had an impact on farms' viability and led to such vacancies. More likely, however, what we are witnessing in Svarfaðardalur and nearby is the result of a combination of various influences on demography and decision-making by landlords and tentants. It is hard to quantify the relative quality or merits of the Icelandic landscape, but it is possible that Svarfaðardalur was more prone to settlement change than many of the other areas listed.[32] The 1402-03 epidemic could still have had residual influence on the vacany of farms in these districts in the 1440s but it would hardly have been the only one.

[31] Total no. of farms includes the farm on which the church was located. Locations of parishes
[32] One might also argue that the record of local historical traditions, *Svarfdœla saga*, reflects this: it is a text far less well preserved and far less coherent than sagas recording other districts' histories. See, for example, R. Boyer, 'Svarfdœla saga' in *Medieval Scandinavia. An Encyclopedia*, ed. P. Pulsiano and K. Wolf (New York: Garland Publishing Inc., 1993), 626–7.

The lists of properties in the 1440s actually name the individual farms owned by the monasteries and one secular owner who owned them. For these farms it has been argued that they were either vacant, recently reoccupied or else vacant but utilised by other farms. This allows us to consider the wealth of those individual farms. The best and arguably only way to do that is to use a tax register which was compiled in 1702-14. This document, the *Jarðabók* compiled by the Danish royal officials Árni Magnússon and Páll Vídalín, gives a snapshot of Iceland. It is commonly used by historians of Iceland to consider the economic history of the country for the period before 1702-14. It is generally considered valid to do so because all the indications are that settlement patterns, farming practices and technology remained largely unchanged from the first time they were recorded in the Middle Ages right through to the eve of the twentieth century. There was, for instance, a great deal of continuity in the way farms' values were measured – in an abstract unit called a hundred (*hundrað*, abbreviated to 'h' below) – with the majority of farms having relatively stable values over the centuries. Rents were similarily expressed in a unit – the 'ell'- derived from a measurment of length often applied to cloth. The vast majority of farms mentioned in medieval written sources have survived into the modern era with most being continuously occupied in those nine hundred years.[33]

The *Jarðabók* of Árni and Páll followed several seventeenth-century royal surveys but has a special significance because it records far more forms of data than the previous ones. It contains a vast array of subjective and numerical data about each farm, giving an opinion on the quality of types of natural resources, as well as pasture, rents and numbers of livestock. For the sake of simplicity the following analysis will use the fuller 1702-14 figures for the two forms of rent which Icelandic farm tenants paid, the land rent (*landskuld*), paid in cash, and what we can translate as cattle-hire (*leigukúgildi*), a standard part of the rent for a tenant in Iceland, usually paid in kind, which involved actually renting adult cows from the landowner. Changes in rents are often discussed in the 1702-14 register as well so that some entries comment on whether a rent had gone up or down within living memory or, for later entries, how the smallpox outbreak of 1707 had affected rents.

Last, but not least, it is important to note that *Jarðabók*, no less than many other sources, demonstrates the variability of the size and value of Icelandic farms. The largest farms were accorded a value of well over 100 h(undreds), and typically the largest farm in a parish would be the farm with the parish church with a value of 60h or more. The smallest farms were typically 6h or 10h. An average farm might have been rated at 20h or 24h and had a rent of 120 ells. We need to bear in mind this varied tenurial and economic landscape when we try to explain the impact of epidemics in Iceland. This was not really possible for the possessions of the see of Hólar but it is for the remaining survey documents. As we shall see, it was often the case that farms that were vacant in the fifteenth or sixteenth century often had low values in *Jarðabók*. There is a risk of circularity of argument here, that a low value for a farm in *Jarðabók* was the result of earlier desertion, but farm values were generally stable as Gunnar Karlsson has noted.[34]

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[33] Orri Vésteinsson, 'A Divided Society: Peasants and Aristocracy in Medieval Iceland.' *Viking and Medieval Scandinavia* 3 (2007), 117–39 highlights the selectivity of some of those medieval sources, however.

[34] In addition it should be noted that relative values between some regions and districts were stable even immediately after the first outbreak. Two exchanges of land in Svarfaðardalur in 1409 and 1414

The second document then, dates to 1446 and is a list of the confiscated property of the outlawed landowner Guðmundur Arason, including his 180 farms, all in the West Fjords. Of these 180, a mere three were definitely vacant, all in one small district (Furufjörður, Reykjarfjörður and Skjaldarbjarnarvík) while two others in another district are listed as if they are not occupied but are being utilised by another farm. Four of these five properties were small, with only one being rated at over 12h in 1710 which, ironically, was the only one of the five which had been abandoned in the smallpox epidemic.[35] It could have been that Guðmundur was a very shrewd dealer on the property market and did not own vacant farms but it seems more likely that there were very few vacant farms in the West Fjords at all. In other words, any epidemic that had affected the West Fjords was undetectable by the 1440s. As suggested above it might even have been the case that the first epidemic had had a limited impact on the region.

The third document is a survey of the monastery at Munkaþverá in Eyjafjörður dated to 1446, mostly covering areas not covered by 1431-32 survey of Hólar's property. Gunnar Karlsson highlight the fact that Munkaþverá had 31 properties in its possession in 1446 of which six farms paid no rent and a further seven paid 'abnormally' low rent (listed for all farms only in ells, without reference to the second form of rent, cattle hire). His conclusion is that the document shows that all 13 of these farms were vacant in 1446, supposing that those paying low rents were being rented out as pasture to other farms. All in all this seems to be an unnecessarily gloomy picture and not one which takes into account local geography, *Jarðabók* data or a realistic assessment of what constituted rent on these farms.

Table 2
Farms with low rents in the list of property of the monastery of Munkaþverá in 1446.[36]

Farm (location) * denotes vacant in 1712	**Rent in ells 1446**	**Rent recorded in Jarðabók (ells)**	**Dýrleiki (hundreds) in Jarðabók**

respectively show farms in that valley being exchanged for comparable farms elsewhere in the north. In the first Tungufell, valued at 30h in 1712, is exchanged for a farm of the same value elsewhere in the north (Hvoll in Húnavatnssýsla, 30h in 1705). In the 1414 transaction two farms totalling 76h (Sakka (60h) and Skáldalækur (16h)) were exchanged for three farms in Öxarfjörður worth a combined value of 80 hundreds (Klifshagi (40h), Hafrafellstunga (20h) and Þverá (20h)).

[35] DI, vol. 4, 687. *Jarðabók*, vol. 7, 83–84, 312–14, 321–22 records that in 1710 Furufjörður (24 hundreds, 120/160 ells) had been abandoned since the smallpox outbreak. Reykjarfjörður (12h, 70/60 ells) and Skjaldarbjarnarvík (6h; 90 ells) were occupied. Birnustaðir (6h; 20 ells) and Fjallaskagi (12h; 120 ells) were occupied, paying their rent in fish. Cf. Gunnar Karlsson and Helgi Skúli Kjartansson, 'Plágurnar miklu', 43.

[36] *Jarðabók* notes that Helluvað, Raufarhöfn and Skálpagerði had been abandoned since the smallpox epidemic of 1707, *Jarðabók*, vol., 11. Helluvað's rent had been 120 ells plus a small payment in labour/goods. Brekka's rent was 80 ells but had traditionally been 60 but also sometimes 120 or 90. Ormarslón's rent had been reduced over the course of twenty years from 120 ells down to 60 and then, after the smallpox outbreak, from 60 to 40. Jódísarstaðir and Höskuldsstaðir are identified here as the farms of close to Munkaþverá itself as opposed to farms with those names in Reykjadalur. Of these, Höskuldsstaðir was unusual in being nothing more than ruin within the boundaries of Jódísarstaðir in 1712. Þórðarstaðir's rent was 90 ells but had been 110 twenty years before.

Belgsá, Fnjóskadalur	60	60 ells	12
*Helluvað, Mývatnssveit	60	60/>120	12
Brekka, Presthólahreppur	60	60/80/90/120	8
Blikalón, Presthólahreppur	60	90/120?	15?
Rif, Presthólahreppur	60	60	5
*Raufarhöfn, Presthólahr.	60	60	5
Ormarslón, Þistilfjörður	90	40/60/80/120	8
Jódísarstaðir, Eyjafjörður	0	120/180	30
*Höskuldsstaðir, Eyjafjörður	0	0	0
*Skálpagerði, Eyjafjörður	0	60	10
Fífilsgerði, Eyjafjörður	0	60	10
Þórðarstaðir, Fnjóskadalur	0	90/110	20
Hof, Flateyjardalur	0	60	8

The farms belonging to Munkaþverá were actually a varied bunch. About half of the 31 properties lay in Eyjafjörður itself while the others were in districts to its east, in the small valley of Fnjóskadalur and then dispersed across the north-east of Iceland at locations where specific natural resources were to be found. One farm was in Mývatnssveit, for example, where the salmon fishing was good (Helluvað); other farms were in Presthólarhreppur on the coast and provided fishing, seal-hunting and other resources; Fnjóskadalur itself was a more wooded valley than Eyjafjörður and was also likely to have been home to charcoal production. What is important to note is that it is mostly non-Eyjafjörður farms with specialist economic functions, *and* with low values, which have the unusually low rents in 1446 (see Table 2). These farms were not only small but also witnessed a lot of variability in their rents in the decades up to *Jarðabók*'s compilation for this region (1712), even by the standards of its post-1707 smallpox surveys. This would suggest that these farms were generally a little bit unstable, more prone to having good years and bad years than other farms but it is hardly clear that most of them were significantly worse off than in 1712. It is possible that they had been vacant at some point between 1402 and 1446 but not at all certain that this had been the case.

