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**Sillages de la peste noire en Afrique subsaharienne :
une exploration critique du silence**

Reflections on plague in African history (14th–19th c.)

Penser la peste dans l'histoire de l'Afrique (XIVe-XIXe s.)

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Reflections on plague in African history (14th–19th c.)

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Et puisqu'un homme mort n'a de poids que si on
l'a vu mort, cent millions de cadavres semés à
travers l'histoire ne sont qu'une fumée dans
l'imagination
Albert Camus, *La Peste* (1947)

Introduction

- ¹ In past publications, I gradually and carefully brought forward the idea that the recurrent pandemics of bubonic plague that struck Europe from the late 1340s¹ may well have spread to Sub-Saharan Africa, with major consequences for the demography of the continent as well as for its socio-political dynamics. Expressed in such a general and hypothetical form, this is hardly a new idea. Several archaeologists and historians before me have suggested the possibility of such an extension of this pandemic to Africa; but seemingly faced with the impossible task of strengthening the argument *ex silentio*, they did not explore the question further.² The Black Death³ has thus remained one of many tangential footnotes to African history and remains a non-issue in the historiography of late medieval Africa and in debates about the transformations of Africa on the eve of the opening of the Atlantic world.⁴
- ² My interest in the question of the possible spread of the bubonic plague in Sub-Saharan Africa during the medieval pandemics first arose from archaeological investigations I led in Ghana in the early 2000s. At the site of Akrokrowa, near Abrem Berase in the KEEA (Komenda-Edina-Eguafo-Abrem) district of the Central Region, I documented a settlement surrounded by an earthwork⁵ founded in the second half of the first millennium CE and abandoned during a radiocarbon time-span centring on the 14th century.⁶ The occupational history of this settlement intrigued me. Why would a

settlement showing clear signs of continuous and stable occupation for more than half a millennium be suddenly abandoned? Even more compelling, why did such settlements surrounded by massive systems of trenches and banks disappear from Ghana's subsequent urban landscapes? This second question emerged from the reinterpretation of the chronology of a number of earthworks excavated in the late 1970s by Kiyaga-Mulindwa in the Birim Valley of southern Ghana.⁷ On the basis of the re-evaluation of the radiocarbon dates and stratigraphic details provided by this author, it became clear that the sites he excavated had been occupied and abandoned within the chronological range documented at Akrokrowa.⁸ By extension, we suggested that these converging observations reflected a larger, regional pattern; and this hypothesis is strengthened by a set of radiocarbon dates later obtained by James Boachie-Ansah from his excavations of a group of earthworks in the Asikuma-Odoben-Brakwa district of the Central Region.⁹

- 3 The 14th century consequently emerges in southern Ghana as a period of deep change, possibly of extensive turmoil. For scholars interested in comparative history and in the long-standing interconnectedness of the African continent with the rest of the Old World, the prospect of the bubonic plague being a factor in 14th-century large-scale changes in the regions neighbouring the Gulf of Guinea naturally imposed itself as worthy of further investigation. This has become a formidable intellectual and methodological challenge for students of the deep African past, and one of the three research foci of the ANR-funded GlobAfrica Project (2014–2018).¹⁰ In 2016, an interdisciplinary workshop brought together an international team of historians, archaeologists, art historians, biologists, and a linguist to share preliminary data and insights into potential theoretical and methodological avenues to address the following question: if the Second Plague Pandemic did spread to Sub-Saharan Africa, what recoverable traces of the crisis did it leave behind? This introduction and the following three papers published in this issue are direct outputs of this symposium.¹¹
- 4 Four principles guided our research agenda. We wanted to revisit available data to generate new knowledge while also creating opportunities for new materials to emerge out of previously underused sources. In addition, we thought it necessary to adopt a comparative approach to juxtapose datasets previously studied in isolation from each other, and to critically examine all material and assumptions that would seem to support or undermine our starting assumption. I need to state very clearly that at the present stage of the research, the spread of plague in Sub-Saharan Africa still stands as a working hypothesis, and it will remain so until stronger, empirical data—possibly in the form of paleomicrobiological evidence—is found to confirm, qualify, or disprove it.¹² However, the potency of this hypothesis in radically transforming the many moving parts of deep-time African historical narratives, and the increasing number of tantalizing hints supporting it, as shown in the present collection of essays, give it a character of urgency and centrality that needs to resonate in all disciplines searching the African past. We call for all research projects exploring periods encompassing the 14th to 16th century to include this hypothesis in their conceptual framework and to share evidence that may solve this conundrum.
- 5 Four papers make up this exploratory thematic dossier on the hypothetical spread of the Second Plague Pandemic in Africa, beyond Egypt and North Africa. The present introduction stands as a thought incubator and a first attempt—with no pretension to exhaustiveness—at reviewing potential evidence of the plague in different sets of

records, especially in the archaeological and the documentary record. Three specific sets of records are also the subject of a supporting essay. Marie-Laure Derat examines 13th- to 16th-century written sources from the Horn of Africa for mentions of the plague in written sources. Daphne Gallagher and Stephen Dueppen provide a preliminary discussion of archaeological evidence for change in settlement patterns in the Western Sahel and savannas, a major theme in Plague Studies. Finally, Monica Green shakes up traditional disciplinary boundaries with her historical interpretation of genetic evidence recovered from a particular strain of plague found in Eastern and Central Africa. In documenting its evolutionary derivation from post-Black Death lineages of the bacterium responsible for late medieval outbreaks in Central Eurasia, she suggests this particular strain of plague arrived in these parts of Africa no later than the 16th century.

- 6 Altogether, I think of this collection of essays as a bold attempt to open an original line of enquiry that may prove decisive in unlocking a new series of early African historical narratives. We also call for a discerning overhaul of medieval and early modern African histories and for their release from the many historiographical straightjackets elaborated around the 1960s (and ever since too often uncritically reproduced in our writings and teaching). Let us imagine, just for a moment, that the Second Plague Pandemic had as many overwhelming consequences in Sub-Saharan Africa as it had in Europe or across the Islamic world. Let us further imagine that the pandemic drastically affected the demography of the African continent, with unrecorded repercussions on land uses, urban landscapes, political economies, industrial and craft production, labour systems, and religious realms. If it did so, we have until now misread key historical events and ignored fundamental transformative processes. In fact, I know of no reason why a bacterium that broke barriers across climates, environments, and species to find its way from the confines of Asia to the western and northern margins of Europe, and to the Arabian Peninsula, the Maghreb, and Egypt, would have failed to spread across the rest of the African continent. It is therefore all too legitimate to examine whether we have misread facets of the history of late medieval Africa. Clearly, imagination does not make history, but the lack of imagination also deprives history of the critical dynamics that everywhere push its frontiers into hitherto uncharted territories. Thinking about the plague in Africa, we believe, is a way to exert our creative duty toward the discipline of African history and to reclaim early African history as a dynamic, unexpected, exciting, and renewed epistemological frontier. Knowledge, after all, is the measure of all cognitive limitations.

Looking for the plague in the archaeological record

- 7 The impacts of plague on the archaeological record are probably beyond full comprehension on a global scale. Indeed, the disease may have generated radically different—and sometimes hardly perceptible—features and traces corresponding to an astonishingly diverse array of social responses. In his groundbreaking book on the Plague of Justinian,¹³ Little listed ‘burial pits, abandoned villages, and aborted building projects’¹⁴ as finds potentially related to plague pandemics. He added that such ‘negative evidence’ was not ‘the best sort for establishing proof’.¹⁵ There is indeed a consensus among archaeologists that, as elegantly put by Stephen Mitchell, beyond the

recovery of palaeomicrobiological evidence from ancient burials, ‘it is difficult to demonstrate or refute claims about the effects of plague on the basis of archaeological evidence’.¹⁶ Clearly, our experience with the archaeology of northern Europe and the Mediterranean world indicates that mass burials as well as patterns of long- or short-term abandonment of structures, settlements, or whole areas could be expected as outcomes of the plague’s brutal decimation of communities. Such features, however, are often much less visible than is commonly imagined, and any one of them could potentially result from a variety of causes and processes other than plague. It is therefore challenging, on the basis solely of archaeology, to establish conclusively the causal relationship between an outbreak of bubonic plague and specific archaeological features, especially when written records documenting the former are not available—as is largely the case in Sub-Saharan Africa. Such difficulties, nonetheless, ought not to prevent us from exploring the hypothesis further and from gradually collecting a larger and stronger set of potential evidence, which, when added to other datasets, will contribute to making the plague theory an integral part of the historiography of Africa.

- 8 In light of Little’s statement quoted above, three consequences of plague particularly require our attention because they may have shaped the archaeological record in patterned, retrievable ways: the rapid abandonment of settlements with a previously long and stable occupational history; the provision of mass burials to cope with the excessive mortality;¹⁷ and the existence of aborted or delayed construction projects. As an extension of the latter consequence, we may add the default of maintenance of existing infrastructures, leading to their gradual or rapid decay, the decline of labour-intensive or highly skilled sectors of activities, but also—paradoxically—sudden surges of labour-intensive projects.¹⁸ We will examine these processes below in the section that focuses on shifts in labour management.
- 9 The history and archaeology of the Mediterranean basin in late antiquity and the medieval era offer relatively well-documented examples of the relationship between such events and the occurrence of plague, and a comparative approach suits our purpose. Our premise implies that in spite of the diversity of social responses, there are also quasi-mechanical, collective and individual reactions to rapid demographic disasters that cut across culture and societal organizations. If such a premise is true, then such responses are expected to be also represented in Sub-Saharan Africa.

Evidence of settlement recombination

- 10 In West Africa, a classic response to lethal epidemics during later periods was to abandon existing settlements for small, dispersed hamlets located on farmlands. Examples abound in oral traditions of such occurrences. For instance, in Abrem in southern Ghana, oral traditions recall an undated epidemic—probably a 19th century outbreak of smallpox—which caused a traumatic number of deaths in the community. Searching for spiritual causes behind such a devastating event, the community eventually came to the understanding that a group of angry souls buried nearby was the source of the premature death of its many members. Oral traditions relate the story of an angry mob visiting the burial ground and firing musket shots at graves, before cautiously deciding to move away from the dangerous spot and re-settle at some distance from the previous settlement.¹⁹

- 11 An overwhelmingly severe mortality rate would be expected to have an even more drastic impact on a regional scale, as people would be likely to flee en masse to safety, unable to provide appropriate funeral rites and to dispose of the rapidly increasing number of bodies in ways that befitted societal norms. The interpretation of exceptionally deadly pandemics as massive spiritual attacks, and the otherworldly fear caused by the expected reprisals from the large number of the dead deprived of befitting graves, would in many cases have prevented the re-settlement of the ‘polluted’, abandoned sites, creating a regional pattern of ghost cities rapidly colonized by vegetation. The digestion of such settlements by the tropical environment would have been rapid, in contrast with the enduring traces some of them would have left in the memories of the refugees. These memories, vividly recalled in oral traditions, would have been durable markers of the common identity of the displaced, despite their spatial dispersion. Almost synchronously, a new network of settlements would have emerged, with survivors regrouping and creating new social bonds. Widespread taboos on farming over burial sites would have fostered the formation of forested landscapes over the depopulated sites, and many would have become shrines. Such anthropogenic landscapes born of apocalyptic deep-past events, however, are not immediately recognizable as such in oral traditions, because they would have long been recycled with new spiritual meaning—or even reclaimed, cleared, farmed, and resettled as time moved on and so did people and political circumstances. Some ancient political centres such as Ife, when resettled, were reinvested with political leadership in the name of their former glory.²⁰ Continuities between the deep past and the present were forged and manipulated to build on powerful legacies, and memories of the crisis and hiatus in occupation were erased from traditions.
- 12 This is not to say, however, that all medieval settlements in Africa would have been abandoned. As in the Old World, some areas were probably less affected than others, for reasons that are as unclear—and probably as multiform, complex, and poorly understood—as the epidemiology of the medieval plague itself. Similarly, some regions in Africa would have been less impacted or might have found the means to deal more effectively with the contagion, on both spiritual and practical grounds. For instance, I have often wondered whether, in some parts of the Gulf of Guinea, the once common practice of keeping sacred (and rat-eating) pythons as pets in the thatch roofs of dwellings had something to do with the simple realization that people living in such compounds fared better in times of epidemics.²¹ In places, settlements may have been only temporarily abandoned, then re-occupied by survivors. In such cases, one may not recover clear stratigraphic evidence of likely brutal, yet short-term interruption in the settlement history, although a reduction in size of the occupied space would be expected.
- 13 The abandonment or downsizing of urban and rural settlements is gradually being recognized in other parts of the world in relation to the first and second pandemics of plague, despite some continuing disagreement among researchers about the interpretation of the archaeological record. In his study of the late Roman Empire, Mitchell’s survey of evidence from a growing but unequal body of archaeological works on rural and urban settlements in the eastern and western Mediterranean, northern Europe, and the Near East between 400 and 800 BCE led him to conclude that ‘the long-term impact of the plague on the population level and the social and political organization of Mediterranean and Near Eastern societies from 550 to 750 was indeed

as devastating as the immediate impact recorded by the contemporary observers'.²² From Denmark to North Africa, and from southern Gaul to Syria, archaeological observations point to some drastic population losses, with a peak between the 6th and 8th century. Among the evidence, the disappearance or alteration throughout the western Mediterranean area of villas—those dispersed, aristocratic rural centres of modest to large agricultural estates that characterized the late Roman world—are particularly intriguing. Some villas were abandoned and never re-occupied. Others later developed into cemeteries, church estate centres, less monumental buildings, or even villages. Almost none were still in existence in the 7th century. Some historians, like Wickham, have suggested such a 'substantial shift' was a marker only of cultural change and militarization of the aristocracies in the post-Roman world.²³ Others, like Mitchell, interpret this transformation as strong evidence of depopulation and impoverishment.²⁴ Indeed, even Wickham—generally so reluctant to speak of crisis—estimates at ca. 50% the population decline for the post-Roman western empire.²⁵ For instance, in an area of 5,000 ha prospected at Marne-la-Vallée, Wickham finds that only 55% of the settlements that existed in early Roman Gaul are still occupied in the early Middle Ages.²⁶ In fact, large-scale archaeological work conducted in three areas around Paris—namely, Pays de France, Marne-la-Vallée, and Melun-Sénart—show that this figure covers much variability from one area to another.²⁷ In Marne-la-Vallée, of the 25 sites identified for the 1st century, only two remain active in the 5th century; and this number gradually picks up in the 6th century to reach 12 in the 7th and 18 in the 8th century.²⁸ In comparison, the Pays de France sees the number of its identified sites increasing more than two-fold between the 1st and the 4th century (from 45 to 119), decreasing slightly in the 5th century (109), before collapsing to only six sites in the 6th century (a daunting 94.5% decrease), five in the 7th century, and even four in the 8th century. In Melun-Sénart, we pass from 12 sites in the 1st century to 20 in the next, and then a slow decline starts in the 3rd century with 18 sites identified, reaching a minimum of six in the 6th century, eight in the 7th, and seven in the 8th century. Overall, the graphs show that the population decline in Île-de-France began before the demographic crisis of the 6th–8th century and was generally accentuated by the latter, but population dynamics were never uniform, even across neighbouring areas.²⁹ Clearly, disease, if it was involved, was not the only factor at work in cutting down numbers. This realization points to the complexity of archaeological data, the idiosyncratic nature of discrete contexts, and the difficulty of disentangling the multitude of possible causes and consequences that shape the material records of human existence.

