The ‘Justinianic Plague’:
An “Inconsequential Pandemic”? A Reply

Abstract: The article takes up an essay published in PNAS 116 (2019), whose authors try to show that the ‘Justinianic Plague’ did not have any demonstrable major effects and in particular cannot be considered a “watershed event” between Late Antiquity and the Early Middle Ages. This attempt to argue against a “maximalist position”, however, contains considerable methodological weaknesses, which are pointed out. It is also argued that the plague must certainly be given a place in the transitional process between Late Antiquity and the Early Middle Ages; this has less to do with demographic aspects than with cultural, religious and social developments. Even if recent scientific data and new analytical methods suggest precise and objective answers to much-discussed questions and this approach currently enjoys broad public support in the media, this material, too – so the conclusion – requires a differentiated discussion that does not make any hasty reductions in complexity and can only be achieved in interdisciplinary cooperation.

Keywords: Justinianic Plague – Epidemic diseases – Epoch transition – Justinian


No Watershed Event?

A few months ago, PNAS published an article that caused an international sensation:¹ The authors believe they can prove that the so-called Justinianic Plague, which struck the Mediterraneum, Europe and the Middle East in several waves between 541 and 750, had hardly any measurable demographic effects and, in particular, did not play a significant role in the questions around the transition between Late Antiquity and the Early Middle Ages: “We find little evidential support for the claim that the JP [= Justinianic Plague, M. M.] was a watershed event” – as the authors conclude.² The study derives its immediate persuasive power and particular appeal for the media and larger groups of interested persons from its unusual methodological approach. For the authors rely primarily on scientific analyses; quantitative methods are even preferred in those passages that deal with the evaluation of contemporary written testimonies. A qualitative, interpretive evaluation of the texts is no longer undertaken. This new methodological approach is accompanied by massive criticism of previous research, which is accused of dealing with the material too lightly. Historians, it is claimed, “tend to read plague passages positivistically and to accept late antique claims and commentary on the JP at face value”³.

Less striking and sensationalist, but more profound in its arguments is a parallel publication put forward by the same authors.⁴ However, since this article has not attracted the same amount of media attention and contains qualitative discussions of the material in addition to the quantitative arguments, it will not be the focus of the present text, which is intended in particular as a critical examination of the general recent trend to market research in the humanities more strongly in the media by clothing it in a scientific garment and placing it in scientific publication organs.

Has the methodological paradigm shift solved the mystery of the ‘Justinianic Plague’ and its evaluation? Do the scientific analyses suggesting accuracy and objectivity now enable us to make a final assessment of the pandemic and its consequences? I am scep-

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² Mordechai et al. (2019), 25553.

³ Mordechai et al. (2019), 25547.

⁴ Mordechai/Eisenberg (2019).
tical in this respect and would like to take a closer critical look at the methods applied by the authors.

Lee Mordechai and his colleagues specifically target the “maximalists” who have claimed (in their view) “that the Justinianic Plague […] caused tens of millions of deaths throughout the Mediterranean world and Europe, helping to end antiquity and start the Middle Ages”.

This, however, is misleading, because in the following the “maximalist position” is defined as

a broad consensus […] across disciplines that estimates JP mortality in the tens of millions, with numbers ranging between 15 and 100 million, or alternatively, 25 to 60 % of the estimated population of the Late Roman Empire.

This definition by no means refers to an exact research position, but rather generalizes a very large, internally finely differentiated spectrum of different perspectives and approaches. Yet, it should make a considerable difference whether the mortality rate for the ‘Justinianic Plague’ is given as 25 % or 60 % – in addition, the period under consideration should have to be asked: Is it only the first plague wave 541/42 or the entire phase in which the pandemic is detectable, i.e. about two centuries? Also a spatial differentiation would be necessary. And finally, it should be pointed out that in the concert of the historians sceptical voices can certainly be heard, whose weight should not be underestimated (e.g. Mark Whittow, Chris Wickham, Peter Heather, Bryan Ward-Perkins).

5 Mordechai et al. (2019), 255–46.
6 Mordechai et al. (2019), 255–46; see also l. c., 25553: “The consensus maximalist position asserts that the JP reduced the population of the late antique Mediterranean and Europe by more than a third, killing tens of millions and ending antiquity”. In the conclusion (25553) they mention a “current maximalist estimate of a 50 % demographic loss”.

7 The literature on the Justinianic Plague has grown rapidly in recent years. In the following I will mention only a few contributions that play or have played a major role in the discussion. For a more detailed orientation I would like to refer to the recent research reviews of Stathakopoulos (2000) and Eisenberg/Mordechai (2019); Russell (1968); Biraben (1975/76); Biraben/Le Goff (1975); Allen (1979); Bratton (1981); Seger (1981); Conrad (1986); (1994); (1996); Leven (1987); (1995); Durliat (1989); Robin (1992); Harrison (1993); Kislinger/Stathakopoulos (1999); Atkinson (2002); Sarris (2002); (2006); (2007); McCormick (2003); Schamiloglu (2004), 255–260; Meier (1999); (‘2004), 321–340; 373–387; (2004a); (2005); (2016a); (‘2020), 817–848; 892; 943–945; 957–975; Meier (forthcoming); Stathakopoulos (2004), problematic for several reasons, see my review in: Byzantinische Zeitschrift 97, 2004, 627–629; (2012); Horden (2005); Arjava (2005); Little (2007); (2011); Sallares (2007); Kaldellis (2007); Leppin (2011), 206–215; 240–242; Harper (2017), 206–287; Gruber (2018). Rather for a public audience and not free of mistakes: Rosen (2007).