For obvious reasons a stronger case can be made for the six farms paying no cash rent in 1446 actually being empty at this point. On the other hand there is an equally valid case for most of these farms being occupied. It is notable again that these farms were almost all small while almost all lay within a few kilometres of Munkaþverá in Eyjafjörður itself. They could certainly still have been paying cattle hire payments to Munkaþverá if not *landskuld* and, given their proximity to the monastery and the absence of any earlier statement on the nature of their tenancies, it is not out of the question that they had always had unusual rent arrangements.

The fourth document is the 1446 list of rents of the monastery at Reynistaður, Gunnar Karlsson takes a maximalist approach to the figures, assuming 14 of the monastery's 52 farms were abandoned. While there are unequivocally seven deserted farms among that 52 – the last seven listed in Table 3 – the others might have been vacated at some stage but were occupied in 1446. For these farms we have a 'kúgildi' value attached to them that appears to be the farm's cattle hire rent rather than value. The cattle hire figure for the seven farms with (very) low cash rents are mostly consistent with those found in *Jarðabók*. This would suggest that the farms were still reasonably productive although, again, most farms in this table tend to be average-sized at best. In some

cases it would seem that a moderately-sized farm was getting some kind of rebate but that is less clear for other farms. The two farms which Karlsson identifies as 'let for three years at a rapidly increasing rent from year to year' probably had only recently been reoccupied and where new tenants had been given a low introductory rent as an incentive to move there. Four of the Reynistaður properties which Karlsson identifies as abandoned or recently reoccupied had ceased to exist by c.1700 and two of those are unidentifiable. These would seem to be cases where the farms had ceased to exist beyond the memory of those alive when *Jarðabók* was compiled. It was not impossible that here, as elsewhere, fifteenth-century farms were being let to very small households because they were barely viable.[37]

Table 3
Farms with low rents in the list of property of the monastery of Reynistaður in 1446.[38]

Farm (location) * vacant in 1709 or 1713	**Rent in 1446 (ells)**	**Rent in Jarðabók (ells)**	**Cattle hire in 1446**	**Cattle hire in Jarðabók**	**Dýrleiki (h) in Jarðabók**
Hraun, Skefilsstaðahreppur	30	120	1	3 or 4	20
Malland, Skefilsstaðahr.	30	140	3	3	?
*Selá, Skefilsstaðahr.	30	80	3	3	>7.5
*Brókarlækur, Skefilsstaðahr.	37.5	60	3	3	>10
Hamar, Rípurhreppur	0?	60 or 120	4	3 or 4	20
Hryggir, Staðarhreppur	60 for 3 yrs	-	5	-	-
Daufá, Lýtingsstaðahreppur	60 for 3 yrs	60 or 120	6	2, 3 or 4	20
Gnúpr?	20	-	-	-	-
Stapahóll?	20	-	-	-	-
Instaland, Sauðárhreppur	30	30, 50 or 80	?	3	12
*Fossárteigur, Sauðárhr.	30	40 or 60	?	2	10
Kárastaðir, Rípurhreppur	40	0, 60 or 140	?	2, 3 or 5	24
*Lýsukot, Rípurhreppur	40	?	?	?	?
*Ferjuhamar, Rípurhr.	40	?	?	?	?

The general point to make about all these farms is that they were mostly small and therefore atypical. Almost all of the other farms owned by Munkaþverá and Reynistaður in 1446 were later recorded as having values of 20h or more.[39] Indeed

[37] J. Hajnal, 'The Icelandic Census of 1703 in perspective', in *Manntalið 1703 þrjú hundruð ára. Tilefni afmælis*, eds Eiríkur G. Guðmundsson and Ólöf Garðarsdóttir (Reykjavík: Hagstofa Íslands og Þjóðskjálasafn Íslands), Table 2, p. 35, estimates that 22% of households in1703 as of 1–3 people.
[38] Hamar appears in this list because no rent is listed for it. This may simply be an omission by the scribe. *Jarðabók* again shows some of the local idiosyncracies of rental and ownership. Hryggir formed an integrated part of the main estate of Reynistaður in 1713 and did not have its own rental value. Gnúpr (nvp) and Stapahóll (stapol) are unidentifiable. Innstaland included an island, Instalandssker. 'Kallastader' is identified as modern Kárastaðir. Kárastaðir had been abandoned for 12 years just before *Jarðabók* had been compiled. It is listed just before Ferjuhamar which is the name of a ruin at Kárastaðir. 'Lusabakke' is most likely synonymous with the 'Lijsekot', a place-name remembered as having been a farm at Helluland, next to Kárastaðir. *Jarðabók*, vol. 9, 60–1.
[39] *Jarðabók* vol. 11. For Munkaþverá only the unusual Espihóll syðri (12h), probably originally an off-shoot of its larger neighbour Espihóll nyðri, and Gásir (10h), essentially a seasonal port rather than a farm, had values below 20h.

some of the remote or coastal properties may not have even been whole farms – something suggested by the cartularies from 1525 discussed below – and are unlikely ever to have supported many people as household sizes correlated roughly to a farm's value and its ability to feed its occupants. Such farms, or even parts of farms, were almost certainly going to be the first farms to be vacated in a demographic crisis – or at least exploited remotely rather than being occupied all year – and then to have been last to be reoccupied permanently when the population expanded again. Thus a more realistic reading of the Munkaþverá document would be that those 13 small, 'abandoned' farms were occupied in 1446 but were neither populous nor successful. Reynistaður's properties do have lower rents than those of Munkaþverá, and seven are clearly unoccupied. These low rents probably best reflect a general lowering of property rents in response to a drop in the population. They represent a kind of renegotiation between landlords and peasants as we see elsewhere in western Europe in the fourteenth century. This is still a poor guide to actual population decreases, however.

The fifth document in Gunnar Karlsson's list is the rental for the monastery of Möðruvellir in Hörgárdalur, also within the wider valley system of Eyjafjörður, dated to 1447. This one potentially provides the strongest case for a demographic crisis for early fifteenth century Iceland. Thirty-nine of its 74 properties were uninhabited leaving just 35 occupied. In this document there is absolute clarity that the occupied farms are paying both cash rent and cattle hire, many of them at levels higher than those recorded in *Jarðabók*, and that these are average to large farms of 20h to 40h with only one or two exceptions. In other words, this section of the monastery's property is in very good health. For the unoccupied farms, however, the picture is generally as it is for the other monastic surveys – farms are generally of 20h or less and some are hard to identify at all in other sources – although it is noteworthy that two vacant farms were apparently larger ones, of 50h and 60h. Otherwise twelve of the 39 are accounted for by a single item, the whole of the remote island of Grímsey, which was divided into twelve properties most of which were later valued at 10h-17h each: these appear to have been small properties which depended heavily on fishing. In 1713 the whole island's livestock, for the now ten farms and one *hjáleiga*, amounted to a meagre five cattle, 67 ewes and a few lambs.[40] Even if Grímsey contained twelve properties it seems unwise to regard them as normal or stable farms, for 1447 or at any time, however good the fishing might have been. Consideration will be given to non-Black Death-related causes of abandonment below, but it is worth noting here that in Grímsey's case, that the island may have been harmed by a raid on the island in 1424 by the English (fishermen) active in Iceland at the time.[41]

Turning to the remaining 27 empty farms on the Möðruvellir list of 1447, 15 have a monetary value listed for them which must be *landskuld* and presumably is a sign of their resources being used in some way, but not of permanent occupation; three of these are listed slightly differently and might actually have been occupied.[42] The last

[40] *Jarðabók* vol. 10, 310-20.
[41] Cf. Gunnar Karlsson and Helgi Skúli Kjartansson, 'Plágurnar miklu', 39.
[42] The phrase 'bykt firir' with a value is also provided for three farms of the 15 vacant farms accorded a *landskuld* (Vaglir, Hóll, Garðshorn) in the same way as it is used for both occupied and unoccupied farms in the Reynistaður document. These farms were valued at 20h (Vaglir) and a combined 60h for the other two which later formed part of the same farm. These look like they might have been occupied.

twelve were undoubtedly not productive. To sum up, this document does not necessarily demonstrate devastation of the human landscape. If Grímsey were treated as less significant, and those three other farms were occupied or used, then only about half of Möðruvellir's properties could be counted as empty.