- 14 In the eastern Mediterranean and the Near East, the overall picture converges toward a similar depopulation pattern, with a turning point in about the middle of the 6th century. This is probably the case in central and western Anatolia; and in Palestine as in Arabia, many cities seem to reach their peak of population and prosperity before the later 6th century.³⁰ In northern Syria, the deserted late Roman villages in the Limestone Massif feature large, well-built, stone, multi-storey houses that were constructed by prosperous peasant proprietors mixing subsistence farming and the production of olive oil for export, principally between the mid-4th and the mid-6th century. House-building stopped after ca. 570 CE. Although the villages continued to be occupied, activity may have declined. Imported pottery is no longer found, and depopulation is possibly illustrated by the build-up of ground-floor occupation levels, suggesting lack of maintenance during the period.³¹ In truth, the

question of a plague-related demographic crisis during the 6th–8th century in the post-Roman world remains controversial—in part because of the difficulty in identifying plague archaeologically,³² but also because several factors may have combined into a ‘perfect storm’.³³ Once again, one should not underestimate the difficulty in comparing and interpreting largely inconsistent archaeological data across vast regions. Mitchell’s call for ‘a systematic and dispassionate re-evaluation of the archaeological evidence, addressed to the question of the lasting impact of the plague’³⁴ is suggestive of the difficulties that lie ahead as we ask the same question in Africa, where the archaeological record is incomparably less well documented than in the Mediterranean world, northern Europe, or the Near East.

- 15 Much less controversial is the demographic crisis that affected Europe in the wake of the Second Plague Pandemic, because of the abundance of written documents, including a wealth of quantitative sources—a major difference from the preceding pandemic of the 6th–8th century.³⁵ Recent projections based on England’s pre- and post-plague demographic estimates suggest that the first wave of plague, which surged between the Black Sea and the British Isles between 1347 and 1354, could have killed about 45% of Europeans—a number estimated at 30–36 million—in a very short span of time.³⁶ Studies show that mortality was not correlated to settlement size, and plague had a similar, deep impact on populations of rural and urban settlements. The difference, however, was in the duration of the epidemic, which closely correlated the physical and demographic scale of settlements, varying from a few weeks for villages of a few hundred inhabitants to up to 18 months for Venice, a hub of 110,000–120,000 inhabitants.³⁷
- 16 Once again, despite the extent of the demographic loss, the effects of the plague are not as visible as one would expect in the archaeological record. As Kennedy rightly stated, the Black Death is ‘virtually imperceptible in the pattern of elite building’, and Lilley noted that the development of new suburbs and even the foundation of new towns counter-intuitively characterized the period 1350–1530 in England.³⁸
- 17 Similarly, we should not expect the study of the stratigraphy and spatial and temporal distribution of African settlements to provide us with unequivocal evidence about the spread of the Black Death pandemics south of the Sahara. Nevertheless, regional or even continental overall patterns of change in settlement patterns, including synchronous abandonment or reduction in size of settlements in the 14th century after centuries of occupation and growth, would be a strong indicator of population loss leading to the widespread societal crisis that we expect took place in the wake of the deadly epidemics. Brought together with other evidence, such a pattern would contribute to strengthening our hypothesis. Beyond in-depth case studies focusing on the abandonment of discrete settlements, we must also look for radical change in the built environment and the settled landscapes, a change which I propose to call settlement recombination, a process mixing abandonment or downsizing of existing settlements and the creation or expansion of others.
- 18 In line with this argument, one of the objectives of the GlobAfrica project was to review the archaeological record for evidence of such patterns in Africa. In a continent known for its extreme diversity in people and environments, and its massive size, such a review is not an easy task. This is not the place to express frustration about the relative paucity of well-documented, comparable, accessible reports on the archaeology of Africa over the last two thousand years. Yet, we need to bear in mind the relative

inadequacy of the available material to develop a truly comparative and comprehensive approach. Such is the challenge that Daphne Gallagher and Stephen Dueppen took on with their paper entitled ‘Recognizing Plague Epidemics in the Archaeological Record of West Africa’. Aware of the daunting nature of the task ahead, they limited their inquiry to four case studies from Mali and Burkina Faso sharing key characteristics while representing a large array of different environmental settings, sizes, and connectedness. Beyond expected variations in the quality of evidence and chronological resolution, they clearly identified depopulation patterns across their sample, which, in their own words, ‘provide strong circumstantial support for the effects of a devastating disease’, with plague as the most likely candidate.³⁹

- 19 In fact, I believe there is enough accumulated circumstantial evidence to suggest the effects of plague are visible in the archaeological record of Africa, and that archaeologists need to envisage it as they plan their research designs. I also advocate for the systematic re-examination of past publications and archives of archaeological projects. What stands in front of us could be nothing less than the most significant discovery of this century for the history of the continent, one that would shake many historiographical foundations and transform many narratives. It is time to engage rigorously with this evidence, instead of limiting ourselves to stargazing soliloquy. Granted, disrupting dominant paradigms comes with its share of risks, but the would-be reward of a historiographical ‘shake’ is worth the academic peril. Gallagher and Dueppen are right to re-examine Jenné-Jeno, the reference site for the Inland Niger Delta, and to note how its widespread abandonment by 1400 CE fits the plague hypothesis.⁴⁰ As recently noted by Monica Green, Roderick McIntosh already, two decades ago, ‘mused on the possibility that plague may have been involved in Jenné-Jeno’s abandonment’,⁴¹ but no specific evidence was brought forward that would transform the hypothesis into an overwhelming paradigm. McIntosh inserted a discrete mention of plague in a later book (2005: 177), along with the horse, Islam, and a new, hegemonic spirit, as factors of change which—together with climate—would have had a role in what he called nothing short of an ‘urban shake’.⁴² McIntosh describes this process as a major shift in settlement pattern that may have begun in the 12th century CE and climaxed in the 13th and 14th centuries. It would have affected not only Jenné-Jeno but also the Inland Niger Delta as a whole:

Occupation was restructured thoroughly in the Upper Delta. Some moved to the non-inundated periphery [...], some abandoned the lower rice soils [...], and the urban population, once clustered, imploded upon fewer (larger?) cities, such as Jenne. Dia witnessed a similar reorganization [...] and the Méma was virtually abandoned by AD 1300.⁴³

- 20 It is easy to understand the prudence of archaeologists with regard to the plague hypothesis in the Inland Niger Delta, as the process qualified as ‘urban shake’ by McIntosh would have begun before the possible arrival of the plague pandemic. Certainly, we could call on Hymes’ hypothesis that the disease might have circulated earlier than generally acknowledged, but in fact there is no evidence right now that these early, pre-Black-Death strains of plague reached Africa.⁴⁴ More convincingly, however, climatic change—and more especially the aridification of the area after wetter conditions in the middle to late first millennium CE—was a long-term, powerful engine of change that could have masked different, short-term contributing factors such as the plague. It remains significant, I believe, that the regional ‘urban shake’ in the Middle Niger ended quite uniformly in the second half of the 14th century. In

conformity with Gallagher and Dueppen's findings, I suggest that in the Sahel and West African savannas, only the *terminus ante quem* of the 'urban shake' is a potential marker of the spread of the plague. In these regions, I suggest the plague pandemic imposed a brutal epilogue to a long process of adaptation of local populations to changing environmental conditions.

- 21 When we examine sites located further away from the edges of the Sahara, we expect aridification and climatic change to have had a lesser impact on human communities and, consequently, we hope to find archaeological evidence of cleaner breaks in the occupation of settlements, resulting from the assumed pandemic. The review of archaeological evidence therefore needs to enlarge its focus to include a variety of climatic and ecological contexts. Ghanaian earthworks discussed above are part of such enlargement, but new clues are also turning up in south-west Nigeria, in a series of excavations undertaken between 2015 and 2018 at Ife and Ilara-Epe as part of the first phase of the Ife-Sungbo Archaeological Project.⁴⁵ The new data is significant because it originates from some of the most humid parts of West Africa. In addition, the plague hypothesis was a part of the project design from the beginning, with participants paying particular attention to possible stratigraphic signatures of the pandemic. At this time, I can provide only a cursory presentation of this new evidence, because it is still under study. Suffice to say that we opened three trenches in different locations across distinct rammed-earth walls and ditch systems that defended Ife before the colonial era. All three had in common a stratigraphic episode characterized by a level of dark, organic soil. In two places (Ita Yemoo and Oke Atan), the dark levels were 20–30 cm thick and included a large number of charcoals and charred palm kernels.⁴⁶ In the third place (Oduduwa College II), the dark level was thinner (5–10 cm) and did not include charcoal remains.⁴⁷ The stratigraphic positions of the different dark levels were similar: all overlaid features and floors dating from the 14th century and underlaid the banks of the defensive earthworks built in the 18th and 19th centuries. Charcoals taken from the dark layers at Ita Yemoo and Oke Atan provided dates centring on the 17th and 18th centuries. On-going studies by a team of geoscientists will throw light on the genesis of these layers. In the meantime, one of the likely scenarios is that of the abandonment of Ife⁴⁸ at some point during the 14th century, triggering the regrowth of the vegetation cover, which left behind an organic horizon in an otherwise very mineral stratigraphy. Pending detailed geomorphological studies, we interpret the variations between the dark layers as differential anthropic impacts on the post-abandonment, newly constituted landscape. Whereas at Ita Yemoo and Oke Atan, farmers eventually reclaimed the land for slash-and-burn agriculture,⁴⁹ no such anthropic process took place at Oduduwa College II, where only the natural decomposition of organic matter contributed to the dark level. The prospect of a large-scale depopulation of Ife came as a shock to our team—and to many scholars of Ife we interacted with during academic events—because it goes against the general historiographical acceptance of a long-term continuity between the medieval and the modern settlements of Ife. Nevertheless, the emerging scenario has the merit of elucidating overlooked questions about the nature of the archaeological record at Ife, especially the discovery, close to the surface, of medieval prestige objects in cuprous alloys which are not commonly found by archaeologists outside very specific contexts such as burials or hoards. We believe the presence of such objects can be explained by the hurried departure of a population subjected to panic. Objects such as those found at Ita Yemoo by Frank Willett can therefore clearly be interpreted as furniture disposed on a shrine made of wood and

leaves similar to that illustrated in Ife pottery decoration. The shrines were left in situ as people died or ran for their lives. The shrines eventually rotted and collapsed, precipitating the durable furniture on the ground, where they were gradually covered by a thin layer of sediments before being rediscovered in the mid-20th century. Many of the prestigious objects found at Ife, however, were part of hoards. The collection of bronze and copper heads found in 1938 at Wunmonije Compound⁵⁰ and the group of terracotta found by Garlake at Obarala's land⁵¹ in 1971–1972 are part of such and seem to have been hidden in the ground in the hope of being retrieved after the crisis, a retrieval that eventually never came to pass. Everything in Ife's archaeological record points to a hurried abandonment at some point in the second half of the 14th century and to the dispersion of its elite. Oral traditions that have become ubiquitously known in Yoruba-speaking parts of Nigeria support this idea of a regional Ife diaspora, and I suggest that the movements of the sons of Oduduwa out of Ife are an allusion to the process of political fragmentation that affected the region after the collapse of Ife as its centre.⁵² This opens an entirely new historiographic frontier, one which is beyond the scope of this paper. It is worth noting, however, that our collapse and then fragmentation of the Ife sphere of influence in the second half of the 14th century CE is supported by archaeological work we conducted in 2016 at Ilara-Epe, on a section of the monumental Sungbo's Eredo enclosure. By dating the latter monument from the late 14th or early 15th century CE, our work supports the idea of the emancipation of Ijebu—formally a periphery of Ife's zone of influence—as an autonomous city-state in the wake of late medieval Ife's demise.⁵³ If the small windows we opened in the archaeological record of Ife and Ijebu (both in Nigeria) and of Ghanaian earthwork sites are representative of events occurring at a much larger scale, the 14th-century crisis would have been brutal, ending a sustained dynamic of growth⁵⁴ and having deep repercussions on the regional political order. As such, it would have resulted in a much clearer break in settlement histories than in the Sahel, where the process might be difficult to disentangle from a long-term dwindling of urban centres, informed primarily by environmental factors.

Evidence from burials

- 22 Mass burials are poorly represented in the known archaeological record of medieval Africa, and we currently have no equivalent on the continent of the well-documented plague pits and trenches of England, France, and Spain.⁵⁵ Such a fact cannot be interpreted, however, as evidence that bubonic plague did not spread into Sub-Saharan Africa, as it is born out of the huge gap that exists between the extent and implementation of preventive archaeological policies in Europe and those in Africa. In Europe, the imposition of legally binding, systematic archaeological investigations prior to large infrastructural projects has generated historical knowledge at an exponential rate that has little to do with the way scientific knowledge is built incrementally and painstakingly in many other parts of the Old World. In fact, many parts of the Mediterranean world with documentary sources pointing to massive mortality during the first or the second pandemics do not have the matching archaeological records in the form of mass graves.⁵⁶ Although the presence of the latter are suspected in many places, post-medieval urban transformation and massive infrastructural developments have often obliterated plague cemeteries. Furthermore, it would be misleading to think that plague pits were the only response to unusual

mortalities and, therefore, their only indicator in the archaeological record. In fact, the diversity of burial practices in times of epidemics is now clearly established by the archaeological data accumulated during the last four decades from many different contexts in Europe.⁵⁷ The result has been that archaeologists do not limit their investigations to mass burials. They define epidemic contexts through a manifold paleobiological methodology, including the examination of skeletal remains, the occurrence of simultaneous burials, the demographic anomalies among well-characterized assemblages, the study of existing documentary records, and paleomicrobiological testing.⁵⁸ Usually, it is not the presence of mass burials *stricto sensu* that matters. As noted by Henri Duday a decade ago, in the absence of textual or epigraphic data, or positive paleomicrobiological testing, the archaeological identification of an epidemic context is often only visible and therefore possible in the case of simultaneous multiple inhumation.⁵⁹ This includes double, triple, or quadruple simultaneous inhumations, which are less spectacular than plague pits but are proven indicators of possible epidemics. Less frequently, the inappropriate orientations of single graves, unusual postures of the corpses, and the use of lime can also be clear enough anomalies in medieval and post-medieval European burial traditions to call for further investigations.⁶⁰ What matters is not really the number of bodies but how the practice of burying the dead deviates from the prescribed norms in each cultural and temporal context.