8 Older extreme positions (e.g. Russell [1968]: 40–50 % between 541 and 600; Allen [1979], 11: 57 % during the first plague wave 541/42; Leven [1987], 151: 50 % by the mid of the 8th c.; Evans [1996], 160: 60 %) have long been overcome by research (see e.g. Horden [2005], 159: 20–30 % during the entire pandemic, i.e. till about 750; Stathakopoulos [2012], 109: about 20 % in Constantinople in 542; an exception is Harper [2017], 244: 50 % demographic decline during the first wave). I myself still consider a reduction of the population of the eastern Mediterranean by about 25 % between 541 and 544 to be plausible, see Meier (2005), 94.

9 Whittow (1996), 66 f.: “there is no evidence for this devastating impact […] business as usual”; Wickham (2006), 548 f.: “the sixth-century plague, however dramatic its local incidences, was a marginal event in the demographic history of our period”; Heather (2018), 307: “But there is little convincing archaeological evidence for any major disruption to the fabric of the East Roman agricultural economy in the mid-to late
Anyone who ignores all these internal differentiations within research and instead speaks of a single “maximalist position” as “consensus” is making rough simplifications that do not correctly reflect the current state of the discussion. Moreover, the supposed research consensus is by no means that plague mortality is alleged to have depopulated the ‘known world’ and to have accelerated the transition to a ‘backward’ ‘Dark Age’ period, devastating the Late Roman Empire and extinguishing antiquity.

Rather, this is an extreme position within a wide spectrum, which certainly includes gradations that are still under discussion. For example, despite the criticism of Mordechai et al., I myself would still adhere to the thesis that ‘Justinianic Plague’ was an important factor in the transformation process between Late Antiquity and the Early Middle Ages (see below), without consenting to the exaggerated position of Kyle Harper. The difficulty ultimately lies in assessing what role the epidemic may have played in concrete terms, particularly in combination with numerous other factors. Our task is to describe and analyze a highly complex transformation process; whether scientific or quantitative methods and the narrowing of their results to monocausal approaches implying determinisms – be they positive (Harper) or negative (Mordechai et al.) – can prove effective remains to be doubted.

**Literary Sources**

This applies in particular to the handling of the literary tradition. In order to prove the problematic nature of contemporary texts, Mordechai and his colleagues first reformulate well-known reservations against an interpretation of the plague as a historical caesura, which had already been put forward in the 1980s (without mentioning Jean Durliat’s publication, which is central in this respect), but which in the meantime were considered to have been dispelled. They concern the low presence of the plague in contemporary literary texts and its almost complete absence in non-literary documents.

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10 Similarly undifferentiated: Mordechai/Eisenberg (2019), 5 f.: “Drawing heavily on the catastrophic Black Death model established by recent scholarship, estimates for the mortality of the Justinianic Plague range between 33 and 60 per cent of the Mediterranean population. Scholars estimate catastrophic death tolls such as fifteen, twenty-five, or even one hundred million people. We refer to this paradigm as the ‘maximalist interpretation.’ […] practically all the relevant scientific publications, despite their contribution to our understanding of the Justinianic Plague, uncritically assume the maximalist interpretation and cement it as fact in scholarly discourse”.

11 Mordechai et al. (2019), 255-56.

12 Taken by Harper (2017). But see the criticism put forward by Haldon et al. (2018a); (2018b); (2018c).


14 See Biraben (1989); Stathakopoulos (2000), 270; Sarris (2002); (2007); Meier (2016a).
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(inscriptions, papyri, legislation, coins) as well as the archaeological evidence. Literary texts, it is stated, “contain significant biases that must be contextualized”\(^{15}\). However, exactly this requirement is not fulfilled in the further course of the discussion; instead, purely quantitative aspects are emphasized. With reference to the exaggerated number of victims in contemporary accounts,\(^{16}\) research is generally accused to be naïve positivism.\(^{17}\) A closer look at the literature, on the other hand, would have quickly made it clear that historians have long since ceased to uncritically adopt the ancient figures as a basis for quantitative calculations (as was still sometimes the case in the 1970s),\(^{18}\) but have long since interpreted them as a manifestation of the quality of the suffering experienced by fellow survivors\(^{19}\) and have accordingly been struggling intensively with the interpretation of each individual text.