For Möðruvellir in 1447 there was still a very high percentage of its farms which were vacant but two features of these require particular emphasis. The first is that, as for the other rentals above, the headline figure of the number of vacant farms probably overstates what this represents in terms of population and overall productive capacity of the land because on average they are smaller farms. The second point is important and takes us back to the first document under discussion which showed a concentration of empty farms in Svarfaðardalur in 1431-32. What we see in the Möðruvellir document is that the greatest concentration of its vacant properties are also in Svarfaðardalur (at least seven of them), where it owns at most five occupied farms in the valley. This contrasts with its holdings to the south of Svarfaðardalur where it owned over 30 occupied farms (most valued at 30h or more) and only 10 or so empty farms. In other words, the high levels of vacancies only occurred in Svarfaðardalur. Our views of levels of occupancy are thus skewed by what could well have been the high, anomalous figures for that valley.[43]

For the last document, a 1449 list of the properties of the diocese of Hólar, a maximum of 23 of 187 properties were either empty or rented out for low rents.[44] The Hólar properties, both occupied and vacant, were distributed across northern Iceland with a slight concentration of smaller, empty ones again in the north-east coastal districts, just as with those owned by Munkaþverá.[45] It is notable that Hólar's large, dispersed group of properties included no more than 12% of its number showing signs of having been empty.

In sum it would seem that Gunnar Karlsson's calculations for numbers of deserted farms are too high. In terms of the population that the occupied farms might have supported in the 1440s, it would seem most likely that this was higher too with the obvious implication that the residual effects of plague might not have been sustained to such a great extent forty years after the epidemic if, indeed, they remained detectable at all.

For the 1494-95 outbreak there is a similar range of surveys of church properties, this time all dated to 1525, and so still a significant time after the epidemic. These sometimes record individual vacant properties and sometimes do not and cover the same landlords as before but with the addition of other church properties in the north including the monasteries at Þingeyrar (to the west of Skagafjörður) and Grenjaðarstaður (to the east of Eyjafjörður). The same general rules would seem to apply for the 1525 documents as for those of the 1440s. Where there are no records of

[43] Gunnar Karlsson's statement that documents 1–6 cover 716 farms is not quite correct, 'Plague without rats', 273. Some farms are counted twice because of Document 1's overlap with many of the others.

[44] 23 is the number claimed by Gunnar Karlsson and Helgi Skúli Kjartansson, 'Plágurnar miklu', 39–40, of which two might not be seen as independent. One of the farms listed here with a full rent is listed as an empty farm for Möðruvellir in 1447. DI, vol. 5, 35–41.

[45] Hólar owned few properties in Svarfaðardalur so it is not possible to compare occupancy rates for Svarfaðardalur using texts 1, 5 and 6 as one might hope to.

individual farms it would seem safest to conclude that vacant farms were either rare or within what was seen as being normal. Grenjaðarstaður in Reykjadalur is the one estate which requires no discussion because it had only 23 properties, the status of which is not made clear in 1525, and for which we have no comparable data from the 1440s.[46]

The lists of properties for Munkaþverá (59 properties) and Möðruvellir (45 properties plus part shares in the islands of Grímsey and Flatey) make no mention of vacancies. Each monastery's property portfolio had changed since the 1440s but with a large core of their properties remaining the same. Both monasteries seem to have disposed of some smaller properties or rights, including a mix of those occupied and empty ones in the 1440s. Munkaþverá had also bought a significant number of new farms and shares in farms while Möðruvellir had sold some of its smaller, unoccupied properties in Svarfaðardalur. Overall it would seem that these monasteries were more likely to buy and sell small farms and the shares in farms of specific resources (*ítök*) which had low rents. Monasteries' buying and selling of these small farms would not appear to be a clear indication of their permanent abandonment but more a sign of a fairly active land market.

Reynistaður had had seven clearly vacant properties in 1446 among its 52 and also had seven properties out of 50 listed in 1525. Those earlier seven show limited overlap with the later ones: three were now occupied, three had disappeared from Reynistaður's roster and only one remained empty. Three occupied farms that were occupied in 1446 were now listed as empty and one of the two farms with a temporarily reduced rent was now vacant. The farms which had been occupied in 1446 but were abandoned in 1525 were among the lower-valued among those occupied ones. This all suggests that farms were being vacated and occupied fairly regularly.

For Hólar it appears the bishop owned whole districts in 1525 rather than individual properties being listed with the result that none can be identified as empty. The remainder of Hólar's properties numbered 285 of which 21 were vacant. For the districts where no vacancies were identified it would seem sensible to conclude again that there were no problems in the minds of those writing, something reinforced by the fact that we have hardly any empty farms in those districts in the 1440s.[47] The 7% rate of vacancy for the diocese's individual properties is therefore unlikely to be any higher for the whole of its possessions and, yet again, the empty ones are small to average in size.

Þingeyrar had 61 occupied properties and, listed separately, 35 vacant ones. The latter, when they are identifiable, seem to have been mostly of 10h or less in *Jarðabók* and lay in districts mostly to the west of Skagafjörður and where there were concentrations of similar small properties. The occupied ones were almost all of 16h or more. There seems to be no grounds on which to judge the occupational status of the properties because no figures are given for rents. What is also noticeable, besides the high rate of unoccupied properties generally, is the possibly continued

[46] Grenjaðarstaður was close to the parishes of Þóroddsstaður and Helgastaðir (see Table 1) but there is still nothing meaningful to say about Reykjadalur in this context.
[47] E.g. DI, vol. 5, 36. Hólar's holdings described as 'east of Héraðsvatn' in Skagafjörður where there were at least 10 properties providing full rents in 1449.

concentration of them around Breiðabólstaður in Vatnsdalur, close to Þingeyrar itself. It is difficult to be certain of the boundaries of the 1440s parish of Breiðabólstaður, where six farms were empty, but if the parish extended westwards from Breiðabólstaður then at least six vacant farms might have fallen within in it in 1525. Regardless of whether the farms were quite the same ones in both lists, this does seem to be a rare instance where seems to have been some kind of long-term but small-scale abandonment of small properties.

In sum then there were relatively few empty farms in the north in 1525 in comparison with the 1440s. The general pattern was still one where smaller properties were likely to be vacant at any given time. By 1525 it looks as if the north generally seems to have had a settlement pattern closer to what is generally perceived of as being normal, whereby most farms were occupied. The fact that a large proportion of Þingeyrar's properties were empty is not easy to explain based on the limited evidence. It may be that there had been a disproportionately high local mortality in the 1490s or else poorer management of the monastery's properties since the epidemic(s), with fewer potential tenants being enticed in relative to elsewhere. Perhaps Hólar and Reynistaður had been able to encourage people to live on their farms. In this respect the annals indicate that poorer people and their families migrated from the Vestfirðir to the north to abandoned holdings in the north. [48]

'Plagues' and other epidemics

Notwithstanding the view of the present authors that the 1402 and 1494-5 epidemics were significant outbreaks of plague caused by *Yersinia pestis*, there were clearly other causes of large-scale population decreases in medieval Iceland, including outbreaks of disease. There can be little doubt that there were regular, small outbreaks of disease and famine which have not been recorded in the kinds of sketchy written texts we have for the fifteenth and early sixteenth century. What information we do have hints at occasional significant outbreaks of disease, although these get less attention than they might in connection with the Black Death.[49] If we look at annals covering just the period c.1380 to c.1525, however, there were several recorded epidemics, albeit rarely on the same scale.[50] The entries for these other, hard-to-identify diseases – also given the rather unspecific name *sótt* (sickness) – are no less credible than those for the epidemics of 1402 and 1494. These epidemics are sometimes said to have affected the whole country and sometimes described as great or large (*mikill*) and as causing high mortality. They still differ from the 'plagues' of 1402 and 1494, however, by not being accorded the countrywide impact and speed of

[48] *Annálar 1400–1800*, vol. 1, 74-5. This might have included not only people from around Þingeyrar (Húnavatnssýsla) but possibly also those people from Vestfirðir whom Björn Jónsson claimed had moved to the north after the 1494–95 epidemic.
[49] Cf. Jón Ólafur Ísberg, 'Sóttir', 205, with reference to previous studies including the influential work of Jón Steffensen, *Menning og meinsemdir. Ritgerðasafn um mótunarsögu íslenzkrar þjóðar og baráttu hennar við hungur og sóttir* (Reykjavík, 1975).
[50] *Islandske Annaler indtil 1578*. Udgivne ved G. Storm (Christiania, 1888), 213. 1347 actually seems to have witnessed one of the largest deadly epidemics, a *bólnasótt* (small pox?) which lasted more than a year and has a fuller write-up than many later epidemics. It is hard to compare the frequency of epidemics over the long term in Iceland because the form of the textual evidence varies so much.