- 23 In Africa, where mortuary practices were unfathomably more diverse than in Europe and in the Mediterranean world, we expect that mass graves would be only one out of many possible manifestations of mass mortality, but certainly not the standard. Although it is expected that records of mass graves will eventually emerge as more work is conducted, other discrete evidence has probably long escaped the attention of archaeologists or is simply unavailable as a consequence of the lack of published, detailed, and anthropologically informed excavations of burials.⁶¹ Christian and Muslim practices provide us with a good record of normative processes for burying the dead. In parts of Africa where such processes were not informed by Abrahamic practices, our understanding of normative versus atypical burial practices, their meaning and variations, and how they evolved through time is limited—and possibly misinformed by poor stratigraphic control, biased perceptions, and interpretations provided by local informants on the basis of modern cultural norms. There is a need to revisit the last 100 years or so of reports resulting from amateurish as well as professional excavations in Sub-Saharan Africa, in search of misinterpreted and/or long-forgotten multiple burials and mass graves, and to foster an expertise in the archaeology of death in Africa.
- 24 So far, the earliest excavation of what resembles a mass grave with 25–30 individuals, including children, was excavated by Lieutenant Desplagnes in 1901 in the tumulus site of Koï Gourrey, in modern Mali.⁶² Unfortunately, the excavation is only briefly described in a few lines, and the excavator noted that most of the human remains turned into dust as he discovered them. In spite of the poor quality of conservation of the skeletal material, he reported having collected a few bones, including an upper jaw and, perhaps, some other dental material. There is no record of the place this material was deposited, and inquiries at the Museum of Natural History of Paris, which holds other human samples collected by Desplagnes, did not yield a positive identification of this specific sample.⁶³ In spite of its small size, such a collection is potentially important because it could produce radiocarbon dates and provide teeth with intact, residual

dental pulp, allowing paleomicrobiological analysis in the search for aDNA traces of *Yersinia pestis* (*Y. pestis*), the standard analysis method used with material from European plague pits.⁶⁴

- 25 In fact, the identification of the pathogen in archaeologically recovered human remains in Sub-Saharan Africa seems to be the only way to create a consensus on the spread of *Y. pestis* in Sub-Saharan Africa, as it did for Europe in the 2000s.⁶⁵ Although promising, the extraction of ancient DNA has its own challenges, as experienced by the GlobAfrica project with the failed attempt to analyse a sample from a collection of skeletal remains currently housed by the Department of Archaeology and Anthropology at the University of Ibadan and originating from the only known medieval mass grave in tropical Africa, excavated by Graham Connah in the 1960s at Benin City.⁶⁶ More specifically, the remains come from the Clerks Quarters, cutting II, feature 21, corresponding to a well-like cistern where a mass burial was discovered at about 14 m below the surface. It formed a mass of human bones that ‘completely plugged the cistern for a depth varying between 3 ft. 9 in. and 4 ft. 8 in’.⁶⁷ Four radiocarbon dates directly associated with feature 21 were provided by Connah in his 1975 monograph. Two originate from charcoals found associated with the human remains (N-377 and I-2722), and two others were obtained from pieces of Iroko wood that were found at different levels in the fill above the mass burial (N-376 and I-3622).⁶⁸ On the basis of these dates and their interpretation in the context of his stratigraphic observations, Connah proposed that events leading to the mass burial unfolded by the mid-13th century, a chronology that would seem to exclude the Second Plague Pandemic as a factor.⁶⁹ New tools that were unavailable in the 1970s, however, allow us to recalibrate these radiocarbon dates and refine Connah’s conclusions. To recalibrate the dates, we first retrieved the published, original Conventional Radiocarbon Age expressed in BP (Before Present = CE 1950 by convention) published in *Radiocarbon* by RIKEN, Tokyo (lab code N). The dates processed by Teledyne Isotopes (lab code I), however, were not published in *Radiocarbon*.⁷⁰ We therefore assumed that the dates Connah published were equivalent to modern Conventional Radiocarbon Age expressed in AD (Anno Domini), and we converted them to BP. We then processed them using the CALIB online Radiocarbon Calibration Program (version 7.10). Results are expressed in Table 1.

Table 1: Radiocarbon dates associated with Clerks Quarters, cutting II, feature 21, Benin City

Lab reference	Dates published	Conventional Radiocarbon Age	1-Sigma calibrated date with relative area (68.3% probability)	2-Sigma calibrated dates with relative area (95.4% probability)
N-377 ⁷¹	AD 1180±105	BP 770±105	AD 1058–1065 (1.9%) AD 1154–1306 (90.1%) AD 1363–1385 (8.1%)	AD 1034–1330 (88.6%) AD 1339–1397 (11.4%)
I-2722	AD 1310±90	BP 640±90	AD 1282–1332 (45.1%) AD 1337–1398 (54.9%)	AD 1222–1438 (100%)
N-376 ⁷²	AD 1230±105	BP 720±105	AD 1209–1325 (72.9%) AD 1344–1394 (27.1%)	AD 1046–1092 (4.6%) AD 1121–1140 (1.7%) AD 1147–1424 (93.7%)

I-3622	AD 1490±90	BP 460±100	AD 1325–1344 (7.1%) AD 1394–1521 (74.7%) AD 1575–1585 (3.3%) AD 1590–1625 (14.9%)	AD 1298–1371 (14.6%) AD 1378–1645 (85.4%)
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- 26 As evident in Table 1, the recalibrated dates are not all consistent with a mid-13th-century date (see I-3622), whereas they are all compatible with an event unfolding during the second half of the 14th-century, as we would expect from charcoal and wood samples contemporary with the Second Plague Pandemic. Similarly, Connah's interpretation of this mass burial as a human sacrificial deposit needs to be taken with extreme caution, as based on observations of European travellers and British colonial officers in the late 19th century.⁷³ In fact, the absence of physical trauma on skeletal remains, their taphonomic position, and the stratigraphic occurrences carefully recorded by Connah all plead in favour of a catastrophic event not unlike what a plague pandemic would trigger. Even in the absence of microbiological evidence, I believe feature 21 encapsulates a powerful statement about the impact of plague in Africa and up to the coastal polities of the Gulf of Guinea.⁷⁴
- 27 It is hoped that more testable evidence will emerge from various regions of Africa as new medieval mass graves are unearthed, sequencing of ancient DNA becomes more efficient, and old evidence is critically re-examined. In the meantime, we need more samples coming from a variety of multiple burials to be tested for the presence of *Y. pestis* and other pathogens. Even one positive result from a well-controlled excavation could turn the tables and give more centrality to what remains today a marginal hypothesis. To do so efficiently while respecting best practices, we will need to promote national and international collaboration, refine protocols, and share information between teams involved in the search. My experience in the framework of the GlobAfrica project suggests this is easier written about than done, as ancient DNA and genomic research drains large budgets and sometimes generates mutually exclusive academic interests.
- 28 Finally, another promising field of inquiry is the study of changes in burial practices before and after the 14th century throughout the continent, especially among societies burying their elite members with accompanying victims. If, as we posit, the plague provoked a demographic crisis in Sub-Saharan Africa and a large-scale social reorganization, we hypothesize that the practice of including accompanying victims in such burials may have become less widespread because socially too costly—unless the practice thrived, as the consumption of scarce resources is often an ultimate expression of individual and corporate wealth.⁷⁵ This could explain the rupture documented in the 14th century in the funerary practices in the Senegambia region, in relation to the use of megalithic circles and tumuli.⁷⁶

Evidence from infrastructure decay, fluctuation in industrial activities, and labour-intensive projects

- 29 In many Italian cities of the post-Justinian plague era, public buildings were in decay, paved streets were abandoned, and 'signs of a generalized lack of control' or 'planning crisis' can be identified, as sewage-disposal and street-cleaning were abandoned,

resulting in the increased accumulation of levels of archaeological deposits.⁷⁷ Medieval Egypt offers a spectacular example of plague-related infrastructure decay, in the form of the collapse of its massive irrigation system made of different types of canals, dams, and retention basins surrounded by large earthworks.⁷⁸ Comparing data derived from the pre-plague survey of 1315 and another conducted in 1528 under Ottoman rule, Borsch concluded that Egypt's irrigation system performance decayed more than 60%, largely as a direct outcome of recurrent rural mortality from plague outbreaks.⁷⁹ Applied to the archaeological data of Sub-Saharan Africa, this means we need to pay more attention to change in the stratigraphic column, to gaps in the maintenance of buildings and monuments, and to the fluctuations in scale of labour-intensive activities. In south-western Nigeria and other parts of the Yoruba-speaking world, we also need to pay attention to the chronology of the use of pavements to cover surfaces outside buildings. Observations made in Ife suggest that the generalization of the use of pavements came as a relatively late occurrence in the urban development of Ife, taking place possibly in the 13th century. By the time plague may have struck the city, well-built pavements were ubiquitous in town, and many have survived to this day. When Ife's urban life resumed about two centuries later, pavements were no longer part of the urban surfacing techniques. Could the abandonment of pavements as a key element of urban landscapes, or changes in paving practices in the Yoruba-speaking world, be an indirect indicator of the spread of plague?

- 30 The decay of rural infrastructures is another potential marker of the plague. The case of Egypt is certainly unique in Africa for the size and complexity of its irrigation infrastructure, but there are less spectacular yet labour-intensive, recurrent rural infrastructures that might have been affected by a demographic crisis. In a pioneering article, Ohadike showed how the 'Spanish Flu' pandemic of 1918–1919 was one of the main causes behind the severe labour shortage that struck farming communities engaged in the cultivation of yam, the preferred but labour-hungry crop. Acute labour crisis in the agricultural sector, he suggested, triggered a food shortage due to the collapse of yam production. In response, cassava, a crop much less demanding in farm hands but hitherto rejected, rapidly emerged as a valuable supplementary crop. This resulted in the partial displacement of yam and the enduring adoption of cassava as a quasi-staple food in Igboland.⁸⁰ From an archaeological perspective, this transition also meant changes in agricultural practices and infrastructures. The production of yam is uniquely labour-intensive because the soil must be tilled deeply to grow new plants, then mounded to receive the root cuttings, especially where the soil is shallow. This results in unique landscape forms, which less-demanding cassava does not require. Such fluctuations in land use and human intervention on the landscape might also have occurred in the wake of the plague pandemic, together with the reforestation of abandoned fields. Some crops might have become more popular while others declined, and some wild plants might also have found a new place in diets and medicinal practices. It is therefore useful to scrutinize the archaeological record for anthrosols, paleobotanical remains, and also large-scale, abandoned anthropic landscapes such as the pre-colonial raised-field systems—which cover thousands of hectares in Gabon, the Republic of Congo, and the Democratic Republic of Congo—and the *Iya* landscapes of southern Nigeria.⁸¹
- 31 While the correlation between demographic crisis and infrastructure decay flatters our common sense, we should be aware that radically different outcomes are also possible,

because different political economies struck by the same calamity may follow distinct paths toward recovery, which in turn could produce a broad spectrum of aftermaths. Borsch's comparison of Egypt and England's socioeconomic evolution in the wake of the Black Death provides an excellent example of the variability of processes triggered by depopulation. By the late 1400s, England had adapted its economic structure to massive population loss, landlords were compelled to submit to the market forces of supply and demand, and the country was already on the way to economic recovery. In Egypt, the Mamluk landholding elite resisted the market force of supply and demand and imposed what Borsch called a 'counter-market correction' that aimed to maintain its previous level of revenues extracted from its now much-reduced population. Using physical force and coercion, they artificially organized the scarcity of arable land to enforce a drop in wages and an increase in grain prices, together with higher taxes and rents. In doing so, they sapped the rural driving force that was the source of the long-standing wealth of Egypt and plunged the economy in a long-term, downward-spiralling collapse.⁸² Borsch's study is important, because it reminds us that the socio-political and economic outcomes of plague are not always predictable. If given the chance, elites tend to apply corrective force and use coercion to take advantage of a catastrophic event for opportunistic gain. In Sub-Saharan Africa, we expect such counter-intuitive processes to have taken place, alongside others that were more in line with our expectations. For example, Ijebu and neighbouring polities of the Bight of Benin that gained or regained sovereignty after the fall of Ife present archaeological features unexpected in a context of labour scarcity in the later 14th century. Excavations of Sungbo's Eredo—the monumental 160-km-long trench system that marked the boundaries of the core of the Ijebu polity—showed it was constructed between the last decade of the 14th century and the first decade of the 15th century. At a period when labour would have been scarce, the Ijebu elite managed to mobilize, settle, and control populations that were in charge of cutting deep in the land the causeway that proclaimed the autonomy, wealth in people, and power of their newly established or reinstated polity. Opportunities and coercion are probably the two keywords that can help us to reconcile Ijebu's archaeology with the broader plague narrative. In a forthcoming co-authored article, we suggest that the plague offered opportunities for elite groups to carve their own polities out of the territories formerly claimed by Ife. Coercion, probably in the form of military force, was the method used by the emerging elite to take control of communities they resettled within the boundaries of the lands they claimed. Plague was indeed a calamity for most; but for some, it offered opportunities that left behind unexpected traces of resilience and post-plague reorganization of human resources.⁸³ Monumentality can therefore be a twisted manifestation of scarcity.

- 32 Finally, I would like to raise the question of the non-transmission of some scientific and technical knowledge from the 14th to the 15th century, as suggested in the archaeological record. The example of the Ife glass production illustrates perfectly this cul-de-sac effect that seems to characterize the 14th century. It has now been vigorously demonstrated that Ife was a centre of production of glass beads during the first half of the second millennium CE.⁸⁴ Most of the raw materials used in the production of glass beads were procured locally and mixed according to a unique recipe that gives the beads their particular chemical signature. Accordingly, the glass produced at Ife was different from that of other types of glass produced in other parts of the world.⁸⁵ At least one ancient workshop is known at Ife, at Igbo Olokun, and recent

analyses suggest other ones might have been active at some point in Ife's development.