Furthermore, we have to put into perspective the following objection that contemporaries had mentioned various accidents “such as earthquakes and relatively minor volcanic eruptions”, but not the plague.\(^{20}\) In the 6th century – and this is what I will concentrate on in the following, as the first and probably most momentous plague wave fell in the years 541/42 – people were particularly sensitized to unusual developments, natural phenomena, catastrophes and accidents. This was the result of specific expectations of the Last Days, which, based on widespread chronological calculations, condensed in the years around 500.\(^{21}\) The fact that in this phase also those events were mentioned to which less importance is attributed from an ex post perspective should not surprise anyone at first. One of these events was an 18-month solar eclipse, which was anything but a mere consequence of a “minor volcanic eruption”, but the result of a gigantic volcanic event, the traces of which can be detected throughout the entire northern hemisphere.\(^{22}\) The ‘Justinianic Plague’ was undoubtedly a culmination in the continuing series of accidents and catastrophes that swept across the Eastern Roman Empire in the 6th century,\(^{23}\) and it was negotiated with appropriate intensity. For contrary to the assertion of Mordechai et al., it is quite present in almost all contemporary texts in which one would expect

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15 Mordechai et al. (2019), 25547.
16 Procopius states in the Bella (Prok. BP 2,23,1–2) that the plague had spread in Constantinople alone for four months and caused 5000, and eventually even 10,000 or more deaths daily; in his Anekdota he draws a disaster scenario for which Justinian is blamed, finally concluding that the plague has killed half of mankind (Prok. HA 18,44). The figures in the work of John of Ephesus are similarly unbelievable: 16000 victims per day; with 230000 dead the imperial officials would have given up counting (Joh. Eph. in the Chronicle of Zuqnīn p. 87 Ed. Witakowski).
17 See similarly Mordechai/Eisenberg (2019), 7: “The direct evidence is almost entirely literary, is frequently exaggerated, and is quoted uncritically”.
18 E.g. Russell (1968); Allen (1979).
20 Mordechai et al. (2019), 25547.
21 See comprehensively Brandes (1997) and Meier (2004); furthermore Meier (2002b); (2008).
23 For a catalogue of catastrophes see Meier (2004), 656–670.
a corresponding mention (not, of course, in the bulk of other texts); this applies to the historical work of Procopius, the report of John of Ephesus, the Chronicle of John Malalas, the church history of Euagrius as well as various smaller chronicles and their later derivatives. When, in order to prove the contrary, it is pointed out that the search in electronic databases has produced only a small number of lexical references, it must be countered, on the one hand, that the quantitative method cannot replace the qualitative argument: It goes without saying that the majority of the databases contain texts in which, due to the genus, topic or interests, no mention of the plague would have been expected anyway; a simple offsetting of this mass of texts against the existing evidence cannot therefore lead to valid results. Or did the Covid-19 pandemic of 2020 have very little impact because it did not play a role in the vast amount of esoteric literature, cookbooks, romance and crime novels produced during this period? On the other hand, a prudent application of quantitative methods would have presupposed, first of all, a reflection on the situation of transmission. For the material available to us represents only a little fraction of the extensive literary production of the 6th century, and the question of the extent to which this fraction can be considered representative of the whole was not raised by the team of authors around Mordechai. However, the mere fact that John Lydus, for example, announces a separate chapter on the plague in the summary of his work De magistratibus, but that this has not been handed down, should be a warning to be more careful when dealing with quantitative methods in literary texts.

By the way, this also applies to single findings: The fact that Procopius dedicates only 1.0 % of his work to the plague, as the authors calculate, certainly does not say anything about a general ineffectiveness of the epidemic, but rather about Procopius’ thematic priorities; the same applies to Gregory of Tours, who is said to have dealt with the plague in 1.3 % of his work. Similarly the fact that contemporary chronicles often deal with the plague in only one sentence is hardly a valid argument against the excessive importance of the epidemic, since this form of recording of events results from the basic structure of ancient and medieval chronicles; to argue with the length and shortness of single entries simply does not make any sense for this genre. Anyway, one should be cautious: For example, Malalas’ original report may have been much more extensive and, like large parts of the rest of his Chronicle, may have fallen victim to far-reaching cutbacks in the course of the transmission process. In any case, an intensive qualitative

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26 On Gregory’s plague accounts, see Bachrach (2007).

27 See, however, Mordechai/Eisenberg (2019), 8.

28 At least, there are still some entries left: Malal. p. 407,95–96 Ed. Thurn; p. 407,12–19 Thurn; p. 418,41–42 Thurn; p. 420,71–74 Thurn; p. 422,41–42 Thurn (= Theoph. a. m. 6053 p. 235,10–11 Ed. de Boor). Morde-
examination of those 1.0% and 1.3% would have been far more effective than context-
less calculations of figures – or, as Mordechai et al. put it (without then carrying it out
themselves): “each passage should be examined critically, both within the context of
the work in which it is encountered and within the work’s particular cultural setting”. This
criticism may well lead to the conclusion that John of Ephesus “used the plague to
offer his audience a series of moral religious commentaries on sinful actions”. It does
not immediately follow from this, however, that the entire representation is historically
worthless. Finally, the fact that Theophanes comments on the plague wave of the year
732–35 in only one brief sentence, should not be misinterpreted as evidence of a lack
of significance: In the mid of the 8th century the reappearance of the disease, which had
been endemic since 541, simply did not cause too much sensation; nevertheless, Theo-
phanes considered it that important that he selected it to be the only event of the year
732 worth mentioning at all. One will therefore have to interpret the short Theophanes
sentence in exactly the opposite way than the authors suggest.

On closer examination, one can hardly agree with the authors when they state the
following as an interim conclusion: “The quantitative approach to the written evidence
suggested above reveals that the common maximalist JP narrative, which is based al-
most exclusively on the written evidence, was founded on weak premises”.