impact which strongly suggest that these two epidemics were widely recognised as being somehow different from others.[51]

If we just tally up the instances of disease outbreaks then there are recorded outbreaks in c.1380 with some annals using very similar wording to record a great epidemic in Norway followed by one in Iceland. Disease in Norway is often identified before it gets to Iceland.[52] It might be, for example, that the entry for 1391 in one annal that an Icelandic traveller dies three weeks after coming home from Norway that it is making a connection between the 'great epidemic' (*Sott micil*) in Norway it has just recorded.[53] *New Annal*, probably written in the 1430s, records a pair of entries for 1420 and 1421 which are probably only credible as an account of a long term outbreak of disease rather than anything more specific. It claims that in the first year (in winter) everywhere was affected by a disease which killed people in their fifties, and barely anyone older. The following year 'many healthy people' in their twenties died.[54] For 1430 or 1431 the 'great epidemic' is recorded which seems to have interrupted or caused the recording of estates by the bishop of Hólar.[55] According to Björn Jónsson's *Skarðsárannáll* yet another 'great plague' caused 'great mortality' in 1472. For either 1479 or 1484 he records that a wealthy woman is identified as dying as a result of *sótt*, presumably an indication of endemic disease or an allusion to another outbreak. Finally another plague is recorded for 1511 or 1512.[56] All these events seem to have been recorded without the additional anecdotes attached to some accounts of the 'plagues' but any one of them could have had a significant role in reducing population and emptying farms. In addition to these are a handful of references to bad weather or bad farming conditions (as well as occasional references to notably good weather) which are also unquantifiable in their impacts. These, like the other epidemics, were not altogether unusual in Iceland but may well have had a greater influence on settlement patterns than is allowed for.[57]

Climate variation and instability

The impact of climate change on the lives of farmers for the period c. 1400 to c. 1525 also needs to be taken into account. It has long been recognised that Europe and the North Atlantic experienced a period of relatively warm temperatures period (the Medieval Climate Anomaly) from c.900 to c.1250. This was followed by relatively low temperatures, especially in the period c.1400 to c.1700.[58] This must have decreased the viability of farms of marginal potential even if it is unclear how readily people perceived such changes in the short term. Similarly recent research on past climates has highlighted significant changes in the predictability of the weather in the fifteenth and early sixteenth century; from about 1425 the weather in the North Atlantic appears to have varied on a year-by-year basis in ways which people would

[51] The Icelandic annals describe the outbreak of the Black Death in Norway as a deadly disease (*drepsótt*). *Islandske Annaler* 213, 275, 354, 403.
[52] *Islandske Annaler* 281, 365, 413.
[53] *Islandske Annaler* 367.
[54] *Annálar 1400–1800*, vol. 1, 23.
[55] *Islandske Annaler*, 370; *Annálar 1400–1800*, vol. 1, 53.
[56] *Annálar 1400–1800*, vol. 1, 67, 71, 78.
[57] *Annálar 1400–1800*, vol. 1, 24, 24–5, 83–84, 84; *Islandske Annaler* 295, 287, 416; *Safn til sögu Íslands*, vol. 1, 44. Recorded for 1390, 1405, 1424, 1426, 1430, 1519, 1520.
[58] M. Mann, Z. Zhang, S. Rutherford and others, 'Global Signatures and Dynamical Origins of the Little Ice Age and Medieval Climate Anomaly', *Science* 326 (2009), 1256-60.

not have witnessed in preceeding decades.[59] In the light of this it would seem that temporarily abandoned farms might not have been so readily risked by potential new tenants who instead might have preferred to stay in established households. While people may have been reluctant initially to leave their homes in crises, they may equally have been resistant to going (back) to a poorer farm. This could explain some part of the lower levels of farm occupancy we see. Furthermore recent research on landscapes in southern Iceland does suggest continued management of the farming landscape in the fifteenth and sixteenth centuries, lessening the impacts of climatic deterioration. This implies a sufficiently large population capable of managing pastures in much the same way as in previous centuries, regardless of where people might have been living.[60]

Competing models of plague transmission in Iceland

Before we discuss how the plague reached Iceland and then how it was transmitted to humans, it is first necessary to review the competing explanations already present in the literature. Following the identification of *Y. pestis* as the aetiological agent responsible for the third pandemic by Yersin in 1894, the role of insect vectors, such as the flea, in spreading the disease was first identified by Simond in 1898 and rapidly became the most widely accepted epidemiological explanation of the phenomenon for late medieval Europe just as for other times and places.[61]

In simple terms, the rat-flea model requires that bacteria are spread from rat to rat by flea bites. During the times of year when the conditions of warmth and humidity are at an optimum, the fleas multiply and the disease becomes epizootic in the rodent population. The rapid spread of infection is thought to cause a 'die-off' of the rodent population and, as the fleas leave the dead rats, they will then attack the less well-favoured human hosts. Hence, the disease is incidental to the rodent-flea dynamic but is no less unwelcome for being so. *Xenopsylla cheopis* was thought to be one of the most efficient at transmitting plague because of its tendency to develop a 'blocked' proventriculus, a valve that connects the oesophagus to the midgut, by forming a large mass of bacteria. When blocked fleas attempt to feed, blood containing large quantities of bacteria from the blocking mass is refluxed into the bite site.[62]

It is important, however, not to be persuaded by an over-simplistic view of the biology of plague. There are numerous rodent species capable of sustaining enzootic

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[59] A. Dugmore, D. M. Borthwick, M. J. Church and others, 'The Role of Climate in Settlement and Landscape Change in the North Atlantic Islands: An Assessment of Cumulative. Deviations in High-Resolution Proxy Climate Records', *Human Ecology* 35 (2007), 169–78. Cf. J. Barrett, 'What caused the Viking Age?', *Antiquity* 82 (2008), 673.

[60] R. Streeter, A. J. Dugmore and Orri Vésteinsson, 'Plague and landscape resilience in premodern Iceland', *Proceedings of the National Academy of Sciences of the United States of America* 109 (2012) 3664–9.

[61] A. Yersin, 'La peste bubonique a Hong-Kong', *Annales de l'Institut Pasteur*, 8 (1894), 662–7; P. L. Simon, 'La propagation de la peste', *Annales de l'Institut Pasteur* 12 (1898), 625–87; G. Lamb, *The Etiology and Epidemiology of Plague: A Summary of the Work of the Plague Commission* (Calcutta: Suprintendent of Government Printing, 1908).

[62] A.W. Bacot and C. Martin, 'Observations on the mechanism of the transmission of plague by fleas', *Journal of Hygiene*, Plague Supplement 3, 13 (1914), 423–39.

foci in various parts of the world.[63] Black Rats are of particular interest simply because they are often commensal, living in close contact with humans, and have a habit of 'hitch-hiking' on human transport (hence the widely used names 'roof-rat' and 'ship-rat').

Although *X. cheopis* is perceived by some to be the most efficient flea vector, recent studies suggest that this imay not actually be the case. This is partly because of a long extrinsic incubation period before blockage formation, and subsequent transmission, together with a high death rate amongst blocked fleas. Moreover, even though *X. cheopis* is the most commonly implicated flea species, there are numerous other species capable of driving and maintaining an epizootic. For instance, *Nososphyllus fasciatus*, more often found in colder climates, can also develop a blocked proventriculus and is also commonly found on rats and other rodents.[64]

Other disease vectors are possible. Spread between humans via the 'human flea' *Pulex irritans* and other ectoparasites such as the human body louse *Pediculus humanus* cannot be ruled out entirely although these modes of transfer are not widely accepted as being significant in the epidemiology of modern plague.[65] However, plentiful specimens of *P. humanus* and other species of louse have been found in excavations of medieval farmhouses in Iceland.[66]

A major difficulty with the rat-flea model, in this context, is that there are no written records of rats being present in Iceland in the Middle Ages and there are no records (as yet) of rat bones being found in archaeological deposits dating to the medieval period. Moreover, it has been argued that the Black Rat could not have survived in Iceland because it prefers warmer climates and, in any case, the environmental conditions required by the 'rat-flea' *X. cheopis* to breed in sufficient numbers were also absent. Karlsson is keen to stress that the annals report that the epidemics continued throughout the winter when, in his view, fleas could not have been involved.[67]

Based on what we know at present it is difficult to see how we can reconstruct the fifteenth century ecology of commensal and sylvatic Icelandic rodents, together with their fleas. As Gunnar Karlsson has rightly emphasised medieval written texts produced in Iceland show a strong familiarity with mice but suggest that rats were

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[63] See, for example, R. Pollitzer, 'Plague Studies: 6. Hosts of the infections', *Bulletin of the World Health Organisation* 6 (1952), 381-465 for thorough review.