⁸⁶ The production of beads at Ife seems to have taken industrial proportions. They probably entered the long-distance trade networks as prestige items and seem to have been consumed by the local elite as a status marker. Recent publications have emphasized the complexity of the scientific and technical knowledge implied in the chain of production of glass beads: the selection and preparation of raw materials; the making of special, refractory clay crucibles able to withstand temperatures of about 1500°C; the mixing of the raw material in precise quantities to obtain specific qualities and colours of glass; the obtainment and control of precise thermal conditions; and the incorporation of the bubble of air and the subsequent drawing of the glass into perforated canes of different diameters, ready to be cut into individual beads.⁸⁷ Who mastered this knowledge and controlled its reproduction? How was the knowledge spread among the different participants in the production process? Today, we are unable to respond to these questions, but we can safely postulate that knowledge was protected by secrecy and strictly controlled and regulated by a very limited number of individuals. While the preservation of the technological process was deemed desirable by its recipients, the transmission of the same could be challenged by a highly lethal epidemic such as plague, which could kill entire compounds in a matter of days. This, I suggest, may explain the demise of the production of drawn beads, including the famous dichroic glass beads, which had probably been a major industrial contributor to Ife's regional wealth, power, and fame. Historians and archaeologists have long entertained the idea that African glass-making had yielded to the overwhelming dumping of European glass beads during the Atlantic age. This idea was supported by the misunderstanding of classical Ife's chronology and depositional history, which was—and still is—extended to the 15th and even 16th century and therefore was thought to intersect with the opening chapters of the Atlantic trade. In fact, we now believe classical Ife, as a city, did not live to hear of the arrival of even the first European vessels in the Bight of Benin. The Ife glass bead industry had died out several generations before the Portuguese arrival, probably a collateral victim of the plague pandemic. The prestige of Ife glass beads, however, remained strongly impressed in the consciousness of the elite living on the Gulf of Guinea. Beads long remained markers of social status, and the demise of its centre of production must have boosted the value of those that continued to circulate, being dug up from ancient sites.⁸⁸ Such beads eventually ended up in the hands of the Portuguese sailors and their Italian partners in trade. Early travel accounts mention African beads from the Bight of Benin as a profitable object of trade from coast to coast.⁸⁹ The strong regional demand for the glass beads and the realization of the very large profits that could be made in their trade must have aroused business interest among European traders. In a reversal of the classic historiographic narrative, I suggest that some of these old Ife beads may have found their way to Europe to serve as prototypes for some of the trade beads which were then mass-produced and re-injected into the African markets and beyond, throughout the Atlantic world. European glass beads therefore did not smother the West African glass industry; rather, they took advantage of the strong local demand that followed its demise to penetrate this lucrative market. That they did so by reproducing types, shapes, colours, and decorations that had been popularized by Ife beads is an intriguing and, once again, counter-intuitive hypothesis that will need to be investigated further. If this hypothesis was confirmed, we would find ourselves confronted by the fascinating idea that the early Atlantic market was, in part, shaped

by the enduring shock-wave born out of a global pandemic—an idea rude enough to challenge historians in their comfortable certitudes that Africa gives diseases to the rest of the world but receives hardly any.

Looking for plague in the documentary records

- 33 If we accept the idea that the plague could have shaped the archaeological record, we also need to concurrently contemplate the possibility that the same pandemics would have had an impact on the written records. In medieval Europe, Western and Eastern Asia, North Africa and Egypt, the plague left literary and non-literary traces in a number of documents that historians use as sources to produce historical narratives about life and death during and after the pandemics. Such written material is relatively abundant and the subject of a vast and rapidly expanding scholarship. For instance, the plague shaped the medieval written record by increasing the number of deaths recorded in registers in times of epidemics; by generating a specific corpus of religious, medical, and political works discussing it; and even by inspiring new literary genres.⁹⁰ Plague may also have shaped the record by causing the death of many practitioners of the art of writing, thus producing immeasurable rifts in the transmission and generation of the written record as we have inherited it.⁹¹
- 34 In Sub-Saharan Africa, to the best of my knowledge, there seems to be no text or epigraphic inscription describing the occurrence of plague in the second half of the 14th century. There is also no mention of a devastating epidemic in later accounts produced by European travellers who came into contact with these parts in the 15th century. What can we make out of such a loud silence? Should an argument from silence be built against the occurrence of the Second Plague Pandemic in Sub-Saharan Africa? Or could the plague, on the contrary, have contributed in some ways to generating such a void in written evidence? Could we also have missed textual evidence in unknown bodies of works or misinterpreted existing ones? Or are we expecting too much from the documentary record because our imaginations are trapped in our experience of literate societies? There is no definitive answer to such questions yet, but the second part of this essay is geared toward a reassessment of the written record, to search for new evidence or to interpret the lack of it.
- 35 In fact, in Sub-Saharan Africa, written records, external and internal, pertaining to the second half of the 14th century are extremely sparse and limited to a number of very specific areas directly south of the Sahara or close to the shores of the Red Sea and the Indian Ocean. Our project aims to review these different source materials, beginning with medieval Arabic sources pertaining to the Western and Central Sahel.

Assessing the silence of the external Arabic sources on Western and Central Sahel

- 36 In the Western and Central Sahel, with the possible exception of the writings of Ibn Battuta discussed below, all known written material pertaining to the 14th century was written in Arabic by scholars who had not travelled across the Sahara and were recycling earlier writings or compiling oral information. Most of this information pertained to the first half of the 14th century, and passages referring to the period 1348–1400 are few and far between. Among them are the writings by Ibn Khaldun in

Kitab al-'Ibar, where he provided historical information about the Empire of Mali up to the 1390s—although, in the most used French and English translated editions, the period following the death of Mansa Musa (allegedly in the early 1330s) is limited to a couple of pages only, and all of these pages are concerned with dynastic succession and political crises.⁹² Ibn Khaldun, who had lost his parents to the plague, is often quoted for a sorrowful description of the disease he included in his *Muḳaddima* ('prolegomena') of *Kitab al-'Ibar*:

In the middle of the eighth century [14th CE], civilization both in the East and the West was visited by a destructive plague which devastated nations and caused populations to vanish. [...] Cities and buildings were laid waste, [...] settlements and mansions became empty, dynasties and tribes grew weak. The entire inhabited world changed [...].⁹³

- 37 There is no doubt that the region we know as the Sahel was part of the 'inhabited world' known to Ibn Khaldun. In fact, we could argue that if he had known that this region had not been afflicted by the disease, such a remarkable case would have been mentioned in his writings. The fact that the disease is absent from Ibn Khaldun's short description of the Bilad al-Sudan can hardly be interpreted as evidence it did not occur there. On the contrary, one could argue that its absence actually signalled the fact that this region was no exception and joined in the painful fate of the rest of the 'inhabited world'. Alternatively—and, I should say, more convincingly—we could argue that plague is not mentioned in reference to Sub-Saharan Africa because, in Ibn Khaldun's political philosophy, plague was a by-product of civilization and urbanization,⁹⁴ which therefore could not have affected societies he considered uncivilized, and increasingly so as one travelled away from the temperate climates. In fact, as noted by Mahmood Mamdani, Ibn Khaldun was not familiar with societies of Sub-Saharan Africa, which he knew only through sources he took for granted—in contradiction to his advice to readers to subject all sources to critical scrutiny.⁹⁵ When reading Ibn Khaldun, one must always be aware of the fact that his historical discourse is less grounded in facts than in a theoretical framework, which Fleisher once called 'Ibn Khaldunism'.⁹⁶ African societies and people were not a domain of inquiry for Ibn Khaldun, only occasional buttresses called on in support of his theory of civilization, time, and climate.
- 38 Another famous traveller and writer from the medieval Islamic world is Ibn Battuta. He is particularly known for his *Tuḥfat al-nuẓẓār fī ǧarā'ib al-amṣār wa 'aǧā'ib al-asfār*, usually referred to as the *riḥla*, an important 14th century compilation of information, experiences, and clichés he gathered about the four corners of the known world, from China to the Western Sudan. His travel accounts were written down by Ibn Juzayy, a scribe working at the court of the Marinid rulers at Fez who should also be considered as a contributor to the travel account.⁹⁷ According to Ibn Battuta, the last of his many peregrinations led him across the Sahara to the Bilad al-Sudan in 1352–1353. Departing from Marrakesh, he tells us he travelled with the Marinid Sultan Abu Inan to Fez, before leaving for a long journey to the capital of Mali, the dominant polity in Western Sahel at the time, through Sijilmasa. After spending eight months at Mali's capital, he came back to Fez through the Aïr.⁹⁸ Although the disease is known to have deeply affected Egypt and North Africa during the same period, the text does not report any particular concern with it on the southern side of the Sahara. In the *riḥla*, caravans come and go and it is business as usual. Did Ibn Battuta travel before the plague started affecting West Africa? Did the Sahara shield the area from the spread of the pandemics? Or did Ibn Battuta never actually travel to the Sudan, contrary to his claims?

39 As we assess all the available Arabic sources for the period, in relation to our central topic, we need to evaluate these different explanations. It may seem intriguing, perhaps wayward, to question the validity of a source that has been erected as a central pillar of the modern historiography of medieval West Africa; but there is in fact a convincing body of scholarship pressing us to do so, as many discrepancies and imaginary descriptions are pinpointed on the map of Ibn Battuta's alleged journeys to the margins of the Muslim world.⁹⁹ Before 2017, with one important exception published in a short-lived journal—an exception which unfortunately remained largely ignored or glossed over by most historians of medieval Africa—the travels to Sub-Saharan Africa had remained outside the scope of such critical inquiries.¹⁰⁰ The situation is now ripe for a change, following the new scholarly work produced by Hadrien Collet in his 2017 doctoral dissertation.¹⁰¹ Collet dedicated a full chapter of his dissertation to analysing the part of the *rihla* that refers to the journey of Ibn Battuta to Mali; and, to date, it is by far the best attempt to historicize this travel account and discuss its nature as a source.

40 Until recently, historians settled for an utilitarian reading of Ibn Battuta's account, trying to sort out authenticity from fabrication;¹⁰² so did one of his early critics, Stephen Janicsek, a pioneering critic of Ibn Battuta's work.¹⁰³ In a critical assessment of the traveller's claim to have visited Bulghār,¹⁰⁴ he recognizes some of the reasons why Ibn Battuta's work has so durably resisted historical critique:

Everyone who has traced out his journey step by step must agree that there are serious arguments against the trustworthiness of his statements regarding several of the cities which he claimed to have visited. On the other hand, it is exceedingly difficult to substantiate the suspicions thus aroused. He was a skillful narrator, and did not himself, as is well known, write down the record of his journeys; consequently the existence of one or two errors in his account of a city or a district does not prove anything against him, since it must be allowed that his memory occasionally played him false.¹⁰⁵

41 In fact, Janicsek suggested that Ibn Battuta had made only part of the journey to Bulghār but that he had not gone beyond Bish Dagħ, where he would have gathered information without enduring the travel fatigue and risks:

If we study the whole narrative of his travels, we see that his principal intention in undertaking them was to visit all the countries of the earth inhabited by Muslims. Probably he had heard, or had read in the works of Ibn Faḍlān, Iṣṭakhri, Ibn Ḥauqal, or other writers, that at that time the most northerly city inhabited by Muhammadans was Bulghār. In consequence of this I suppose that he was very eager to visit this famous city, and on reaching the camp at Bish Dagħ he proposed to do so. But when he heard that it was so far away, instead of going to Bulghār in person, he preferred to write or dictate his trip to Bulghār as if he had actually accomplished it.¹⁰⁶

42 This might have been a strategy used by Ibn Battuta in most of his travels: finding one or several settlements with hospitable hosts and a good vantage point on a particular region of interest, ideally a trading crossroad on long-distance routes.

43 Today, it seems that any reader of Ibn Battuta's account of his journey to Mali,¹⁰⁷ with so much as the beginning of an understanding of historical criticism, would find a certain appeal in Janicsek's thought process and would want to see how it would apply to the African part of the voyages. Ibn Battuta is elusive, perpetually punctuating vagueness with anecdotes in which he is wrongly mistreated or rightly praised for what he claims to be: a hero whose mission is to carry high the project of narrating the

‘reality and unity’ of the Muslim world,¹⁰⁸ or perhaps to offer this world to the imperial ambitions of his patron, the Marinid Sultan Abu Inan.¹⁰⁹ It is the anecdotes that provide the glossy allure of a firsthand, personal account. The rest is characterized by repetitive sequences of facts behind which one can perceive the shadows of a loose questionnaire,¹¹⁰ which must have been administered to informants and functions as if to fill the gap between the precious anecdotes. Ibn Battuta achieves the rare combination, for a traveller, of being both everywhere and nowhere. Imagining that his narrative was based on material he would have gathered from a limited number of vantage points over a broader region would certainly help our understanding of the text. Walata could have been such a place, and possibly Timbuktu or Takedda. Yet, it is rather puzzling that before the 2000s, the travels to Mali maintained their reputation among historians and archaeologists of Africa as a faithful reflection of a real journey. As noted by Collet, this fact translated into a ‘rush toward rather unhappy toponymical interpretative adventures’, penned by renowned scholars.¹¹¹

- 44 A positivist, critical reading of Ibn Battuta’s work can cast doubts on the authenticity of some of its parts. It helps dismantle long-entrenched convictions, but it does not substitute for them with new meanings that would take into account the complexity of *riḥla* as a genre, or the overall intellectual and literary reasoning behind a project such as the *Tuḥfat al-nuẓẓār*. Where next to go after we realize that Ibn Battuta / Ibn Juzayy’s *Tuḥfat al-nuẓẓār* is a patchwork of cleverly assembled excerpts of previous writings,¹¹² literary *a posteriori* transcriptions of personal observations from limited vantage points, and hearsays that were not always recognized as such? In particular, what to do with Ibn Battuta’s description of Mali? Such is the challenge that Fauvelle-Aymar and Hirsch and, more recently, Collet contributed to address.¹¹³ When the best methods of historical criticism are applied to this text as a whole, interrogating its genre, rhythm, context, and narrative structure; examining in detail the internal evidence and contradictions; exploring the identity and historical traces left behind by the Muslim scholars and merchants Ibn Battuta claims to have met—one perceives more clearly the limits of simply looking for truths vs. fabrications, or observations vs. speculations.
- 45 Whereas Janicsek’s idea of a partial journey finds an echo in Fauvelle-Aymar and Hirsch’s 2003 article, they further suggested there were deeper—topological and dialogical—levels of interpretation, or perhaps higher purposes for the work, that needed to be taken into consideration to fully appreciate the *riḥla sūdāniyya* as a potential source of history.¹¹⁴ In his recent dissertation on the Sultanate of Mali, Collet reopened a case that had been cold for too long. He carefully re-examined the different positivist options—Ibn Battuta went or did not go to Mali—to conclude that the question of authenticity is key only if we consider the work as a mere travel account (*riḥla*), whereas the text is a literary project aimed at a balanced description of wonders (cities, sultans, ceremonies, etc.) and marvels (organized in five categories), as suggested by the full title of the work: ‘Gift from those who observe the wonders of the great cities and the marvels of travelling’.¹¹⁵ Collet does not solve the question of authenticity, but his reframing and contextualization of the project¹¹⁶ helps us to understand that the absence or presence of a particular item in the *riḥla sūdāniyya*, should not be seen as evidence of its actual presence or absence in the tangible reality of the Western Sudan in the mid-14th century. The overall project piloted by Ibn Juzayy was not to be faithful to the reality of the world, but to transform travel experiences into a literary work that met particular intellectual standards, as well as political and religious expectations, through a well-structured statement about the centrality of

Islamic civilization in a world otherwise defined by its savage and dark margins. In this context, it is not easy to give meaning to the absence of any mention of plague in the *riḥla sūdāniyya*, and we can measure the relative vacuity of the questions we asked at the beginning of this immersion into Ibn Battuta and Ibn Juzayy's world.¹¹⁷ Yet, if we agree that the work includes original material that could have been observed by the traveller or, at the least, collected from other wanderers from some vantage points in the southern Sahara, we need not dismiss his silence as irrelevant. Looking at the fact that Ibn Battuta did mention the disease in other places he claimed to have travelled to, the absence of any mention in the southern margins of the Sahara suggest that he did not observe or hear about the disease in that part of the world.¹¹⁸ It follows that we can hypothesize that in the years 1352–1353, when Ibn Battuta was in or near Mali, the disease had not yet spread to the Western Sudan. This implies the presence of some initial obstacles to the spread of the Black Death to western Sub-Saharan Africa, or the slower pace at which the pathogen was able to establish itself in the area.¹¹⁹ This resistance, I hypothesize, could have been breached in the following decades.¹²⁰ At this point, only the recovery of genomic material of the pathogen in West Africa would enable us to test this hypothesis. In the meantime, after Collet, let us accept Ibn Battuta's travels as an intriguing and complex source worth the inquiry—and no more as a historiographical lifeline¹²¹ worth the fetishism.