Justinian’s Legislation

Similar methodological objections one has to raise against the arguments that Mor-
dechai and his colleagues put forward with regard to the laws promulgated by Justi-

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29 Mordechai et al. (2019), 25547. Even Mordechai/Eisenberg (2019) do not achieve this to a sufficient extent. They dedicate a short section to the historiographer Procopius (ibid., 9–11), but his detailed report in the *Bella* is quickly disqualified (Prok. BP 2,22–23) with the laconic remark “The social effects of plague in his description, however, are partial and restricted to the capital” (9 f.). Instead, it is then shown in more detail that the mention of the plague in the *Anekdota* and especially the assertion that it had destroyed half of mankind (Prok. HA 18,44) – the latter is allegedly “frequently” (Mordechai/Eisenberg [2019], 10) cited by scientists (for which no proof is provided) – is worthless, since it has to be seen in the context of sharp criticism of the emperor. The latter, of course, has long been known; hardly anyone would take Procopius’ statements in the *Anekdota* seriously nowadays! However, it would have been worthwhile to deal with the description of the plague in the *Persian Wars*, which Mordechai and Eisenberg refuse, however.


31 As Mordechai/Eisenberg (2019), 12 f., suggest.

32 Theoph. a. m. 6125 p. 410,19–20 Ed. de Boor: τούτῳ τῷ ἔτει ἐτένετο θανατικόν ἐν Συρία, καὶ πολλοὶ ἀπέθανον (“In this year there was dying in Syria, and many people died”).

33 Mordechai et al. (2019), 25547.
Jean Durliat had already pointed out in 1989 the (supposed!) strange silence on the plague of the Novels; in the meantime, however, it has been shown that there are several allusions to the epidemic in the legal texts. Thus, for instance, in the 7th edict of March 1, 542, reference is made quite bluntly to the “encompassing presence of death in all places” (ἡ γὰρ εἰς πάντας τοὺς τόπους διελθοῦσα τοῦ θανάτου περίστασις) – a clear reference to the plague, which by the way had reached Constantinople not only in spring 542, as I have shown elsewhere. Also the 122th Novel of 23 March 544, with which the government tried to counteract the price increases resulting from hunger crises, addresses in the Praefatio unmistakably (and by no means “obliquely”) the plague as the cause of the disaster (paideusis). It is also incorrect to claim that “it refers to the capital rather than the rest of the empire, implying that any broader effects were limited”, since in the epilogue the legislator does not only address the city prefect of Constantinople, but also another authority not mentioned by name, which – what parallel formulations suggest – is probably the praefectus praetorio Orientis. The law was therefore conceived to apply to the entire Roman East. Finally, Novel 77 is indeed undated, but can be placed in the period from ca. 542–550/51 with good arguments. The fact that the plague (λοιμοί) is mentioned here in a series with hunger and earthquakes does not speak against its relevance, but points out once again that it was embedded in a longer series of catastrophes. In this context, also those legal testimonies that indirectly refer to the effects of the plague should not be overlooked. The 128th Novel (a. 545), for instance, regulated the so-called epibolé, i.e. the allocation of land that had been abandoned (presumably due to plague losses) to certain persons, who thereby also had

34 Mordechai/Eisenberg (2019), 20 ff., in explaining their scepticism concerning the non-literary sources.
36 In this direction, however, point Mordechai/Eisenberg (2019), 21, n. 66 (following the general consensus and a mistaken statement in Prok. BP 2,22,9). If there is an explicit reference to the plague in a law of 1 March 542, the following argument is surprising: “The four laws issued from April onward are not concerned with plague” (Mordechai/Eisenberg [2019], 21). One gets the impression that investigation periods are defined here arbitrarily in such a way that the desired results can be derived from them. Why was the year not counted from 1 January?
37 See Meier (’2004), 326; (2005), 92 f.
38 Mordechai/Eisenberg (2019), 22.
39 Nov. Iust. 122 pr.: Ἐγνώμεν ὡς μετὰ τῆς παίδευσιν τήν κατὰ φιλανθρωπία τοῦ δεσπότου θεοῦ γενομένην […] (“We have recognized that after the chastisement, which was due to the humanity of God the Lord […]”).
40 Mordechai/Eisenberg (2019), 22.
41 Nov. Iust. 122 epil.: Ταῦτα δὲ ζητεῖσθαι καὶ ἐκδικεῖσθαι κελεύομεν παρὰ τῇ τῆς σῆς ὑπεροχῆς καὶ τοῦ ἐνδοξοτάτου ἐπάρχου ταύτης τῆς εὐδαίμονος πόλεως (“We order that this will be investigated and punished by your excellency and the most illustrious prefect of this fortunate city”).
42 So Miller/Sarris (2018), II 801, n. 1, referring to Ed. Iust. 13.
43 In detail demonstrated by Meier (’2004), 591–599.
44 See Nov. Iust. 77,11: διὰ γὰρ τὰ τοιαῦτα πλημμελήματα καὶ λιμοὶ καὶ σεισμοί καὶ λομοί γίνονται (“Due to such crimes hunger, earthquakes and diseases do arise”).
45 But see Mordechai/Eisenberg (2019), 22.
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to bear the tax burden lying on the land— a procedure about which Procopius also bitterly complained.46