[64] R. J. Eissen and others,,'Early-phase transmission of Yersinia pestis by unblocked fleas as a mechanism explaining rapidly spreading plague epizootics', *Proceedings of the National Academy of Sciences of the United States of America* 103 (2006), 15380–5; L. K. Little, 'Plague historians in lab coats', *Past and Present* 213 (2011), 286 comments, 'plague historians can henceforth safely retire their exclusive fixation upon *Rattus rattus* and *Xenopsylla cheopis*.'; A. L. Burroughs, 'Sylvatic plague studies. The vector efficiency of nine species of fleas compared with *Xenopsylla cheopis*', *Journal of Hygiene* 45 (1947), 371–96.

[65] M. Drancourt, L. Houhamdi and D. Raoult, 'Yersinia pestis as a telluric, human ectoparasite-borne organism', *The Lancet Infectious Diseases* 6 (2006), 234–41; L. Houhamdi, H. Lepidi, M. Drancourt and D. Raoult, 'Experimental model to evaluate the human body louse as a vector of plague', *Journal of Infectious Diseases* 194 (2006), 1589–96; Pollitzer, 'Plague Studies', 392.

[66] Guðrún Sveinbjarnardottir, Egill Erlendsson, K. Vickers and others, 'The palaeoecology of a high status Icelandic farm', *Environmental Archaeology* 12 (2007), 189.

[67] Gunnar Karlsson, 'Plague without rats', 263, 267, 277-8.

either unknown or exotic.[68] Archaeological excavations up to 1996 had not yielded evidence of rat skeletons of medieval date in Iceland and, indeed, still have yet to be found. Mice bones and evidence of rodent gnawing on other animal bones have been found on sites as early as the tenth-century (Hofstaðir in Mývatnssveit) and at other sites for before c.1700.[69] Other excavations in recent decades have shown much later evidence of the presence of rats in Iceland, through the evidence of gnawed animal bones in both urban contexts (in Reykjavík at Tjarnargata 3c) and rural ones (Bessastaðir near Reykjavík, Skútustaðir in Mývatnssveit, Möðruvellir in Hörgárdalur).[70] At present it may not be possible to distinguish between the evidence of gnawing of rats and mice but the absence of rat bones might prove to be telling. Equally it is still the case that relatively little attention has been paid to late medieval archaeology in Iceland and that much Icelandic settlement archaeology was not done sufficiently well to have necessarily found or recorded animal bones rigorously. That said, one would have expected any large late medieval rat population to have left skeletal traces, although it is possible that rats may have died in places where archaeologists are less likely to find them, i.e. within the confines of a dwelling or outbuilding. This, however, does not undermine the argument of this article for temporary appearances of rats in Iceland and for a more complex view of the plague's spread.

Pneumonic cases often occur in bubonic epidemics that are associated with epizootics in commensal rodents. The infection is initially transferred from rodent to man by flea bites. If the infection spreads to the lungs the patient is said to have developed secondary pneumonic plague. Plague may then spread via droplet infection to others who are then said to have contracted primary pneumonic plague.

The pneumonic model, though an attractive explanation of plague when rats and their fleas are not obviously responsible, also has its problems since the aerosolisation of *Y. pestis* appears to be an inefficient means of spreading plague. For instance, it was found that the distance that coughing patients projected bacteria in the plague wards of Manchuria was limited to 1 metre and there is ample evidence that potential contacts or even those who co-habit with sufferers frequently do not catch the disease.[71] Another problem is that the time from the onset of symptoms to death for untreated patients is short (two to three days) and they are thought to be infectious

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[68] Gunnar Karlsson, 'Plague without rats', 279–80. Jón Ólafur Ísberg, 'Sóttir', 195, could not get round this problem either.

[69] T. H. McGovern, 'The Archaeofauna' in *Hofstaðir. Excavations of a Viking Age Feasting Hall in North-Eastern Iceland*, ed. G. Lucas (Reykjavík: Fornleifastofnun Íslands, 2009), 172, 180, 221; M. T. Hicks and R. Harrison, *A Preliminary Report of the 2008 Midden Excavation at Skútustaðir, N. Iceland*. Unpublished NORSEC Report No. 45 (CUNY Northern Science & Education Center, 2008).

[70] S. Perdikaris, C. Amundsen and T. H. McGovern, *Report of Animal Bones from Tjarnargata 3C, Reykjavík, Iceland*. HERC/NORSEC Zooarchaeology Laboratory Report No. 1 (2002), 13; R. Harrison, *Möðruvellir in Hörgárdalur, N. Iceland: General Overview of the Archaeofauna Analyzed from the 2006-08 Midden Mound Excavations*. HERC/NORSEC Zooarchaeology Laboratory Report No.59 (2011), 13, 35.

[71] R.P. Strong and O. Teague, 'Studies on pneumonic plague and plague immunization', *Philippine Journal of Science* 8 (1912), 129–268; H. Wang, Y. Cui, Z. Wang and others, 'A Dog-Associated Primary Pneumonic Plague in Qinghai Province, China', *Clinical Infectious Diseases* 52 (2011), 185–90; G. W. Gale, 'An outbreak of pneumonic plague in the Kalahari', *South African Medical Journal* 15 (1941), 369–73.

only when capable of projecting infected sputum whilst low contact frequency may also be expected because patients rapidly become incapacitated.[72]

Nevertheless, it must be remembered that the most notorious modern pneumonic outbreaks which occurred in Manchuria and adjacent provinces of China in 1910/11, 1917/18 and 1920/21 were responsible for 60,000, 16,000 and 9,300 deaths respectively.[73] These show that, under certain circumstances, this form of plague can be responsible for large-scale disasters. Moreover, the Manchurian outbreaks took place over winter in conditions of extreme cold, conditions under which fleas are unable to breed or develop.

The transport of *Y. pestis* to Iceland: Imports, exports and fishing

If we are to explain the medieval epidemics of plague in Iceland in terms of the spread of *Y. pestis* there needs to be a credible explanation of how it got to Iceland and then how it spread amongst the human population. There is no need, as we shall see, to exclude the rat and its fleas as part of this phenomenon although, for the reasons pointed out by Karlsson, it is hard to accept that the dominant phase of the epidemics could be bubonic.

It is well documented that modern plague moves along trade routes frequently associated with 'rat-favoured' merchandise.[74] The bacteria may be carried by rats themselves or adult fleas which can survive months without feeding. The aetiological agent which caused the Icelandic epidemics was likely brought to Iceland through such trading activities and, as mentioned above, the *New Annal* records that the first epidemic arrived by ship in the harbour of Hvalfjörður on the south west coast.

There was regularly trade between Iceland and Europe throughout this period with Norwegian, English and Hanseatic merchants holding sway in succession.[75] Before the fourteenth century, Iceland's exports were largely *vaðmál* (coarse cloth) but the emphasis changed to *skreið* (stockfish/dried fish) in the fourteenth century and this trend continued with the arrival of English vessels from around 1412, hence one modern scholar's view that the English were the most likely carriers of the plague.[76] The ships used by the English ranged from 16 tons to 80 tons but were relatively

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[72] J.L. Kool, 'Risk of person-to-person transmission of pneumonic plague', 1166-72.
[73] L.-T. Wu, 'Plague in the Orient with special reference to the Manchurian outbreaks' *Journal of Hygiene* 21 (1922), 65.
[74] G. Lamb, The Etiologyand Epidemiology of Plague: A Summary of the Work of the Plague Commission (Calcutta): Suprintendent of Government Printing, 1908).; M. McCormick, 'Rats, communications, and plague: towards an ecological history', *Journal of Interdisciplinary History* 24 (2003), 1–25; M. Sharif, 'Spread of plague in the southern and central divisions of Bombay province and plague endemic centres in the Indo-Pakistan subcontinent', *Bulletin of the World Health Organisation* 4 (1951), 75–109
[75] M. Gardiner and N. Mehler, 'English and Hanseatic trading and fishing sites in medieval Iceland: report on initial fieldwork', *Germania* 85 (2007), 385–427; B. Gelsinger, *Icelandic Enterprise. Commerce and Economy in the Middle Ages* (Columbia, SC: University of South Carolina Press, 1981). See also Björn Þorsteinsson and Guðrún Ása Grímsdóttir, 'Hafskipahafnir á 14. og 15. öld' in *Saga Íslands IV* , ed. Sigurður Lindal (Reykjavík: Hið íslenska bókmenntafélag, 1989), 143–59; R. Harrison, H. M. Roberts and W. P. Adderley, 'Gásir in Eyjafjörður: International Exchange and Local Economy in Medieval Iceland', *Journal of the North Atlantic* 1 (2008), 99-119.
[76] Jón Ólafur Ísberg, 'Sóttir', esp. 210.

small by European standards. The Hanse merchants, however, used ships which could carry between 80 and 200 tons. Iceland imported a variety of goods. Of these, grain and cloth provide a suitable substrate for the inadvertent transport of rats and fleas: such ships were certainly large enough for rats and fleas to have gone unnoticed.