Ethiopian material for the history of diseases

- 46 Beyond external Arabic sources, the other major corpus of written sources about 14th-century Sub-Saharan Africa was composed in Ethiopia. Compared with the previous set, these sources present the advantage of having been internally generated. In this issue of *Afriques*, Marie-Laure Derat presents the results of her study of Ethiopian material for clues about the medieval plague and its later recurrences. Ethiopian sources regularly mention deadly epidemics and report on the death of large numbers of victims. In an earlier version of this paper written a couple of years ago, I pointed out a chronicle written in the 15th century, which included mention of an epidemic that had wiped out 'such a large number of people that no one was left to bury the dead'.¹²² I noted this was a well-known figure of speech, commonly associated with the bubonic plague in the documents pertaining to the first and second pandemics around the Mediterranean,¹²³ but I added that it was not possible on the basis of this sole piece of evidence to conclude about the nature of the disease referred to in the chronicle. At that point, there was no scholarly work on the history of plague in Ethiopia but only a very broad synthesis on the history of subsistence and epidemics, published by Richard Pankhurst.¹²⁴ Today, after reading Derat's paper, the situation looks very different. Not only was she able to find mentions of possible outbreaks of plague in north-eastern Africa, but she confirmed that hagiographies were a very promising source for the study of epidemics in Ethiopia, as references to diseases and epidemics that affected Ethiopian society abound in the lives of Ethiopian saints, a major genre among available written sources. In the hagiography of Saint Zena Maryam, a 14th century Ethiopian nun whose biography was probably written in the 15th century, mention is made of an epidemic that led to the death of her brother and threatened the life of her ecclesiastic masters and of members of the royal entourage.¹²⁵ Many saints are portrayed as healers and thaumaturges, and their hagiographies bring hope to the sick patients who listen to them. These documents, until now largely unexploited from the

perspective of medical history, form an important corpus to question the reoccurrence of epidemics and their typology, as well as the practices related to them (medical protocols, burial practices, quarantine of infected people). On the question of the typology, Derat's discussion of the choice of terms used in Ge'ez to name the disease points toward the term *bedbed*—often adjoined to an epithet evoking fever, suffering, or death—as the word used for bubonic plague. The fact that this word traces its etymology to the Syriac might point to its introduction from the Near East, although, in this latter language, plague was rendered as *mawtānā* (a general term for mortality) or as *shar'ūtā* (a more precise term referring to the presence of buboes).¹²⁶ The Ge'ez construction *mota bedbed* (lit. 'death that brings disorder') seems to echo the Syriac *mawtānā rabbā* (lit. 'great mortality'). Although Derat did not identify in Ge'ez a specialized term equivalent to the Syriac *shar'ūtā* or to the Arabic *ṭā'ūn*, she convincingly shows that the word *bedbed* was specifically associated with what seems to be plague, characterized not by its symptomology but by its deleterious impact on social order.

- 47 One of the many strengths of Derat's study is to pinpoint a number of possible crises from the 13th to the 15th century, associated in the sources with the terms *ṭā'ūn* or *bedbed*, and their concomitance with similar events unfolding in Egypt and in the Arabian Peninsula. Even when the plague events are not directly documented in the sources, their impact on societies is such that they commanded social responses which, sometimes, are still discernible in the documentation to someone who is looking for them. In the Ethiopian case, the creation of an institution in charge of burying the dead left behind by communities fleeing mortal outbreaks, the consecration of new churches to seek the protection of local saints, the rise of a religious discourse associating demons with plague, and the reliance on magical and talismanic protection—all seem to be indirect evidence for the durable presence of plague in Ethiopia during the late medieval and early modern periods. Nothing, however, is more convincing than the importation into Ethiopia of European plague saints, particularly Saint Roch, whose cult had spread quickly in the 15th century from northern Italy to most of the Christian shores of the Mediterranean, and beyond.¹²⁷
- 48 Ethiopian sources are particularly interesting because of their relative abundance at a period when almost no internally generated written sources are available elsewhere on the African continent. The situation changes, however, after the 16th century with a growing number of documents being written by West African scholars, in Arabic and Ajami. In the next section, I will discuss why we find it important to integrate this category of sources in our project.

Sudanic chronicles and other internally written sources in West Africa

- 49 In Western Europe, bubonic plague may have been first reported in 1346, but it is usually understood that the disease remained present until at least the 1720s, perhaps later;¹²⁸ and fatal outbreaks in London in the 17th century and in Marseille in the 18th century reminded communities of the persistence of the disease, several centuries after it had first struck. In Russia, the Ottoman Empire, and Egypt, plague remained a major concern through the 19th century. On the basis of the Eurasiatic experience of the disease, we have reasons to believe that plague might also have persisted in Sub-

Saharan Africa, at least in some areas where the pathogen encountered suitable conditions.¹²⁹ We must therefore expect mentions of plague outbreaks after the 14th century and interpret them as retroactive evidence of the initial spread of the disease.

- 50 Mention of such recurring outbreaks appears in a number of manuscripts pertaining to the pre-19th-century history of West Africa, written and/or copied between the 16th and the 20th century, especially in the form of chronicles, king lists, and transcriptions of oral traditions.¹³⁰ The song of Bagauda, published by Mervyn Hiskett in 1965, is such a document.¹³¹ The poem in Hausa contains a king list which is thought to be ‘a very old tradition’.¹³² One verse was added at the death of each king of Kano (*sarki*), at least until the 1920s. Together, the poem, which was recorded in Ajami by Hausa Islamic scholars, seems to have been a popular song and, at the same time, a history lesson that was shared by the inhabitants of Kano. Besides the names of kings, marking events were also recorded, and although its internal chronology presents some difficulties, the comparison of the king list with that of the Kano Chronicle provides a historical thread starting in the 11th century. Some verses are relevant for this essay, as they mention a plague:

[...]

Four months Sheshe was on his throne;
There was a plague, and the people were dropping dead.
It cut down Sheshe. Yakubu, his younger brother succeeded.
Then after six months came death, the buffeter.

[...] ¹³³

- 51 The verse suggests a dynastic crisis as the plague kills at least two kings in a six-month interval, at a period that Hiskett situates between 1373, on the basis of the internal chronology of the Song of Bagauda, and ca. 1582, on the basis of the Chronicle of Kano.¹³⁴ The words used suggest a great mortality that lasted several months and succeeded in marking the memories of the inhabitants of Kano. The Hausa word for plague, (*a*)*loba* or *anoba*, is probably derived from the Arabic *waba*’ and is evocative of a great mortality. Could this be an oral recording of an outbreak of bubonic plague? The editor of the song thought it possible.¹³⁵
- 52 Groundbreaking research was conducted in the late 1960s by historian Sekene-Mody Cissoko, in a study of floods, famines, and epidemics—a triad that is very familiar to the student of the first and second pandemics of plague—in Timbuktu and in the Inland Niger Delta.¹³⁶ In an attempt to build a first chronology of the different catastrophic events, he used some of the classic 17th- and 18th-century Sudanese chronicles, including the *Tarikh al-Sudan*, the *Tarikh al-fattash*, and the *Tedzkiret en Nisian fi Akhbâr molouk ès Soudan*, in their French translations established by Houdan in the beginning of the 20th century. Unfortunately, the path he opened was not followed by others and his work remained in isolation. Since 1968, a number of other West African chronicles, king lists, and a variety of important sources (written and oral) have been edited and published in translation, although of various quality. There is a need to go back to the original sources and work with the experts in this material to create meaning, as illustrated by the work conducted by Derat with Ethiopian documentary sources. Among recently available material, I note the recent edition in Arabic of the *Book of the Cure of Internal and External Diseases Affecting the Body*,¹³⁷ authored by Sayyid Aḥmad al-Raqqād (or al-Raqqādī) al-Kuntī before 1684.¹³⁸ The search for the Arabic terms for plague shows that they were known by the author, although they appear only

marginally and in contexts that are not easy to historicize.¹³⁹ Although the use of these words implies that the author of the manuscript knew about the disease—and how could he not know it if he was familiar with the medical literature of the Muslim world?—there is no evidence he mentions them because it was present in the Timbuktu area in the 17th century. Could other Sahelian manuscripts throw light on the questions relative to epidemics in the Niger valley and its urban centre during the 16th and 17th centuries? This is an intriguing question that further research might help to answer. None of the would-be responses, however, would solve the conundrum of the spread of the plague in the 14th-century valley of the Niger, because plague could have been (re)introduced from Egypt or North Africa where it long remained endemic. In the absence or quasi-absence of documentary sources in the Sahel from the 14th to the 16th century, I believe progress in determining whether plague did or did not affect Africa south of the Sahara will come from archaeological findings rather than the written word. We should not totally dismiss written sources, however, not only because later documentation could throw light on these periods, but also because of the presence of contemporary epigraphic sources which require our attention. To date, the only corpus of such material is that published by De Moraes Farias.¹⁴⁰ Particularly interesting is the corpus of inscribed tombstones, not because they mention causes of death—they do not—but because their chronology is meaningful for the study of plague. The fact that only one of the documented tombstones at Gao is posterior to 1350 may point to the rapid abandonment of this particular practice in the second half of the 14th century.¹⁴¹

Conclusion: Creative ways to challenge the invisibility of the plague in Sub-Saharan Africa

- 53 Plague appears as an ideal case study of the possibility to intertwine archaeological and documentary evidence, each with its strengths and its weaknesses, into what we hope will become a strong case for or against the spread of the Black Death in Sub-Saharan Africa in the 14th century and its reoccurrences throughout the early modern period. It is worth noting that the archaeological evidence is not meant to support the documentary evidence, or vice versa. This would be too much to ask from an interdisciplinary approach. In fact, the disciplines of archaeology and history are equipped with different, not complementary, toolkits. Each of these disciplines evolved along the lines of distinct epistemologies that share very little but the same objective: attempting a reconstruction of the past. Here, historical and archaeological methods work independently of each other, though in the same direction. Ultimately, distinct sets of evidence independently retrieved and discussed in the framework of the two disciplines can be mobilized to generate a stronger, deeper, better-argued narrative.
- 54 In fact, the picture is even more complex, as several other disciplines and sets of evidence have yet to be discussed in this paper. The most intriguing one is that presented by Monica Green in the fourth paper offered in this special issue of *Afriques*. This paper is the perfect embodiment of the commitment of a generation of historians eager to engage deeply in interdisciplinary research, far beyond the consortium Humanities–Social Science which we inherited from the historians of the *Annales* school. Using her extensive knowledge of the historiography and epistemology of global health science, together with her deep acquaintance with the best of recent and

cutting-edge microbiological advances in the genetics of pathogens, Monica Green stands at the forefront of a promising new field of interdisciplinary inquiry located at the intersection of history and genomics. This new sub-field uses the principles of phylogenetics to develop interpretations of a new category of sources, which until now was the preserve of bioscientists. Monica Green was among the first historians to challenge scientists in their worrisome (in)ability to interpret this data in historical terms outside the realm of the historian's craft.¹⁴² She devised a method to apply to this new form of 'big data', using a critical apparatus satisfying the canons of the best practices in history. Her work has positioned history not only as a credible but also as the most appropriate disciplinary framework in which genetic data can become new kinds of sources of the past, in their own terms.¹⁴³ In the age of genomics, she is the architect of a real historiographical revolution, one that can address global historical processes with the combined rigours of historical criticism and biological science. In 'Putting Africa on the Black Death map: Narratives from genetics and history', Monica Green explored one set of evidence that had never been discussed to date: the genetics of *Y. pestis* bacillus as it exists today in some environments of East Africa. But how could the genetic heritage of a pathogen be a source of history? The response is in the question, and more particularly in the term 'heritage'. It is now clearly established that *Y. pestis* did not originate from Africa but came from outside the continent.¹⁴⁴ It follows that each bacillus present today in Africa is genetically connected to the specific strain of pathogen that spread to the continent from other parts of the Old World. Since the broad outline of the evolutionary history of *Y. pestis* is quite well known, and phylogenetic trees have been built that enable us to visualize how each pandemic of plague has favoured the emergence of genetically distinct groups of modern pathogens, we can associate each modern strain of *Y. pestis* with a particular pandemic event.¹⁴⁵ In plain language, this means that we can distinguish between pathogens that evolved from strains of *Y. pestis* that came to Africa after 1899, during the Third Pandemic arriving from Asia on steamboats, and those that descend from earlier strains of the bacillus that circulated during the Second Pandemic, in the second half of the 14th century. Even more exciting, progress in documenting the full genome of past pathogens through the recovery of aDNA from plague cemeteries enables us to start the process of reconstructing chains of transmission between pathogens of the past and their modern heirs, by pinpointing specific successful mutations in time and space. Reconstructing such chains of transmission implies asking the following questions: Which of the ancient strains of plague is ancestral to specific modern strains of plague? Where is the ancestral strain of a modern pathogen documented, and how did the pathogen circulate from these known ancestral loci to its modern location? Through the careful comparison of genomic information, possible scenarios of transmission emerge that need to be investigated against documentary and archaeological evidence. Because plague is endemic in several parts of East Africa and in Madagascar,¹⁴⁶ it is possible to compare the genomes of modern strains of *Y. pestis* in these parts of the world¹⁴⁷ against the global modern and ancient genomic database.¹⁴⁸ This is indeed the departure point of Green in this groundbreaking paper, where she demonstrates that plague in East Africa preceded the Third Pandemic and that the currently known isolates are a part of the history of the Branch 1 lineage of *Y. pestis*.¹⁴⁹ Green suggests that the diffusion of this particular strain in Africa took place after the Black Death, possibly between the 15th and 17th century, and she suggests several scenarios of possible circulation of the disease—which will await the development of successful

strategies to recover and explore aDNA in Africa, to be further discussed. This is definite proof that the plague hypothesis and the framework of the Second Pandemic is to be taken seriously. It is not the full story, however, as all strains of *Y. pestis* that circulated in East Africa could not be successful at persisting in the landscape. On the contrary, such an evolutionary success story appears to be an exception, as most pathogens would eventually burn out as a result of having destroyed the rodent infrastructure and in the absence of a reservoir, not unlike how wild fires die out in the absence of available fuel. The fact that the pathogen might have entered East Africa at some point during the early modern period does not mean that it had not already circulated earlier on. On the contrary, it suggests that plague could thrive in the environments of East Africa as they existed before the colonial period, that it did circulate as a predictable outcome of the connectedness of this part of the continent with Asiatic and Mediterranean plague foci, and that the pathogen that could have spread into East Africa as early as the mid-14th century might have done so without leaving behind known living descendants. The story of Kilwa and that of Great Zimbabwe—and of many other communities¹⁵⁰ showing clear signs of population decline and abandonment of architectural projects or even of extensive landscapes—remain possible testimonies to their participation in the fate of global communities in the wake of the Black Death.¹⁵¹