Mordechai and his co-authors, however, once again sharpen the quantitative argument by opposing the thesis that there is a connection between the significant decrease in legislative activity since 543 and the outbreak of the plague.48 However, their approach has methodological weaknesses in this respect as well: On the one hand, they fail to recognise the real amount of Justinian’s legislation up to the end of the 530s, since they only refer to the Novels promulgated after the publication of the second edition of the Codex Iustinianus (534) (although these alone add up to the stately number of 106 of the 155 Justinianic Novels that have survived);49 but if we add the 403 constitutions of Justinian50 from the Codex (and I see no reason why they should be excluded from a quantitative list), we arrive at a total of 509 (!) laws passed before the year 540. They are compared with only 46 Novels from the years 540–565.51 The fact that, in quantitative terms, a caesura must be stated for the beginning of the 540s can therefore hardly be denied. Mordechai et al. explain the extremely high legislative activity of the years 535–539 by the fact that after the publication of the Codex Iustinianus (534) various additions and corrections to the codification work were necessary. With this, however, the entire Novel legislation of the second half of the 530s is in fact reduced to an appendix of the Codex Iustinianus – a thesis that can be argued, but which does not plausibilize why the Codex itself already contains a conspicuously high proportion of Justinianic constitutions, and which would have required a detailed justification and foundation from the source material – which, however, is not provided.52 Instead, the authors only put an assertion into the room, which proves to be problematic even on a superficial glance at the contents of the Novels: It becomes clear that Justinian took up various very fundamental reform themes in the constitutions of the second half of the 530s, which can hardly be explained as a mere completion or correction of the Codex Iustinianus – for example, the complex provincial reform legislation of the years 535/36. Here it is clearly evident that new fields were taken on.53 In contrast to this, since the early 540s the thematic priorities of Justinian’s political activity as a whole have shifted towards the area of church and religious policy;54 the quantitative decline thus corresponds – what the authors ignore – to a fundamental reorientation and for this reason alone must be taken seriously. Even less plausible, however, is the subsequent assertion that the decline in published laws in the 540s was due to the fact that there was no longer any need for

46 Teall (1965), 318; Sarris (2002), 177; (2007), 129.
47 Prok. HA 23,15–16.
48 This interpretation has recently been advocated once again by Harper (2017), 235. See also Sarris (2002), 174 ff.; (2006), 219 (states the quantitative evidence); (2007), 126 ff.
49 For the numbers see Meier (2004), 103 f.
50 Ebrard (1947), 44. Noethlichs (1999), 704 ff., has a different sum: A total of 4680 entries of the Codex Iustinianus contain about 360 Justinianic provisions from about 290 laws.
51 For the years 547, 549, 550, 557, 560, 561, 562 and 564 there are even no laws handed down at all.
52 There is only a general reference to Honoré (1978).
53 For this see Maas (1986).
54 For this see in detail Meier (2004), 234–306; see also Leppin (2011), 276–280; 284–288; 293–308.
regulation.\textsuperscript{55} On what basis, one wonders, would one like to judge this, especially since it was the emperor and his advisors who decided what had to be regulated and what not in their perception?

Finally, Mordechai and his colleagues emphasize the fact that the number of surviving Novels (the question of the tradition of the Novels is another issue that should be discussed separately) decreases already from the year 540 and not only from the plague year 541.\textsuperscript{56} But this is likewise hardly suitable as an argument for relativizing the epidemic events, but first of all refers to the fact that the epidemic, as indicated, was embedded in a larger catastrophic scenario, which had already come to a threatening head in 540,\textsuperscript{57} when the Persians unexpectedly invaded the Roman East and quickly advanced as far as Antioch.\textsuperscript{58} Historical contexts such as these must also be taken into account when discussing quantitative aspects of legislation. The figures alone do not allow a valid interpretation of what happened.

\section*{Papyrological Evidence}

With regard to the papyrological tradition, Mordechai and his colleagues also formulate arguments that have been discussed since the 1980s.\textsuperscript{59} The finding that (1) the quantity of dated papyri from the years 520–570 shows no significant increases or reductions, that the material (2) contains no indications of population losses, the abandonment of land or tax reductions, that (3) no known papyrus refers directly to the plague, and finally that (4) even the extensive documents of the Apion archive show no evidence of economic stress, is once again taken as an indication, “that plague had no discernible economic effects”.\textsuperscript{60} However, also in this case it remains questionable how viable the quantitative method is. As is well known, the papyrological evidence for other epidemics during the principate, such as the ‘Antonine plague’ of the 2nd century, is also quite limited; epidemics seem to have played no significant role in those texts that were preferably recorded on papyrus, or to have been reflected only very little in them.\textsuperscript{61} In addition, it must be stated that, despite the comparatively large number of preserved papyri, the state of tradition is far too incomplete to allow plausible conclusions to be drawn from just what is not mentioned; in any case, the papyri do not provide usable

\textsuperscript{55} Mordechai et al. (2019), 25548: “Both immediately before the plague outbreak and afterward there would have been fewer pressing reasons to issue more laws”.
\textsuperscript{56} Mordechai et al. (2019), 25548, with fig. 2; Mordechai/Eisenberg (2019), 21.
\textsuperscript{57} So I have interpreted the evidence several times, see Meier (2004), 307–341.
\textsuperscript{58} Most recently Leppin (2011), 223–229.
\textsuperscript{59} Durliat (1989), 109 f. For the lack of explicit evidence about the plague in the papyri see also Casanova (1984), 199.
\textsuperscript{60} Mordechai et al. (2019), 25548.
\textsuperscript{61} See Meier (forthcoming). There are a few exceptions, which are, however, hotly debated: P.Thmouis 1,104,10–18 (S. Kambitsis [Ed.], Le Papyrus Thmous 1, colonnes 68–160, Paris 1985, 98); SB XVI 12816, with van Minnen (1995). See also van Minnen (2001).
serial data, as is available for late medieval or early modern regions and places. From a methodological point of view, this means that mentioning the epidemic or at least implicit references to it should be of far greater importance than stating negative evidence; especially in the papyrological field, the already problematic argumentum e silentio is thus particularly fragile and susceptible to criticism.