Archaeologists have identified another important consequence of the stockfish trade which was the generation of large quantity of waste in seasonal fishing stations where large quantities of fish were processed ready to be exported or transported to other centres where they would be consumed. The presence of a very high number of cranial bones and vertebral elements combined with a comparative shortage of cod caudal vertebrae characterise a fish processing site.[77] Such material would have provided an excellent source of food for rats.

Written evidence also includes references to islands or coastal areas where seasonal fishing took place but this is now being more precisely charted by the physical evidence of seasonal fishing booths, relatively small structures designed to temporarily house a boat's crew but not a farming household.[78] This supported a movement of workers from farms to fishing sites which, again, is another facet of behaviour which may have facilitated the movement of plague and will be discussed below.

The establishment of enzootic foci on Iceland

Previous authors have tended to take at face value the annals' account of the arrival of plague at one particular point in Iceland and its subsequent spread throughout the country.[79] It has also often been assumed that the plague was completely absent in Iceland before the first recorded epidemic, and again between the first and second epidemics. Neither of these assumptions need be true. If the plague travelled by ship and was carried by either rats, or merchandise, there is the possibility that there were multiple introductions at different landing sites where trade was carried out. This means that although the plague gave the impression of a progressive spread across Iceland, the phenomenon may, instead, have been a function of the movement of ships around the coast, as has been argued for the British Isles by Maddicott for both the first and second pandemics.[80]

The literature of modern plague records many incidences of pneumonic plague outbreaks throughout the world which are small and die out without developing into

[77] Y. Krivogorskaya, S. Perdikaris and T. H. McGovern, 'Cleaning up the farm: a later medieval archaeofauna from Gjögur, a fishing farm of NW Iceland', in *Dynamics of northern societies proceedings of the SILA/NABO Conference on Arctic and North Atlantic Archaeology, Copenhagen, May 10th-14th, 2004*, eds B. Grønnow, J. Arneborg and H.C. Gulløv (Copenhagen: Aarhus University Press, 2005), 383–95Jón Ólafur Ísberg, 'Sóttir', 198 appears more sceptical about the possible role of fishing stations as homes for rats.
[78] R. Edvarðsson, S. Perdikaris, T. H. McGovern and others, 'Coping with hard times in North-West Iceland: Zooarchaeology, History, and Landscape Archaeology at Finnbogastaðir in the 18th century', *Archaeologia Islandica* 3 (2004), 20–48.
[79] E.g. Gunnar Karlsson, 'Plague without rats', 268-71; Benedictow, Plague.
[80] J. Maddicott, 'Plague in Seventh-Century England', in *Plague and the End of Antiquity*, ed. L. K. Little (Cambridge: Cambridge University Press, 2007), 171–214.

widespread epidemics.[81] Bearing in mind that pneumonic plague is outwardly indistinguishable from other kinds of bacterial pneumonia, save for the high mortality rate,[82] it is unlikely that small outbreaks would have been sufficiently noteworthy to be recorded by annalists. Similarly, small outbreaks of bubonic plague might also have gone unreported.

A necessary step in the development of a sustained plague epidemic, characterised either by bubonic or pneumonic spreads is the establishment of a population of animals which act as a reserve, or 'focus', of the disease. This leads us to consider the possibility that rats (with fleas) could have established colonies on the mainland even though it has been suggested that rats were not present in Iceland in the Middle Ages and that Iceland is too cold for them to establish themselves.[83]

The presupposition that Iceland's climate was too harsh for rats needs to be debunked first of all. Despite its northerly latitude, Iceland's climate is essentially maritime with cool summers and mild winters since the Irminger Current, a branch of the North Atlantic Drift encircles the south, west and north coasts .[84] The inhabited coastal regions tend to be milder than its uninhabited highland interior and it is potentially significant that the fifteenth century was relatively mild.[85]

While there is no unambiguous written evidence for rats in Iceland, the fact that mice were reported in Icelandic records is suggestive that other rodents might have been there. It is even possible that the annalists used 'mouse' for different species of rodent, even if there are arguments against this.[86] It also should be noted that rats, when present, are not always to be seen, even when associated with an epizootic. For instance, Petrie *et al* describe searching for rats in mud-brick houses in Egypt where plague-carrying fleas were found in abundance.[87] These investigators had to dismantle the houses before a large numbers of dead rats could be found in burrows and shafts which served as cesspits.

Black rats may be relatively scarce in contemporary Northern Europe, but the reasons for this are unlikely to be solely temperature related since it has been found that this

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[81] For example: H. Wang and others, 'A Dog-Associated Primary Pneumonic Plague in Qinghai Province, China', *Clinical Infectious Diseases.* 52 (2011), 185-90; D. van Zwanenberg, 'The last epidemic of plague in England? Suffolk 1906–1918', *Medical History* 14 (1970), 63–74; M. Baltazard, M. Bahmanyar, C. Mofidi and B. Seydian, 'Le foyer de Peste du Kurdistan 1. Recherches dans le foyer', *Bulletin of the World Health Organisation* 5 (1952), 441–72.

[82] K.F Meyer 'Pneumonic Plague' Bacteriological Review 25 (1961), 249; K. L. Gage, D. T. Dennis, K. A. Orloski and others, 'Cases of cat-associated human plague in the Western US, 1977–1988', *Clinical Infectious Diseases* 30 (2000), 897 for examples of misdiagnosis.

[83] Duncan and Scott, 'What caused the Black Death?'; G. Twigg, The Black Death: a problem of population-wide infection', *Local Population Studies* 71 (2003) 40-52.

[84] Markús Á. Einarsson, 'Climate of Iceland' in *World Survey of Climatology: 15: Climates of the Oceans*, ed. H. van Loon (Elsevier: Amsterdam, 1984), 673–97.

[85] G. Massé, S. J. Rowland, M.-A. Sicre and others, 'Abrupt climate changes for Iceland during the last millennium: evidence from high resolution sea ice reconstructions', *Earth and Planetary Science Letters* 269 (2008), 565–9. Based on measurements of a biomarker (IP_{25}) found in stratified sediments produced by sea ice algae but historical sources are unreliable or limited from 1430 to 1560.

[86] Gunnar Karlsson, 'Plague without rats', 279-80

[87] G. F. Petrie, R. E. Todd, RE and others, 'A report on plague investigations in Egypt', *Tropical Disease Bulletin* 21 (1924), 875–6.

species has established itself on a number of sub-Arctic islands.[88] It seems that a mean winter temperature of 2°C may be a limiting factor for black rats living away from human habitation although this may not be an effect of cold *per se* but on the ability of the species to forage[89] since Black rats have been found to infest cold stores at a constant temperature of 4°C.[90] The black rat is not niche dependent, it is an opportunist and able to modify its behaviour to survive in a variety of environments. There therefore seems no environmental reason for the black rat not to have made it to Iceland and survived in the Middle Ages just as the attested mice which have a smaller body mass. Interestingly, the flea *X. cheopis*, more frequently found in warmer climates, also exhibits a surprising ability to survive in unexpected places. It has been observed in Manchuria where the flea survived winters more severe than those in Iceland through living close to heating systems.[91]

As noted above, the absence of archaeological rat bones in medieval Iceland is not definitive evidence of their absence. It had been thought that *R. rattus* was relatively uncommon in northern Europe in the middle ages but they have now appeared in the archeology in substantial numbers.[92] The most recent presentation of the archaeological evidence for presence rats in Norway demonstrates that they were present in urban or proto-urban settlements in the period before 1350.[93] These places, particularly Bergen and Trondheim, were the primary points of contact with Iceland. Given, the tendency of rats to stow away, and the regular contact between Iceland and Norway, and later England, it would be surprising if rats never arrived and periodically established colonies in several locations in Iceland.