- 55 Thinking about the natural history of plague also forces us to consider its impact on the environment, and particularly on populations of rodents that everywhere served as infrastructure to the disease. The spread of plague in Sub-Saharan Africa would not have taken place without a massive die-out of rodent species encountered, which did not have strong enough resistance mechanisms to become reservoirs. The archaeology of rodent population dynamics is still in its infancy, as remains of rodents have hardly been identified as priorities in the archaeological record.¹⁵² This needs to change, and I invite archaeologists and zooarchaeologists not only to pay more attention to these often marginalized parts of faunal assemblages, but also to attempt the extraction of pathogenic aDNA from these remains coming from pre-19th century contexts. If rodent populations were severely affected by the spread of *Y. pestis*, we expect it caused some forms of genetic drifts due to a population bottleneck, which might still be visible when comparing ancient and modern rodent genetic materials. It might also have caused the displacement or even replacement of some species of rodents by others, a phenomenon that could also be visible in the zooarchaeological record.¹⁵³ Another aspect of the history of plague is the impact of the massive loss of human lives on the environment. We have seen that a classic effect of plague was the abandonment of settlements and marginal lands, resulting in the expansion of woodlands. This we have seen to be visible in Ife, but it must have been a large-scale phenomenon in Africa, resulting in a contraction of farmlands and the rapid expansion of forested landscapes, which were reclaimed by communities during the following centuries at the price of massive labour mobilizations—although some were probably never reclaimed and developed instead into the bulk of modern forested landscapes in Africa. There is a need to interrogate our botanical remains and charcoal samples in terms that speak to this phenomenon. There is equally a need to interrogate the impact of the deanthropization of landscapes on the wild fauna. In the wake of the plague, we would expect to recover less faunal remains as a whole, as human consumption would have collapsed together with population, before picking up again in the 15th and 16th centuries. Parallel to the abandonment of landscapes by human populations, we would expect the return of wild

species which would have been under quite some pressure in highly anthropicized landscapes. We would expect elephants and wild bovids, but also carnivores such as lions, panthers, or crocodiles, to have benefitted from the anthropic depression, to be again hunted and therefore more visible in the zooarchaeological records of the recovering phase. It is possible that such expectations coincide with the rise of some wild species in the record of the Banda area's Mid-Late Kuulo (1400–1600), as published by Amanda Logan and Ann Stahl in 2017.¹⁵⁴ It is during this phase that large, wild carnivores are best documented, including lions, panthers, and leopards, and the ubiquity of elephant is multiplied by three.¹⁵⁵ The mere over-representation of the large carnivores suggests an earlier period (Banda area's Ngre and Early Kuulo¹⁵⁶) characterized by a lesser anthropicized landscape that favoured reproduction, and an increase in the availability of wild fauna on lower levels of the food chain. As such, the over-representation of the large, wild carnivores and herbivores during the Mid-Late Kuulo phase suggests a rebound of the local population and an on-going process of re-penetration by humans (especially hunters) of lands previously lost to the wild—although the enduring decrease of the number of grains¹⁵⁷ recovered archaeologically from the corresponding contexts suggests the emergence of food strategies less dependent on the cultivation of cereals than previously. This was noted by Amanda Logan for the Mid-Late Kuulo period in terms of coping strategies characterized by the adoption of wild/weedy plants to supplement pearl millet and, later, maize.¹⁵⁸ I would like to suggest that in West Africa, landscapes, therefore, continued to remain less anthropicized and more forested than they were before the mid-14th century. It took time for farm lands to be gradually reclaimed as population levels slowly picked up between the 15th and the 17th century.

- 56 Third, we also need to add to our already complex equation a number of intriguing terracottas representing individuals exhibiting lesions in the form of buboes, which coincide remarkably—by their shape, size, and positions along the network of lymphatic nodes—with historically and medically documented symptoms of bubonic plague. These terracottas are usually referred to, together with many other types, as 'Jenne' or 'Djenné'; and most of the specimens published originate from illegal and therefore uncontrolled/undocumented excavations of archaeological sites in the Republic of Mali.¹⁵⁹ A reflection on their possible healing function(s), distribution, meaning, chronology, and, more basically, their 'usability' as sources of medical history is urgently required.¹⁶⁰
- 57 Finally, I believe this series of papers argues conclusively for the necessity to include plague as a possible factor of change in Sub-Saharan Africa during the Second Plague Pandemic. An increasing number of colleagues—historians, archaeologists, and historical linguists¹⁶¹ in particular—are open to the idea and ready to test some of the principles and reflections suggested here with their own data. At this point, despite the considerable progress we have made in strengthening the plague hypothesis, we need to remain prudent and admit that we are not yet in a position to fully demonstrate that the plague did or did not spread in Sub-Saharan Africa from the mid-14th century onward. If it did not, our project will have been an excellent opportunity to re-open and update a number of important files about the history of Africa during the first and early second millenniums CE and to re-galvanize the study of West Africa before the opening of the Atlantic trade. If the plague *did* impact Sub-Saharan Africa, the

repercussions for our discipline are potentially so revolutionary that I prefer, in the meantime, to think of them as unfathomable.

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NOTES

1. Plague is an infectious zoonosis due the bacterium *Yersinia pestis*, which has caused several deadly pandemics in human history. Recent breakthroughs in the genetics of the modern bacterium and the recovery of fragments of DNA (aDNA) of some of its past strains in historical burial sites have fostered a revolution in the way historians think of the disease. For recent overviews about the disease and its historiography, see M.H. GREEN, 2014a, 2014b; N. VARLIK, 2014, 2015, p. 17-127; B. CAMPBELL, 2016, p. 267-394. See also M.H. GREEN, this volume. In a path-breaking essay published in 2014, R. HYMES took up the suggestion of Y. CUI *et al.*, 2013 that the sudden divergence of *Yersinia pestis* strains started in the first decades of the 13th century. His hypothesis that the disease may already have been moving across Eurasia for about a century before becoming visible in the Mediterranean—and perhaps Sub-Saharan African—records, however, remains to be substantiated by molecular evidence.
2. See D. GALLAGHER, S. DUEPPEN, 2018, §3, n. 6. Others believed in an African exception, usually based on the idea that the Sahara acted as an epidemiological buffer that protected Sub-Saharan Africa from Old World epidemics. See for instance J. ILIFFE, 1995, p. 67. This view, however, does not take into account the great diversity of ecological circumstances under which plague persists in Africa, including on the edges of the Sahara, nor the fact that Africa was connected to the Old World through many more routes than those followed by caravans across the Sahara. See for instance S. NEERINCKX *et al.*, 2008 and I. BITAM *et al.*, 2010.

3. Here, this term is used as a synonym of the Second Plague Pandemic. Its use is often applied only to the first wave of the Second Plague Pandemic in the European record, starting in 1347 and dying out at about 1354, soon to be replicated by a second wave in 1360–1363 (sometimes referred to as *pesta secunda*) and followed by more recurrent sweeping waves and many secondary outbreaks lasting up to the 18th century, and probably later.
4. See for instance the remarkable monograph by T. GREEN, 2012; or G. BROOKS, 1993.
5. In the Archaeology of Ghana, the term earthwork is used to describe a settlement surrounded by an enclosure made of one or several ditches and rammed-earth banks. See for instance D. KIYAGA-MULINDWA, 1979.
6. This is based on Beta-184397, published in G. CHOUIN, C. DECORSE, 2010, p. 137.
7. D. KIYAGA-MULINDWA, 1979, 1982.
8. G. CHOUIN, C. DECORSE, 2010.
9. J. BOACHIE-ANSAH, 2010, 2014.
10. ANR-14-CE31-0015. Reconnecting Africa: Sub-Saharan Africa and the World prior to European Imperialism, coordinated by Adrien Delmas.
11. Participants in the workshop included Suzanne Blier, Sandro Capo Chichi, Gérard Chouin, Marie-Laure Derat, François-Xavier Fauvelle-Aymar, Daphne Gallagher, Kelly Harkins, Monica Green, Susan McIntosh, Henrik Poinar (via teleconference), Jeremy Pope, and Kristina Van Dyke.
12. We are aware of the limitation of our inductive method, which aims at generating a new theory based on the available data rather than testing well-established general principles to reach a demonstrable conclusion. In the present state of knowledge, however, we believe this inductive method is the only way to propose and promote new ideas to the scientific community.
13. The plague of Justinian or Justinianic Plague (c. 541 – c. 750) has also been called the First Plague Pandemic. Although its full geographic extent is unknown, it is known to have spread in parts of Africa, Europe, and Asia.
14. L. LITTLE, 2007, p. xi.
15. The expression ‘negative evidence’ can be confusing. Here it means evidence of absence—that is to say, evidence of non-activity, of decay. It points in a negative direction (abandonment, decay) rather than in a positive direction (growth). As such, it has its inherent limitations, in that *absence* cannot be used to infer *why* things were abandoned/allowed to decay. L. LITTLE, 2007, p. 15. I thank Monica Green for her comments on this expression.
16. S. MITCHELL, 2015, p. 480.
17. S. MITCHELL, 2015, p. 480 invites us to pay attention, more generally, to divergences ‘from normal local funerary practice’. See also M. MCCORMICK, 2015, 2016a, and 2016b.
18. The quantitative decrease of domestic pottery used by a population before and after the crisis could also provide new insights into changes in medieval demography. See for instance C. LEWIS, 2016.
19. See G. CHOUIN, 2009, p. 408–409.
20. On Ife, see below.
21. An encounter with such a royal python emerging from the roof of an adept of Uli art, in Igboland, is recounted in C. IKWUEMESI, 2016, p. 4. European visitors in the early 18th century also noted brass pythons decorating the roofs of the royal palace at Benin City. See Van Nyendaël in W. BOSMAN, 1704, p. 252. They were probably already present in the 16th century since they figure in the two known bronze plaques depicting the palace. See K. GUNSCH, 2018, p. 59 and 63. One of them was found during excavations of the palace, led by Goodwin in 1956–1957 (A. GOODWIN, 1963). In *Things Fall Apart*, Chinua Achebe reminded us that pythons ‘ate rats in the house’ (C. ACHEBE, 1967, p. 144). On pythons in Achebe’s work, see also R. MCDANIEL, 1976.
22. S. MITCHELL, 2015, p. 491.
23. C. WICKHAM, 2005, p. 476–481.

24. S. MITCHELL, 2015, p. 481.
25. C. WICKHAM, 2005, p. 507-509.
26. See fig. 7 in I. DAVEAU, 1997, p. 28.
27. I. DAVEAU, 1997.
28. We could not explain differences in the data provided by the author in fig. 4 and 7 with those in fig. 8 (I. DAVEAU, 1997, p. 28-30).
29. In addition to the reduction in number, the archaeologists also noted a gradual decline in size over the period. All graphs are found in I. DAVEAU, 1997, p. 30.
30. J. LIEBESCHUETZ, 2001, p. 43-54, 57-63.
31. See C. WICKHAM, 2005, p. 443-450; S. MITCHELL, 2015, p. 486-487.
32. See for instance the excellent review of evidence from Byzantine Syria by H. KENNEDY, 2007.
33. Climatic change and other natural disasters are often mentioned by archaeologists as possible causes of decline, besides plague. See for instance Y. HIRSCHFELD, 2006 about the abandonment of many Levantine settlements in the 6th century. While acknowledging the shrinkage of population due to the combined effects of plague, wars, and other natural disasters, J. LIEBESCHUETZ also suggests that cities and villages were affected by the running down of empire that brought about 'the end of a political tradition, the end of a pattern of urban design related to a political tradition, the end of a secular idea of education' (2001, p. 415).
34. S. MITCHELL, 2015, p. 491.
35. On written accounts about plague during the First Pandemic, see for instance the case of Syriac sources in M. MORONY, 2006. On historical sources of the Black Death and the risk of misusing them, see J. ROOSEN, D.R. CURTIS, 2018a.
36. B. CAMPBELL, 2016, p. 309-310. On the Second Pandemic and its historiography, see above footnote 1.
37. B. CAMPBELL, 2016, p. 303-304.
38. See H. KENNEDY, 2007, p. 89; K. LILLEY, 2015.
39. D. GALLAGHER, S. DUEPPEN, 2018, §54.
40. D. GALLAGHER, S. DUEPPEN, 2018, §36.
41. M.H. GREEN, 2014b, p. 32; R. MCINTOSH, 1998, p. 247-250.
42. R. MCINTOSH, 2005, p. 177.
43. R. MCINTOSH, 2005, p. 177. I disagree with this chronological interpretation of T. Togola's work in Méma (see also S.K. MCINTOSH, 1995, p. 376). Dates obtained at the sites of Akumbu (T. TOGOLA, 2008, p. 34, Beta-40441) and Kolima (P. FONTES *et al.*, 1991, p. 35), for instance, suggest these two settlements were abandoned in the 14th century rather than by the end of the 13th century.
44. R. HYMES, 2014. Hymes' hypothesis applies to East Asia.
45. In French, *Mission Archéologique d'Ifé-Sungbo*, with core funding provided by the French Ministry of Foreign Affairs. The project is co-directed by Adisa Ogunfolakan, Obafemi Awolowo University at Ile-Ife, and the author of the present paper. It currently includes several dozen international scholars from Nigeria (University of Ibadan, Ahmadu Bello University at Zaria), France (IRD, CNRS, CEREGE, University of Paris-I, University of Rouen, University of Aix-Marseille), USA (William & Mary, Northwestern University), South Africa (The Heritage Foundation), Italy (University of Milan), and Switzerland (University of Lausanne).
46. Frank Willett, who had previously excavated the city wall at Ita Yemoo in 1962-1963, had already noticed the presence of the dark layer he called 'the old land surface' (F. WILLETT, 2004, II, 2, p. 7). He rightly associated it with farming land on which the inner bank of the town wall system was erected, although he did not provide any stratigraphic interpretation.
47. Soil samples from the dark level at Oduduwa College were wet-screened and floated, but we did not recover any charcoal fragments.