But the papyrological material does indeed provide implicit clues. Apparently, people in Egypt began to conclude long-term leases in the middle of the 6th century, which in turn reflects a change in the assessment of the value of agricultural land and may be related to the experiences during the plague; if this thesis were correct, it would not only indicate the great importance of the epidemic as a whole, but also its penetration into rural, less urbanised regions.

**Inscriptions**

As is well known, the ‘Justinianic Plague’ is only twice explicitly mentioned in inscriptions – a reason for Mordechai and his colleagues to critically examine the epigraphic evidence as well. Despite the methodological problems of a purely quantitative analysis, which are at least briefly touched upon, the authors also see a clear picture here: “Neither a sharp decline in inscription production reflecting demographic collapse, nor a slow decrease in inscription production that could reflect economic decline”. While in the East a clear caesura only became manifest around 610, which the authors explain by the extensive Persian and subsequent Islamic conquests of Roman territories, regarding the Latin inscriptions of the West one should note: “no immediate plague effect was observed”.

A temporary decline in the 550s must “probably be seen as a random outlier”, and otherwise one has to state: “temporal correlation is insufficient to confirm

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62 In this sense also Sarris (2002), 174; (2007), 126: “While it is true that the papyrological evidence for Egypt furnishes the social and economic historian of Late Antiquity with much fascinating and rewarding material, it nevertheless remains highly fragmentary. The papyri simply do not provide the economic historian with the sort of population cohorts, traceable through time, such as the documentary sources for late medieval England provide and on which historians of the Black Death are so reliant”.


64 The first document comes from Zora (modern Azra’a, Syria), Koder (1995), 13: † οἱ ἀπὸ ζορ(ᾶς) ἐξ ἰδίων ναὸν ἱλισσὸν γεννήθηκεν ἐν ἔτει υλζ᾿ / ἐκτισαν ἐπὶ οὐάρον θεοφ(ιλεστάτου) ἑπίσκοπον | ᾧ ἐπήγαγ(ε) ὁ θ(εὸ)ς πότμον βονβῶνος (καὶ) μάλῃς – “The people of Zora built a church of the prophet Elijah with their own means, on the initiative of the deacon John, son of Menneas, in the year 437, under Varus, God’s most beloved bishop, to whom God taught the deadly fatality of the groin and armpit”.

A further inscription comes from Aphrodisias in Caria (Turkey) – a honorary inscription for a certain Rhodopaios, who is praised since he had expelled disease and hunger (λοιμὸν καὶ λιμόν), see Roueché (1989), 137, No. 86.

65 Mordechai et al. (2019), 25548 f.

66 Mordechai et al. (2019), 25549.
a connection”. This methodological guiding principle, however, does not prevent the authors from interpreting the decline of the inscriptions in Rome after 543 without comment as a consequence of the Eastern Roman-Gothic War. Here, evidence that is by no means clearly negative – the study by Nancy Benovitz, which was not taken into account, points to significant increases in grave inscriptions in Palaestina and Arabia, which can be plausibly correlated at least with the first plague wave in 541, probably also that of the year 592 – is levelled with contradictory arguments. More important, however, would have been to discuss the question of how the material should be interpreted in the context of the “epigraphic habit” of a given society. Durliat also did not address this problem (which, of course, was not recognized in research until later), so little weight can be attached to his conclusions as well. In addition, there is the problem mentioned by Procopius of burying a rapidly increasing number of victims, which also assumed depressing proportions in several countries (Italy, Spain, USA) during the so-called Corona Crisis of 2020, and also shows many parallels (not only) from the late Middle Ages. It cannot therefore be relativized exclusively as a topical exaggeration or dramatic distortion by the historiographer, but deserves to be taken seriously: The conditions must have been chaotic in some regions at times. In such situations, it was no longer possible to think of writing elaborate epitaphs; one simply had to cope with other problems. Moreover, as John of Ephesus rightly remarks, the plague (at least at first) mainly affected the poor, who could not have afforded to write epitaphs anyway. So the fact that the plague is not strongly reflected in the epigraphic evidence does not mean anything at all.