In the fourteenth and fifteenth centuries the North Atlantic economy was scaling-up, as fishing became more industrialised, even before the documented arrival of English fishing vessels in 1412. One result of this process was the creation of environments in which rats might thrive. We have clear evidence that rats could survive in the major nodes of this exchange network, the emerging small towns of Norway. But at other locations in Iceland the production of dried fish for export (*skreið*), produced waste from the gutting and preparing of the fish which would have fed any rats brought to Iceland in ships bringing grain or other foreign goods. While the sites of fish processing were not necessarily the same as those which were directly visited by foreign trading vessels, there were regular lines of communication between many coastal sites in Iceland such that from time to time rats could arrive and survive sufficiently long to pass on infection. If rats or fleas, carrying *Y. pestis* subsequently did turn up, the infection would spread to colonies already established close to human

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[88] Z. Pucek, ‘A preliminary report on threatened rodents in Europe’ in *Rodents: A World Survey of Species of Conservation Concern*, ed. W. Z. Lidicker. Occasional papers of the IUCN(SSC) 4 (1989), 26–32; Y. Frenot, S. L. Chown, J. Whinams, and others, ‘Biological invasions in the Antarctic: extent, impacts and implications’, *Biological Reviews* 80 (2005), 45–72.
[89] B. Studholme, ‘Ship rat (*Rattus rattus*) irruptions in South Island beech (*Nothofagus*) forest’, *Conservation Advisory Science Notes No 318* (Wellington: Department of Conservation, 2000), 1–9.
[90] D. K. Kocher and V. R. Parshad, ‘Shorter tails: a thermal adaptation of rats in cold stores’, *CSIRO Rodent Research Newsletter* 20 (2005), 6.
[91] Ibid., 331-2.
[92] P. L. Armitage, ‘Unwelcome companions: ancient rats reviewed’ *Antiquity* 68 (1994), 231–40. M. McCormick, ‘Rats, communications, and plague: towards an ecological history’, Journal of Interdisciplinary History 24 (2003), 1–25
[93] A. K. Hufthammer and L. Walløe, 'Rats cannot have been intermediate hosts for Yersinia pestis during medieval plague epidemics in Northern Europe', *Journal of Archaeological Science* 40 (2013), 1754–5.

habitation thus facilitating the transfer of plague to humans. Plague bacteria and infected fleas are able to winter in nests and burrows, thus helping to maintain the presence of the disease organism.[94]

The transfer of Y pestis from animal to man

As mentioned above, infection can be passed to humans via fleabites. However, whilst there are many species e.g. the more northern *N. fasciatus* in addition to *X. cheopis* identified as plague carriers, it seems most unlikely that any of them could have been present in significant numbers outside animal burrows in winter months, at a time when the annals tell us plague was still rampant. However, there is evidence that both bubonic and primary pneumonic plague can be caught directly from plague-affected animals without an insect vector. This possibility was noted by Benedictow but he felt that it was of little significance since the prevailing view, until at least the middle of the last century, was that it was not proven.[95]Nevertheless, he cited the review by Gage *et al* of the incidence of plague, caused by exposure to infected cats, amongst American veterinary surgeons. The infection was passed on pneumonically by exposure to infected droplets and bubonically by scratches. There was no evidence of flea involvement.[96] There are numerous other reports in the literature of outbreaks of pneumonic plague which appear to have started when individuals handle or skin infected wildcats, cats, dogs and rodents.[97] Dogs and cats are now considered to be a significant risk factor in rural areas of the USA where *Y. pestis* is enzootic; they have been well established as domestic animals in Iceland for many centuries.[98]

The evidence summarised above shows that the transfer of plague bacteria from animal to man can be facilitated by domestic animals acting as intermediate vectors. In this scenario, rat fleas are not required to bite humans. Thus, even when rat fleas are not plentiful, cats and dogs will seek out rodent pests and thus acquire plague themselves which they convey to their human companions by the pneumonic route, the bubonic route or both.

The transfer of Y pestis from human to human

The transfer of the bacterium via fleabite alone does not provide us with a satisfactory explanation of a large and sustained epidemic of plague in Iceland since we would need to postulate the existence of large populations of rodents, a plethora of fleas and

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[94] Pollitzer, *Plague*, WHO Monograph Series, No. 22 (Geneva: World Health Organisation, 1954), 319.
[95] Benedictow, *Plague*, 214.
[96] K. L. Gage, D. T. Dennis, K. A. Orloski and others, 'Cases of cat-associated human plague in the Western US, 1977–1988', *Clinical Infectious Diseases* 30 (2000), 893–900.
[97] M. L. Gupta and A. Sharma, 'Pneumonic Plague, Northern India, 2002', *Emerging Infectious Diseases* 13 (2007), 664–6; Gage and others, 'Cases of cat-associated human plague', 894; Wang and others, 'A Dog-Associated Primary Pneumonic Plague', 186; W. H. Kellog, 'An epidemic of pneumonic plague', *American Journal of Public Health* 10 (1920), 600.
[98] K. A. Orloski and S. L. Lathop, 'Plague: a veterinary perspective', *Journal of the American Veterinary Medical Association* 222 (2003), 444–8; Stefán Aðalsteinsson, 'Uppruni íslenska húsdýra' in *Íslensk þjóðmenning. Uppruni og umhverfi*, I, ed. Frosti Jóhannsson (Reykjavík: Bókaútgáfan þjóðsaga, 1987), 41-44, 45; E. P. Jones, K. Skirnisson, T. H. McGovern and others, 'Fellow travelers: a concordance of colonization patterns between mice and men in the North Atlantic region', *BMC Evolutionary Biology* 12:35 (2012), doi: 10.1186/1471-2148-12-35.

a substantial die-off for which there is no evidence and, as discussed above, the epidemics continued throughout the winter months. We can argue for a jump from animal to man from established enzootic foci but the case for multiple infections on a larger scale is more difficult to envisage. This means that we need to consider the pneumonic route as a more probable explanation for the sustained Icelandic plague epidemics.

It is well documented that, travellers infected with the bubonic form, but fit enough to walk, are prone to develop secondary lung infection and then spread the disease in its pneumonic form and outbreaks can also occur without the help of fleas as discussed above.[99] However, as previously discussed, the bacterium is not readily transferred through air by aerosolized droplets. So, in order to understand how the pneumonic form of the disease spreads more easily than expected, the role of human behaviour needs to be considered.

It is widely reported that people who travel to visit the sick may catch the disease and return to their own rural communities where they can infect others. The latency of two to three days between contact and the development of debilitating symptoms is sufficient to allow this.[100] Secondly, the bodies and clothing of the deceased retain the potential to infect others since it has been frequently observed that individuals who handle infected clothing, those responsible for preparing bodies for funeral, and those attending funerals can catch the disease.[101] By way of explanation live aerosolised *Y. pestis* remain viable on metal, polyethylene or paper surfaces for up to five days providing that the culture is maintained within controlled levels of temperature and relative humidity.[102] It is reasonable to suppose that viability would be comparable on articles of clothing or skin. Thus, it is not the sick (who are usually incapacitated) that initiate contact as it might be with, say, influenza. Contact is made when healthy individuals visit immobilised infected individuals, attend funerals or prepare the dead for burial.

The Manchurian epidemics exhibit another set of circumstances which appear to facilitate transfer of the disease between humans. In conditions of intense cold and overcrowding, the disease was first spread amongst Tarbagon fur trappers and then amongst itinerant Chinese workers who were prepared to move long distances by rail and road to get work.[103] A similar epidemic was also noted more recently amongst

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[99] Wang & Others: 'A Dog-Associated Primary Pneumonic Plague in Qinghai Province, China', Clinical Infectious Diseases. 52 (2011):185-90.; W. J. Simpson, *A Treatise on Plague dealing with the Historical, Epidemiological, Clinical, Therapeutic and Preventative aspects of the Disease* (Cambridge: Cambridge University Press, 1905); E. R. Brygoo and M. Gonon, 'L'épidémie de peste pulmonaire de Doany en octobre 1957', *Archives de l'Institut Pasteur de Madagascar* 13 (1958), 865–936.

[100] F. Thielmann and F. Cate, 'A plague of plagues: the problem of plague diagnosis in medieval England', *Journal of Interdisciplinary History* 37 (2007), 371–93.

[101] A. K. Chalmers, 'Notes on cases of plague in Glasgow', *British Medical Journal* Sept 29, 1900, 953–4 on clothing and funeral attendance; L-T. Wu, 'The second pneumonic plague epidemic in Manchuria', *Journal of Hygiene* 21 (1923), 262–288 re clothing and preparing bodies; Simpson, *Report on plague in the gold coast in 1908*, 11.

[102] L. J. Rose, R. Donlan, S. N. Banerjee and M. J. Arduino, 'Survival of Yersinia pestis on Environmental Surfaces', *Applied and Environmental Microbiology* 69 (2003), 2166–71.

[103] A. S. Loukaskin, 'The Tarbagan or the Transbaikalian Marmot as a carrier of plague', *Extrait des Comptes Rendus du XIIe Congrès International de Zoologie, Lisbonne, 1935*: 2097–111; M. Gamsa, 'The epidemic of pneumonic plague in Manchuria 1910–1911' *Past and Present* 190 (2006), 147–84.

itinerant mine workers in the Congo although intense cold was clearly not a factor in this case.[104]

In later medieval Iceland it was usual for many men to work spend part of their year working on a farm and then migrate to fishing stations in the fishing season where they lived in purpose built turf-walled dwellings. While such fishing was on a small scale it would still have resulted in contact with any established rodent-based foci of the disease as described above and a sufficiently crowded environment where the disease, having made the jump from animal to man, could be passed on easily.

The migration of workers from farms to fishing sites where they came into contact return of the sick and the deceased to farmsteads for care and burial, the movement of people between farmsteads to visit the sick, the preparation of the dead for burial and the attendance of funerals would all assist the spread of the disease. Travellers suffering from the bubonic form of the disease, later to develop secondary pneumonic symptoms, would assist in the transfer of the disease inland. The point to emphasise here is that the single route of the disease's diffusion identified in the annals' account for 1402-4 is extremely simplistic and unlikely to reflect the reality.