48. We are still struggling with the term ‘abandonment’, which does not consider the fact that the site continued to occupy a central place in memories and in regional political imaginations.
49. On the basis of available radiocarbon dates, we think this process could have taken place between the mid-16th and mid-17th century (G. CHOUIN, A. OGUNFOLAKAN, 2018).
50. Unfortunately, this hoard was not recovered by archaeologists and the context of the discovery was recorded only succinctly. See W. BASCOM, 1938; F. WILLETT, 1960, p. 238-239.
51. P. GARLAKE, 1974, p. 122-123.
52. See for instance a synthesis in R. SMITH, 1986, p.14-15.
53. See G. CHOUIN, O. LASISI, forthcoming, 2019.
54. Archaeological data we collected at Ife suggest continuous development of the Ife settlement from the 10th to the 14th century CE. Evidence of exponential growth is visible between the mid-13th and the mid-14th century, with the large-scale expansion of pavements throughout the settlement, the development of a prestigious glass industry, and the commission of spectacular works of copper and bronze by the elite. In her last grand opus on the art of Ife, Suzanne Blier even coined the vivid term ‘florescence era’ to render the cultural effervescence and artistic ‘flowering’ during the period c. 1250–1350 CE; S. BLIER, 2015, p. 43-44. It is worth noting that, in the same volume, Suzanne Blier independently suggested that the plague (‘Black Death’) might have been one of the likely agents in the ending of the florescence era (p. 66-67). However, she did not present new evidence.
55. To my knowledge, there was no overview of Black Death cemeteries in Europe before the recent study by B. BRAMANTI *et al.*, 2018, p. 4, table 1. Here, I will limit myself to the best-known cases from England, France, and Spain to contrast the differences between Europe and Africa. In England, the 14th-century site of East Smithfield is particularly well known. See for instance D. HAWKINS, 1990; I. GRAINGER *et al.*, 2008; S. DEWITTE, 2014. In France, classic cases include mass burials related to the 18th-century epidemics of plague in Provence (see for instance O. DUTPUR *et al.*, 1994; M. SIGNOLI, 2006; S. TZORTZIS, M. SIGNOLI, 2009), and an increasing number of cases have been studied in detail for the 16th- to 18th-century period. See for instance P. POULLE, 2007; I. SOUQUET-LEROY *et al.*, 2015, p. 64-72. Published collective burials dating from the mid-14th century are relatively rare and include sites in Montpellier, Dreux, Vilarnau, and Saint-Laurent-de-la-Cabrerisse, although recent excavations in Toulouse (16, rue des 36-Ponts) and Paris (Ancien hôpital de la Trinité, Monoprix Réaumur-Sébastopol) might soon add to knowledge for that period. See E. CRUBÉZY *et al.*, 2006, p. 254-255; D. CASTEX, 2008, p. 25-27; O. PASSARRIUS *et al.*, 2008, p. 200-204; S. KACKI *et al.*, 2011. In Spain, mass burials (*fossa communa*) are known for the late 14th century and later periods. See for instance J.B. DE HEREDIA BERCERO, I.G. PINEDA, 2014 (Basílica dels Sants Màrtirs Just i Pastor, Barcelona). At Tarrega, archaeologists identified mass burials associated with the first wave of the Second Pandemic in 1348, including trenches where victims of collateral pogroms against the Jewish population were buried. See A. COLET *et al.*, 2009; A. COLET *et al.*, 2014.
56. ‘In light of the dramatic contemporary descriptions of massive mortalities and plague pits, how do we explain the apparent archaeological absence of unambiguous mass burials [...]’ (M. MCCORMICK, 2015, p. 326). Such was the question asked by McCormick in the introduction to a two-part article where he took up the challenge of ‘tracking mass death’ in the late Roman Empire. M. MCCORMICK, 2015, 2016a, 2016b. A similar question was first raised by H. Kennedy about the First Plague Pandemic; H. KENNEDY, 2007.
57. CASTEX, D. *et al.*, 1962 already discussed the complexity of the interpretation of multiple burials.
58. The multiplication of new methodological approaches and tools has since transformed the approach to burials. See for instance D. CASTEX, 2007; D. CASTEX, 2008; S. DEWITTE, 2010; D. CASTEX, S. KACKI, 2016; S. TZORTZIS, M. SIGNOLI, 2016.
59. H. DUDAY, 2008.

60. For a proposed typology of burials related to epidemic catastrophes, see A. FORNACIARI, 2017. On lime burials, see E. SCHOTSMANS *et al.*, 2015.
61. Burials in Africa are rarely excavated by trained archaeothanatologists or social bioarchaeologists. The theory and practice of mortuary archaeology is rarely taught in Sub-Saharan Africa and seldom applied to the excavation of burial grounds. On the theory and practice of archaeothanatology or social bioarchaeology, see for instance H. DUDAY, 2009. Work on the site of Wanaar, in Senegal, provides excellent examples of sound recording and study of complex burial contexts. See for instance J.-P. CROS *et al.*, 2013.
62. L. DESPLAGNES, 1903.
63. Alain Froment 2014, pers. comm.
64. See for instance S. MARCINIAK, H. POINAR, 2018; A. MARGARYAN *et al.*, 2018.
65. See for instance L. LITTLE, 2011.
66. I thank the Head of the Department of Archaeology and Anthropology, University of Ibadan for selecting and authorizing the export of this material to France. Professor Caleb Folorunso and Mr. Kola Adekola provided much appreciated support. The material was first studied by the team of Gérard Aboudharam at the University of Aix-Marseille, then by Lars Fehren-Schmitz and Kelly Harkins at the UCSC Human Paleogenomics Lab. In both cases, no ancient DNA was recovered. Sequencing of ancient DNA (aDNA) from human remains in Africa has proved to be challenging because of the notoriously poor preservation of DNA in warm, tropical areas. For a review of recent progress in this matter and associated ethical concerns, see M. PRENDERGAST, E. SAWCHUK, 2018. In the case of the material discussed here, although the sequencing of aDNA failed, the study of associated dental calculus led to a contribution on ancient global oral microbiomes; N. RASCOVAN *et al.*, 2016.
67. G. CONNAH, 1975, p. 59. 3 ft. 9 in. and 4 ft. 8 in. are, respectively, 1.14 and 1.42 m.
68. G. CONNAH, 1975, p. 182.
69. G. CONNAH, 1975, p. 66.
70. The only Benin date published in *Radiocarbon* is I-2721, and this was later considered inaccurate by the laboratory. See G. CONNAH, 1975, p. 182; J. BUCKLEY, E. WILLIS, 1969, p. 86.
71. Published in F. YAMASAKI *et al.*, 1969, p. 461.
72. Published in F. YAMASAKI *et al.*, 1969, p. 461.
73. G. CONNAH, 1975, p. 66.
74. I am currently preparing a detailed re-evaluation of feature 21. On the potential impact of the plague on the Benin polity, see G. CHOUIN, O. LASISI, forthcoming.
75. On wealth, power, and consumption, see for instance F. RICHARD, 2017. On accompanying victims, see A. TESTART, 2004.
76. For a general discussion, see S. DUEPPEN, 2016, p. 253-254. For on-going discussion of the chronology of megalithic sites in Senegambia and its ceramics, see A. GALLAY, 2010; A. DELVOYE *et al.*, 2011.
77. C. WICKHAM, 2005, p. 599, 647.
78. S. BORSCH, 2014.
79. S. BORSCH, 2014, p. 147-148.
80. D. OHADIKE, 1981.
81. On the raised fields, see D. MCKEY *et al.*, 2016; G. DE SAULIEU *et al.*, 2018. On the ancient trench systems of Nigeria, see P. DARLING, 1984.
82. S. BORSCH, 2005.
83. G. CHOUIN, O. LASISI, forthcoming.

84. See for instance J.W. LANKTON *et al.*, 2006; A. OGUNDIRAN, O. IGE, 2015; A.B. BABALOLA, 2016; A.B. BABALOLA *et al.*, 2017; A.B. BABALOLA, L. DUSSUBIEUX, S.K. MCINTOSH, T. REHREN, 2018; A.B. BABALOLA, T. REHREN, A. IGE, S. MCINTOSH, 2018.
85. A.B. BABALOLA *et al.*, 2017; A.B. BABALOLA, L. DUSSUBIEUX, S.K. MCINTOSH, T. REHREN, 2018.
86. This is based on unpublished analyses conducted by Laure Dussubieux at the Field Museum on a small sample of beads from recent excavations at Ita Yemoo and Oke Atan, both located at Ile-Ife.
87. See for instance A. OGUNDIRAN, O. IGE, 2015. All the more recent publications derived from the work by A. Babalola and listed above also insist on the same.
88. See A. OGUNDIRAN, 2002, p. 432-437. Ancient African beads became known by European traders as *contas de terra*, a Portuguese expression meaning 'beads from the ground' or 'ground-beads'. See for instance R. MAUNY, 1958, p. 211; J. FAGE, 1962, p. 344-346.
89. See for instance A. OGUNDIRAN, 2002, p. 434-435.
90. Plague studies form a field on their own, with animated debates and an eventful intellectual history. Here, I will simply point to recent scholarship looking at the broad notion of impact. Economic historians played an important role in deciphering a number of archives pertaining to the demographic impact of the plague. Their work points to a devastating loss of human life everywhere, with variations but no exceptions. See for instance J. ROOSEN, D. CURTIS, 2018b. Medical response to plague formed a body of work often known as plague treaties and which equally flourished in the Christian and Islamic world. On medical writing on plague in England, see for instance L. JONES, 2018; on the culture of plague in England, see K. MILLER, 2017. On the impact of plague on the Islamic world, we shall note the very innovative contribution of Nūkhēt Varlik for the Ottoman Empire, a long-forgotten piece of the plague experience in the Mediterranean world. See N. VARLIK, 2015, 2017.
91. The impact of plague on the transmission of knowledge remains insufficiently discussed in the literature. But see for instance M. HUDSON, 2014. See above our discussion on the non-transmission of glass beads technology in the region hitherto dominated by Ife.
92. See N. LEVTZION, J. HOPKINS, 2000, p. 317-342; J. CUOQ, 1985, p. 348-350. For an excellent reflection on Ibn Khaldun's treatment of Mali's history, see R. AUSTEN, J. JANSEN, 1996.
93. Ibn Khaldun quoted in J. ILIFFE, 1995, p. 49.
94. Much has been written on Ibn Khaldun's philosophy of history, and all others agree on the central place occupied by urban centres in his understanding of the cyclical rise and decay of civilizations. See for instance B. STOWASSER, 1984; F. BAALI, 1988, p. 69-82; G. MARTINEZ-GROS, 2006.
95. M. MAMDANI, 2017:15
96. On Ibn Khaldunism, see C. FLEISHER, 1983; S. ALATAS, 2007. See also R. AUSTIN, J. JANSEN, 1996.
97. After B. Ouasti, Collet writes of a four-handed work, 'une oeuvre écrite "à quatre mains"'. B. OUASTI, 2006: 73; H. COLLET, 2017a: 441, 445. On Ibn Juzayy and recent scholarship on this scholar and poet as co-author of the *Tuhfat al-nuzzār*, see H. COLLET, 2017a, p. 441-448.
98. For an important history of the text and its different editions/translations, see H. COLLET, 2017a, p. 397-415.
99. For a discussion of the arguments, see F.-X. FAUVELLE-AYMAR, B. HIRSCH, 2003, p. 75-78; D. WAINES, 2010: 3-26. Ibn Khaldun was among the critics of Ibn Battuta and expressed his doubts in the *Muḳaddima*. See D. WAINES, 2010: 6.
100. The exception is F.-X. FAUVELLE-AYMAR, B. HIRSCH, 2003.
101. H. COLLET, 2017a.
102. We follow Collet's use of the term 'positivist' to define such a practice. See H. COLLET, 2017a, p. 410.
103. S. JANICSEK, 1929. See also H. COLLET, 2017a, p. 424 and n. 1047.

104. Bulghār was located on the Volga, in the modern Republic of Tatarstan, in south-western Russia.
105. S. JANICSEK, 1929, p. 791.
106. S. JANICSEK, 1929, p. 800.
107. Below, I use '*riḥla sūdāniyya*' to refer to this particular part of Ibn Battuta's work. For a new French translation derived from the 1997 new Arabic edition by 'Abd al-Hādī al-Tāzī, see H. COLLET, 2017a, p. 548-579.
108. As suggested by F.-X. FAUVELLE-AYMAR, B. HIRSCH, 2003, p. 95.
109. H. COLLET, 2017a, p. 446-448.
110. On the questionnaire, see F.-X. FAUVELLE-AYMAR, B. HIRSCH, 2003, p. 96-100.
111. H. COLLET, 2017a, p. 424: '[...] ruée vers des aventures d'identifications toponymiques plus ou moins heureuses'. My translation.
112. For a discussion of the 'troubling intertextuality' between Ibn Battuta's work and Al-'Umari's *Masalik*, see H. COLLET, 2017a, p. 428-432.
113. See F.-X. FAUVELLE-AYMAR, B. HIRSCH, 2003; H. COLLET, 2017a: 395-469. A useful synthesis of the main arguments developed in the dissertation can also be found in H. COLLET, 2017b.
114. See F.-X. FAUVELLE-AYMAR, B. HIRSCH, 2003; H. COLLET, 2017a: 424-428.
115. الأسفار وعجائب الأمصار غرائب في النظر تحفة, my translation.
116. In the words of the author, an 'off centering' from the usual perspective. H. COLLET, 2017a, p. 464.
117. See above, §38.
118. I thank Hadrien Collet for this astute remark.
119. The transmission of the primary infection of plague in the Mediterranean from an assumed Eastern reservoir was probably sped up by the maritime trade in grains. See for instance M. WHEELIS, 2002, p. 974. Although the camels crossing the Sahara are often compared to vessels crossing a sea of sand, the nature and quantity of the goods transported, and also the modalities of the transportation and packaging of the goods, were very different. Caravans would not have easily transported plague from one side of the Sahara to the other in the way Venetians or Genoese vessels could from the confines of the Black Sea to the Italian peninsula. If plague crossed the Sahara, it would have been through a much more complex process, perhaps involving commensal rodents associated with nomadic communities, as illustrated in I. BITAM *et al.*, 2010. Plague could, however, also have progressed westward along the southern edge of the Sahara, from the Nile or Red Sea area.
120. At this point, it is too early to try to tie our African hypothesis to the emerging literature on the recurrence of plague in the Mediterranean world and northern Europe in the late 1350s and 1360s. When genomic information on plague becomes available for Sub-Saharan Africa, there will be interesting comparisons to be made with recent advances in the understanding of *pestis secunda*, a term for which there is no consensual definition at present but that refers to a series of outbreaks between ca. 1357 and 1366. See for instance M. SPYROU *et al.*, 2016; S. DEWITTE, M. KOWALESKI, 2017, p. 12-15; A. NAMOUCHI *et al.*, 2018, p. E 11790. See also M.H. GREEN, 2018, §22. I thank Monica Green for sharing with me her insights on *pestis secunda*.
121. On the difficulty of letting go of a source in a context where they are so rare, see F.-X. FAUVELLE-AYMAR, B. HIRSCH, 2003, p. 76.
122. J. PERRUCHON, 1893, p. 73, my translation. This reference is discussed in M.-L. DERAT, 2018, §20.
123. See for instance the case of Syriac sources about the First Plague Pandemic, in M. MORONY, 2007, p. 70-78.
124. R. PANKHURST, 1985. See also M.-L. DERAT, 2018, §2.
125. See M.-L. DERAT, 2018, §12.
126. M. MORONY, 2007, p. 60-61.