Coins

A similar assessment will be reached with regard to the numismatic evidence. In this field, too, Mordechai and his colleagues have a clear opinion: “Numismatic evidence

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67 Mordechai et al. (2019), 25549.
68 Mordechai et al. (2019), 25549.
69 See Benovitz (2014). Also the increase of epitaphs in Gaza and in the Negev during the year 541, which has been known for some time (see Benovitz [2014], 492, with note 26) is not discussed (Mordechai/Eisenberg [2019], 25, at least mention it briefly, but only as an example for “circular reasoning”). Di Segni (1999) had already pointed out an equally significant decrease in building inscriptions in the same region between 540 and 550.
70 Prok. BP 2,23,3–12.
71 See Bulst (1979), 61.
72 In this sense see also Horden (2005), 154; Sarris (2002), 174; (2007), 126; Benovitz (2014), 490. But see Mordechai/Eisenberg (2019), 24, claiming that only Procopius mentions the burial problem; against this, I would like to point to Malal. p. 407,14–15 Ed. Thurn: ἐπεκράτησεν γάρ ἡ θνήσις ἐπί χρόνον, ὡστε μὴ αὐταρκεῖν τοὺς θάπτοντας (“For a while there was such a dying that the undertakers were no longer sufficient”).
73 Joh. Eph. in the Chronicle of Zuqnin p. 86 Ed. Witakowski, with Horden (2005), 154. This was a consequence of the poorer hygienic living conditions, which encouraged the spread of rats (and thus fleas). However, these circumstances only delayed the spread of the plague to the more affluent.
for the JP’s impact on economic relations is also nonexistent”. This statement is based on two quantitative surveys, for which the question of representativeness is not discussed further: The cessation of minting in Antioch in the 540s is exclusively justified by the incursion of the Persians, whereas the anything but clear evidence for Berytus is only briefly commented with the words “no visible drop associated with the plague”. The apocalyptic scenario that John of Ephesus sketches, thus the conclusion drawn on a narrow material basis, cannot be reconciled with the numismatic evidence in any case.

**Palynological Data**

With the evaluation of palynological data Mordechai and his colleagues are breaking new ground with regard to the ‘Justinianic Plague’. Under the question of whether the palynological data point to similar devastating consequences as in the case of the Black Death in the late Middle Ages, analyses of selected places in the Balkans (Greece, Bulgaria) and Turkey are presented. It is shown that already existing trends in the use of agricultural land continued and that no excessive advance of pine trees – an indicator for the abandonment of previously used arable land – can be observed. Deviations (Northern Turkey) must be considered “insignificant”, because – a strange argument – they could not be correlated with written sources on the plague. The fact that John of Ephesus claims to have seen completely deserted villages is therefore irrelevant to the authors. Instead, they refer to the specific evidence of the Nar Gölü in Cappadocia, whose sediments do not show a clear break until the end of the 7th century.

In view of the fact that palynological analyzes are an innovative approach in the context of research on the ‘Justinianic Plague’, one would have wished for at least a few explanatory notes on possible methodological problems and the significance of the results. For problems also arise for this supposedly objective procedure: On the one hand, the authors have been able to evaluate a larger number of places for their questions, but these – like the literary sources – remain selective, and the question of their representativeness requires intensive discussion, especially when results that do not confirm the supposed trend are unceremoniously described as “insignificant”. On the other hand, it is not always clear whether the places where pollen material suitable for analysis is found are also identical with those regions where land was actually used for agricultural purposes in late antiquity. Therefore, palynological analyses can provide first indications, but they need to be embedded in further – not least literary – source material. Against this background, the statement that

74 Mordechai et al. (2019), 25549.  
76 Mordechai et al. (2019), 25550.  
palynological data from several eastern Mediterranean regions [...] not only fail to support the maximalist interpretation of the JP, but provide evidence for rejecting the 14th century Black Death as a model for the 541 outbreak of the JP.\(^78\)

does not allow any conclusions about the effects of the plague beyond the current state of research.

**Ancient DNA**

The authors become rather vague in the discussion of recent Yersinia pestis DNA evidence, which has now been found in about 45 individuals from late antique Central and Western Europe. Here we first encounter familiar patterns of argumentation again to confirm the thesis that the evidence “fails to support the claim that the JP significantly impacted population levels”.\(^79\) For example, the sceptical attitude is based on the fact that only a few Yersinia pestis finds can be correlated with contemporary literary accounts – but why should they have to be? It would have been far more plausible to interpret this very fact as an indication that the epidemic has also spread into areas for which it was not previously documented in the written accounts (e.g. present-day Bavaria; the cities of Vienne, Sens and Poitiers; southern England; Valencia).\(^80\) The remarkably high number of mass graves in the 6th/7th century (ca. 40),\(^81\) compared to the 5th and 8th century, is disqualified as “minuscule tally” without further explanation.\(^82\) Later, however, in view of an increase in mass burials in Britain since the 4th century and a “gendered patterning” in the grave findings, the importance of “social identit(ies)” for burial practice is emphasised.\(^83\) Nevertheless, this aspect is not discussed in any further depth, but again only used to justify the contention that “the claim that trends in multiple burials evidence plague mortality in late antiquity is dubious”.\(^84\)

However, the biological arguments with which a significantly lower mortality rate of the ‘Justinianic Plague’ compared to the Black Death is to be justified are particularly problematic, in my view. These include, above all, the absence of “descendants” of the late antique Yersinia pestis genome in current material, whereas the genome of the

\(^{78}\) Mordechai et al. (2019), 25551.

\(^{79}\) Mordechai et al. (2019), 25551.


\(^{81}\) See McCormick (2015), 356; (2016).

\(^{82}\) Mordechai et al. (2019), 25551.