As a further cautionary note, it must be said that conclusive proof that *Y. pestis* was the disease agent in medieval Iceland cannot be based on incomplete reports of the disease symptoms and epidemiology.[105] This could only be confirmed by finding DNA, or antigens specific to this bacterium, in skeletal remains, and that is not easily done.[106] However, recent studies are beginning to uncover this evidence elsewhere in Europe although there is likely some way to go before it is accepted as the consensus view.[107] As yet, no such studies have been carried out in Iceland.

Estimating mortality

Gunnar Karlsson's estimates of mortality, of 50-60% for the first epidemic, 30-50% for the second, rely on two blocks of evidence: firstly, the brief accounts given in the written sources and secondly, the number of farms which were unoccupied, "some 40 years after the first epidemic receded".

We have argued that the collection of anecdotes, stories and oral tradition do not provide a sufficiently robust foundation on which to construct a statistical analysis. It is also significant that of the nine items of evidence used by Gunnar in his analysis, seven appear to be connected to religious sites so recording the deaths of clergy and their servants. Benedictow has emphasised that there are strong reasons for not using mortality figures from records concerning the clergy to judge mortality rates more

[104] E. Betherat, K. M. Lamine, P. Formenty and others, 'Epidémie de peste pulmonaire dans un camp minier de la république du Congo: le réveil brutal d'un vieux fléau', *Médecine Tropicale* 65 (2005), 511–4.
[105] Jón Ólafur Ísberg, 'Sóttir', 180, on the sheer varieties of possible forms of transmission and associated symptoms.
[106] M. Drancourt, M. Signoli, L. V. Dang and others, '*Yersinia pestis orientalis* in remains of ancient plague patients', *Emerging Infectious Diseases* 13 (2007), 332–3; V. J. Schuenemanna, B. Kirsten, S. DeWittec and others, 'Targeted enrichment of ancient pathogens yielding the pPCP1 plasmid of *Yersinia pestis* from victims of the Black Death', *Proceedings of the National Academy of Sciences of the United States of America* 108 (2011), 1–7.
[107] Little, 'Plague historians in lab coats'.

generally.[108] If the annals were written by scribes associated with the church, the devastating effect that the plague might have had on the scribes' immediate religious community might inform their views to a greater extent than would a less intense mortality elsewhere.

Gunnar's use of farm 'abandonment' as an estimate of mortality is also questionable. Here, we are invited to believe that the rate of unoccupancy, immediately following the first epidemic, was such that a strong signal could still be detected some 40 years after the event despite an assumed population growth of 1% per annum. Even if we accept that all the surveys of the 1430s and 1440s used by Gunnar Karlsson record genuinely empty farms then we see wildly different percentages of abandoned farms, ranging anywhere from as few as 2% (3 of 180) in the West Fjords, to 35% for those of Hólar which suggests a rather patchy effect of the epidemic. Moreover, it can be shown that no more than 20% of farms, mostly of below average size, were empty at the time of the surveys. It is reasonable to believe that smaller farms would be more susceptible to economic challenges than larger ones and that there would always be a number of them unoccupied at any given time but that the number would vary continuously due to environmental changes or other disease epidemics e.g. those recorded in the *New Annal* for 1420, 1421 & 1430/31.

If we accept that the written sources are not credible, in the statistical sense, and that farm vacancy rates some forty years after the event are not reliable indicators of the mortality caused by original outbreak, then what evidence is there to provide an estimate of mortality other than to accept that it was sufficiently high to excite the interest of the annalists? Rather than relying on guesswork, it might be useful to consider the impact of pneumonic plague in a modern epidemic where there is an acceptable record of mortality and population numbers. If, for instance, we consider the famous Manchurian epidemic of 1911, we find that the town of Harbin suffered the highest mortality which was 9,000 deaths.[109] This town had an estimated population of around 30,000 which suggests a mortality rate of no more than 30%.[110] The environment offered for the disease to spread in Manchurian towns was ideal with overcrowding and a population largely ignorant of the need to take preventative measures. Nevertheless, outbreaks were mostly concentrated along the railway lines which provided a means of rapid communication and there is no evidence of a wider

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[108] O. J. Benedictow, *Plague in the late medieval Nordic countries. Epidemiological studies* (Oslo: Middelalderforlaget, 1992). Taking the English parish clergy as his example, he argued that although priests might have had a significantly higher risk of exposure to the disease by carrying out their pastoral duties, on balance they would have suffered a lower mortality. This is because the other epidemiological factors such as better diet, better nursing care, better housing and relatively high average age would serve to increase survivability of the disease once it had been contracted. This view was based on a consideration of bubonic plague which is, indeed, survivable. If we accept the view that the dominant phase of this disease in Iceland was the pneumonic, the opposite would be true since this form of the disease is virtually unsurvivable (without modern antibiotics) and none of the epidemiological factors considered by Benedictow apply except for higher risk of exposure. We would therefore expect the clergy to have a higher mortality rate than the general populace. Indeed, a greater opportunity for nursing care would increase the opportunities for passing on the disease.

[109] L.-T. Wu, 'Plague in the Orient with special reference to the Manchurian outbreaks', *Journal of Hygiene* 21 (1922), 66.

[110] M. Gamsa, 'The epidemic of pneumonic plague in Manchuria 1910–1911', *Past and Present* 190 (2006), 154. Note that Gamsa indicates that the total deaths in Harbin were 1500. He attributes a greater mortality of 7-8000 to the town of Fujiadian, adjoining Harbin, "amounting to about a third of the Chinese town's population".

dissemination throughout the whole of Manchuria.[111] In contrast, Iceland was entirely rural with its largely scattered population likely to have been less susceptible to the spread of disease. It therefore seems more likely than not that the mortality rate in Iceland would have been less than that observed, locally, in Manchuria. It cannot be argued that the disease in Iceland was more lethal than anywhere else since the death rate (untreated) in modern times remains virtually 100%.

Given that one purpose of this article has been to question previous estimates of mortality caused by the Black Death in Iceland, we would speculate that for the 1402-4 outbreak no more than 25-30% of the population died as a result of it. There is every reason for following Gunnar Karlsson's rationale for seeing the 1494-5 outbreak as having less impact – because the levels of vacant farms are lower in the sixteenth-century estate surveys than in the fifteenth-century ones – and so 25% might be a maximum figure for the 1494-5 event.

Conclusions

Hopefully, this consideration of the scale and demographic impact of the Black Death has illustrated the complexity of the issues involved. The Black Death outbreaks of 1402-4 and 1494-95 were no doubt rightly remembered as more deadly than epidemics of other kinds. Mortality, however, was very likely less than the estimates put forward by Gunnar Karlsson.

The description of the plague in the available historical records are considered to be insufficiently robust to permit a useful assessment of mortality and the correlation between numbers of abandoned farms and the size of the population is uncertain. Even allowing for the population to have replenished itself, and assuming that it did, after each Black Death epidemic and before the records, it seems unlikely that either outbreak killed much more than about 25% of the population. This was still a huge catastrophe but this has to be seen in the context of the continued impact of outbreaks of disease, recorded and unrecorded, to which the Icelandic population was subjected.

We argue that the plague bacteria were brought to Iceland by normal trading activities and the vectors were rats, fleas or a combination of the two. On reaching Iceland bacteria were transmitted to established colonies of commensal rats or sylvatic rodents via their mutual fleas. There may have been multiple introductions if ships visited several trading sites.

Colonies of rats, which took advantage of opportunities presented by the waste generated by the stockfish industry, probably existed as small enzootic reservoirs of the disease near trading sites and other human habitations. The disease would then transfer to humans through fleabites or, pneumonically, by direct contact with rodents or companion animals such as cats or dogs. The movement of the bacteria among the human population would then be facilitated in pneumonic form by the migration of workers to and from fishing and trading sites, the return of the deceased to the home

[111] R Farrar, 'Plague in Manchuria' Proceeds of the Royal Society of Medicine, 5(Epidemiological Section) (1912) 1-24. Farrar estimated an attack rate of only 2.25 per 1,000 population across Manchuria as a whole.

farmsteads, visiting the sick, preparing the deceased for burial and contact with the deceased in funeral ceremonies.

The idea that the disease spread in a continuous wave throughout Iceland is unlikely to be correct. The model presented here is one of multiple introductions to Iceland, the establishment of small, multiple enzootic foci and intermittent transfer from animal to man over an extended period of time. A more sanguine approach to some of the evidence for the impacts of the Icelandic epidemics – their being smaller and patchier than has been suggested – supports this view. We do not have to postulate the continuous presence of rats over centuries but accept that the environmental conditions required to drive a significant epizootic and epidemic may have been at an optimum on relatively few occasions.