127. To my knowledge, this is the first time that the diffusion of the cult of Saint Roch in Africa is documented. On Saint Roch, see for instance P. BOLLE, 2001; A. RIGON, A. VAUCHEZ, 2004. More recent scholarly resources are also listed on the website of the Centro Studi Rocchiano Internazionale, <http://www.sanroccodimontpellier.it/>. Beyond Ethiopia, we need to think of the ways plague might have affected belief systems on the continent, and the necessity to scrutinize the latter for clues of the plague.

128. See M.H. GREEN, 2014a.

129. Our prime objective is to demonstrate that plague affected Sub-Saharan Africa as a whole but, in a second phase of the reflection, we will need to think of the dynamics of the disease within the African continent. There is clearly a difference in the way plague seems to have become established in parts of East Africa (see M.H. GREEN, 2018), on the highlands of Ethiopia (see M.-L. DERAT, 2018), and perhaps in parts of the Niger valley (see paragraphs below)—while elsewhere the pathogen might not have found the proper conditions for the establishment of plague foci, in terms of environmental and climatic factors and rodent infrastructure. We hypothesize that in most of forested West Africa, plague never became endemic in the late medieval period and that the first wave of epidemic was not followed, unlike the case in Europe and in the Mediterranean world, by recurring outbreaks. This is supported by the absence of reports of plague epidemics in coastal West Africa by European travellers and traders throughout the early modern period. Pending additional research, we hypothesize that the first wave of plague circulated in the existing rodent infrastructure, then burnt out in the absence of a suitable reservoir.

130. These sources still need to be fully assessed for evidence, as we were unable, in the course of the GlobAfrica project, to create the necessary network of those who curate or have a strong knowledge of existing collections.

131. M. HISKETT, 1964, 1965a, 1965b.

132. M. HISKETT, 1964, p. 540.

133. M. HISKETT, 1965a, p. 117. The original text is published in M. HISKETT, 1964, p. 544.

134. M. HISKETT, 1965b, p. 365-367. These dates need to be taken with caution. The known versions of the Chronicle of Kano do not include dates. Historians disagree on its process of creation and transmission. On the Chronicle of Kano, see for instance H.R. PALMER 1908; M.G. SMITH, 1983; P.E. LOVEJOY, J. HUNWICK, 1993; J. HUNWICK, 1994; P.E. LOVEJOY, 2018.

135. M. HISKETT, 1965b, p. 369-370.

136. S.-M. CISSOKO, 1968. On the 'triad', see for instance D. STATHAKOPOULOS, 2017; R. BRÁZDIL *et al.*, 2018.

137. My translation.

138. See B. SALVAING, 2015, §150-152; F. SANAGUSTIN, 2011.

139. The non-specific term for plague – **وباء** – is more frequent than the specific term – **طاعون** (only 3 occurrences in about 600 pages of text). These terms appear only in volumes 1 and 3.

140. P.F. DE MORAES FARIAS, 2003.

141. None of the tombstones is posterior to 1362. P.F. DE MORAES FARIAS, 2003, ccxlv. The author rightly noted that because the cemetery has been badly impacted by the expansion of the modern town, the reconstructed chronology of the burial ground may be incomplete.

142. See for instance M.H. GREEN *et al.*, 2014; M.H. GREEN, 2014c; H. KING, M.H. GREEN, 2018.

143. See for instance 'Tiny Changes with Huge Implications: Counting SNPs in Plague's History', a dialogue between Monica Green and theoretical biologist Boris Schmid on the Blog of Michelle Ziegler, 'Contagions.' <https://contagions.wordpress.com/2016/06/27/plague-dialogues-monica-green-and-boris-schmid-on-plague-phylogeny-i/> and <https://contagions.wordpress.com/2016/06/29/plague-dialogues-monica-green-and-boris-schmid-on-plague-phylogeny-ii/>

144. M.H. GREEN, 2014a, p. 36- 45.

145. M.H. GREEN, 2018, §19-24.

146. This would not be possible in Western Africa where plague did not become endemic. The current plague epidemic in Madagascar is due to a strain of *Y. pestis* that is related to the Third Pandemic (group 1.ORI, see M.H. GREEN, 2018, §15, fig. 1), suggesting that the latter is not over yet. G. MORELLI *et al.*, 2010, p. 1141-1142. On the persistence of plague in Madagascar, see V. ANDRIANAIVOARIMANANA *et al.*, 2013. For an innovative anthropological perspective on the disease in Madagascar, see G. SODIKOFF, 2017.

147. Unfortunately, much of the genomic information pertaining to Eastern African strains of plague is still lacking or not publicly available. See M.H. GREEN, 2018, §4.

148. It is worth noting that, until very recently, the number of available ancient genomes of *Y. pestis* related to the Second Plague Pandemic remained few in number. Only six genomes, from five different cities, were described for the whole of the Old World before the recent publication, in October 2018, of five more genomes—from Norway, the Netherlands, France, and Italy. See A. NAMOUCHI *et al.*, 2018. Such a limited number of genomes generates ‘a patchy low-resolution picture of the transmission dynamics involved during the Second Plague Pandemic’ (A. NAMOUCHI *et al.*, 2018, p. E11790), which complicates the pioneering effort of Monica Green. In November 2018, however, an impressive additional 32 genomes from nine sites, located in Russia, Germany, Switzerland, England, and France, were published as a pre-print document, which considerably expands the range of available data. See M. SPYROU *et al.*, 2018. Such an acceleration announces very rapid progress in the phylogeography of the Second Plague Pandemic and promises to offer new insight into our African case study.

149. See A. NAMOUCHI *et al.*, 2018, p. E11793, fig. 3; M. SPYROU *et al.*, 2018, p. 9.

150. See M.H. GREEN, 2018, §36.

151. I am aware that asking this question only about plague is insufficient. This is an invitation to open Pandora’s box and think about all other diseases that might have circulated in Africa before the 19th century. I thank Monica Green for pointing out this epidemiological ‘elephant in the room’.

152. Banda is once again an exception. Remains of *Rattus sp.* were found associated with the periods 3 and 4 (Late Ngre/Early Kuulo and Mid–Late Kuulo periods), suggesting this invasive species originating from Asia found its way to the hinterland of West Africa earlier than usually expected. See A. LOGAN, A. STAHL, 2017, p. 1371 and p. 1393, n. 5. Most research attempting to historicize the diffusion of *Rattus rattus* (also known as house rats or ship rats) is based on genetics, and zooarchaeological data are hardly mobilized; but see M. MCCORMICK, 2003. On the diffusion of *Rattus rattus* in Senegambia, see A. KONEČNÝ *et al.*, 2013. On the Democratic Republic of Congo, see P. KALEME *et al.*, 2011. On the western Indian Ocean and Madagascar, see D. FULLER, N. BOIVIN, 2009, p. 12-14; C. TOLLENAERE *et al.*, 2010.

153. The fact that *Rattus norvegicus* largely displaced *Rattus rattus* in Europe from the 15th century onward, could have resulted in part from the demographic weakening of *Rattus rattus* populations in the wake of the Black Death. From Europe, *Rattus norvegicus* is believed to have diffused to Africa. See for instance E. PUCKETT *et al.*, 2016.

154. I thank Amada Logan for pointing out the material used below.

155. A. LOGAN, A. STAHL, 2017, p. 1372.

156. A. LOGAN, A. STAHL, 2017, p. 1361.

157. A. LOGAN, A. STAHL, 2017, p. 1391.

158. A. LOGAN, 2016, p. 110-111.

159. For a reflection on the ethics of including this material in Western scholarship, see K. VAN DYKE, 2007.

160. Reflections applied to the ‘Koma’ terracottas of northern Ghana are a useful precedent. See B. KANKPEYENG *et al.*, 2011.

161. Historical linguists are the missing piece in this special issue, but we hope to be able to present ongoing work in a later issue of *Afriques*.

ABSTRACTS

In 1347, the western and Mediterranean parts of the Old World recorded the first outbreaks of a returning mortal disease that would make its presence felt over several centuries. Known today as the Second Plague Pandemic—a zoonosis due to the bacterium *Yersinia pestis*—it scythed between a third and half of the population without regard for wealth or status. It deeply transformed all facets of societies, ignited fears, violence, and pogroms, tested the flexibility of religions, hierarchies, and traditions, and excited ambitions. Although the plague is commonly described as a pandemic, historical knowledge about the initial Black Death and the many recurrent waves of the disease is largely restricted to Western Europe and the Mediterranean world, where the literate elite left an impressive documentary record that served as resource to the long-lasting and flourishing heuristic tradition of Plague Studies. If, as suggested by Monica Green, the concept of ‘pandemic’ is to be taken seriously, we must consider the many excluded parts of the Old World, and especially Africa, in our plague narratives. We must recognize that these societies that did not practise ‘the reduction of speech to graphic forms’—to use the expression coined by Jack Goody—also could have experienced the brutal mortality of the plague and its radical transformative power, while producing no organized and specific, long-lasting traces. By extension, we must also recognize that all literate societies that had in common the art of writing did not practise this art in the same way and may not have produced identical categories of documentary records. Cultural differences affect the nature of the documentary archive, as illustrated by literate practices in medieval Nubia and Ethiopia. The invisibility or limited visibility of the plague in the documentary record is, therefore, a challenge for historians and a disguised invitation to accept the absence of evidence as evidence of absence. This paper is my attempt to resist this temptation, to challenge the quasi-absence of interest in the plague problem in the historiography of Sub-Saharan Africa, and to lay out the foundation of a research strategy that will be multi-disciplinary and comparative. The plague problem is not a footnote to African history. If the plague impacted African societies as it did in documented parts of the Old World, we must have missed or misread fundamental processes of change it entailed. Would we understand and interpret the history of Western Europe or the Mediterranean as we do if we ignored that the plague had occurred? Here, I do not solve the plague conundrum in Sub-Saharan Africa; rather, I build on the persuasive arguments made by other contributors to this special issue about the presence of plague in different parts of Africa before the 19th century. My purpose is to propose multiple, critical, and cumulative—but far from exhaustive—pathways to reading and rereading the traditional and less traditional sources of African history in the light of the possibility of societal crises related to plague. Besides presenting fragments of evidence, this paper also serves as an introduction to three groundbreaking papers exploring the archaeological, documentary, and genomic sources of the disease in the African past.

En 1347, les régions occidentale et méditerranéenne de l'Ancien Monde enregistrèrent les premières flambées de peste qui marquaient le retour de cette maladie mortifère. Elle allait de nouveau faire sentir sa douloureuse présence durant plusieurs siècles. Connue sous le nom de deuxième pandémie de peste – une zoonose due à la bactérie *Yersinia pestis* – elle faucha entre un

tiers et la moitié de la population, sans trop d'égards pour la fortune et le rang social. Elle transforma profondément toutes les facettes des sociétés, provoquant guerres, violences et pogroms, testant la flexibilité des religions, hiérarchies et traditions, et exacerbant les ambitions. Bien que la peste soit couramment associée à la notion globalisante de 'pandémie', les connaissances historiques concernant la poussée de Peste Noire initiale et les nombreuses flambées qui s'ensuivirent sont, pour l'essentiel, limitées à l'Europe occidentale et au pourtour méditerranéen. Dans ces régions, une élite lettrée s'appliqua à produire un impressionnant corpus documentaire qui a nourri la longue et florissante tradition heuristique des études sur la peste. Si, comme le suggère l'historienne Monica Green, le concept de pandémie doit être pris au pied de la lettre, alors il nous faut inclure dans le champ des études sur la peste les nombreuses parties de l'Ancien Monde jusque-là non représentées, à commencer par l'Afrique sub-Saharienne. Il nous faut reconnaître que parmi ces sociétés, celles qui ne pratiquaient pas « l'art de réduire la parole en formes graphiques » – pour reprendre l'expression de Jack Goody – purent elles aussi faire l'expérience de cette brutale mortalité et de sa radicale puissance transformative, sans pour autant en conserver des traces spécifiques, durables et organisées. Du même coup, il nous faut aussi reconnaître que toutes les sociétés ayant eu l'art d'écrire en commun ne le pratiquèrent pas à l'unisson, et ne produisirent donc pas forcément des catégories d'archives documentaires comparables. Les différences culturelles affectent la nature de cette archive documentaire, comme l'illustrent les pratiques de l'écrit en Nubie médiévale ou en Ethiopie. L'invisibilité ou la visibilité très limitée de la peste dans les sources est donc un défi pour les historiens et une invitation à peine déguisée à accepter l'absence de preuves pour preuve d'absence. Cet article est une tentative de résistance aux sirènes de la facilité, de contestation face au peu de curiosité que suscite la question de la peste dans l'historiographie de l'Afrique sub-Saharienne. Il s'agit de poser les fondations d'une stratégie de pensée qui soit résolument interdisciplinaire et comparatiste. La question de la peste ne doit pas demeurer une note de bas de page de l'histoire de l'Afrique. Si la peste eu pour les sociétés africaines ne serait-ce qu'une fraction de l'impact qu'elle eut dans les recoins documentés de l'Ancien Monde, alors il se pourrait que nous ayons manqué ou mal interprété des processus de changement fondamentaux qu'elle implique. Ecrivons-nous l'histoire de l'Occident ou des rivages méditerranéens comme nous le faisons aujourd'hui si nous ignorions que la peste fut ? L'ambition des pages qui suivent n'est pas de résoudre tout-à-fait l'énigme de la peste en Afrique sub-Saharienne. Il s'agit plutôt de construire sur les arguments persuasifs bâtis par les autres contributeurs à ce dossier concernant la présence de la peste dans diverses parties d'Afrique avant le XIX^e siècle. Je propose des chemins multiples, critiques et cumulables – mais non pas exhaustifs – pour lire et relire les sources traditionnelles et moins traditionnelles de l'histoire de l'Afrique à la lumière encore faible mais déjà persistante des traces de crises sociétales en lien avec la deuxième pandémie. Au-delà de la présentation de fragments de preuve, il s'agit surtout de préparer le lecteur à la découverte des trois autres articles innovants qui explorent à la suite les sources archéologiques, documentaires et génomiques possibles de la peste en Afrique.

INDEX

Mots-clés: peste, peste noire, deuxième pandémie de peste, archéologie, histoire documentaire, génétique, environnement, occupation de l'espace, pratiques funéraires, Ibn Khaldun, Ibn Battuta

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