\(^{83}\) Mordechai et al. (2019), 25552. For further criticism see also Eisenberg/Mordechai (2019), 167–169.

\(^{84}\) Mordechai et al. (2019), 25553.
Yersinia strain that caused the Black Death is closely related to that of the causative agent of the third pandemic (ca. 1894–mid-20th century; East Asia was particularly affected) and current Yersinia genomes – from which the authors conclude:

That currently sequenced Y. pestis strains associated with the JP and the First Pandemic are not directly ancestral to later strains could mean that late antique plague was not established in multiple, long-lived, or highly active reservoirs in any region. […] Notably, the fewer, shorter lived, and less active the reservoirs, the less human plague there will be.85

The lack of biological relationship between the ancient plague pathogen and currently active pathogens is thus used as an argument against a comparably high lethality of the disease, without taking into account specific living conditions and possibly different susceptibilities and dispositions of the respective contemporaries. In the following, it is combined with the reference to a possible instability of the late antique Yersinia pestis genome – an “attenuation” that “could have minimized the number of successful transmissions (i.e., virulence) and, therefore, the number of potentially infected individuals”.86 As speculative and hypothetical as these and subsequent considerations on possible “transmission mechanics” are – even they are put in the service of the overarching thesis that “in the available DNA evidence itself one cannot find confirmation or support of maximalist readings.”87 Nevertheless, they do not provide a reliable reason for the postulated “minimalist reading” – nor do the preceding arguments.

**Concluding Remarks: “Maximalists”, “Minimalists” and a Historical Perspective**

What results from the statements of Mordechai and his colleagues? There is no doubt that they have succeeded in showing that statements on the effects of the ‘Justinianic Plague’ – and here they seem to be particularly concerned with the demographic aspects, since these can still be quantified most plausibly88 – can only be made with great reservations because of the uncertain sources and evidential basis. Their criticism of the “maximalist position”, however, is misleading because this label summarises a broad spectrum of interpretations in a generalised way and does not take account of differentiations within the research landscape. As shown above, however, even the critique itself, which at first glance pretends to deconstruct the “maximalist position” but actually promotes a “minimalist position”,89 proves to be methodologically highly questionable.

85 Mordechai et al. (2019), 25551.
86 Mordechai et al. (2019), 25551.
87 Mordechai et al. (2019), 25552.
88 See Eisenberg/Mordechai (2019), 173: “the central question driving all plague studies”.
89 See Mordechai et al. (2019), 25553: “We contend that this is sufficient evidence to reject the current scientific and humanistic consensus of the JP as a major driver of demographic change in the 6th century Mediterranean region. […] Based on the evidence presented above, we believe that the JP and the so-called ‘First Pandemic’ bear comparatively little resemblance to the Second Pandemic and the Black Death, which significantly affected the demography, economy, and landscape of western Eurasia and North Af-
and attackable, since the quantitative approach cannot replace a qualitative analysis and contextualization of the written testimonies in particular.\textsuperscript{90} In the controversially discussed question about the demographic effects of the plague, Mordechai et al. do not make any offer of their own on the attempt to deconstruct older positions. Above all, however, they overlook the fact that the question of the effects of a pandemic can hardly be limited to quantifications and considerations of demographic aspects, but rather leads to the broad field of cultural consequences. Only the embedding of the ‘Justinianic Plague’ in a multifactorial overall context, only the discussion of the question of what a pandemic does to (or makes with) the affected societies can provide information about longer-term consequences – and only against this background can the question of the plague as a factor within the transformation process between Late Antiquity and the Early Middle Ages, which was only briefly touched upon (and negated) by Mordechai and his colleagues, be discussed in depth. My sketchy concluding remarks are dedicated to this complex, in which I would like to explain my own point of view – as I have already formulated it elsewhere (and which has already been criticized by Mordechai and Eisenberg).\textsuperscript{91} Before, it should be pointed out once again that the plague was by no means a single catastrophe in an otherwise quiet period, but rather the condensing moment of an extraordinary series of disasters extending over several decades, which for its part must be interpreted against the background of very specific eschatological expectations. This embedding of the plague in a larger catastrophic context alone makes a purely quantitative analysis focused on individual ‘plague years’ difficult. In contrast, there are four points that I consider to be particularly relevant in view of the cultural impact of the pandemic: (a) an increase in Marian devotion in the Roman East (which had long-term consequences), (b) the gradual unfolding of the cult of images, (c) a further intensified sacralization of the emperor as well as – vaulting over all these phenomena – (d) the encompassing process of liturgification of Eastern Roman Byzantine society.

(a) Of course, the devotion to the Mother of God did not only develop in the context of the plague epidemic of the years 541/42, but already had a longer history.\textsuperscript{92} The Council of Ephesus 431, which established the role of Mary as the Bearer of God, was an important stage in this process, whereby (quite existing) older forms of Marian devotion began to develop a new dynamic.\textsuperscript{93} From then on, a continuous increase and differentiation of Marian miracles, Marian festivals and Marian
legends can be observed, for which not least the Kontakia sung in church services – and thus in front of large audiences – by the popular poet Romanus Melodus, who was active in the first half of the 6th century and himself was considered a singer inspired by Mary, are fine examples.\textsuperscript{94} The growing devotion to Mary is particularly well documented for Constantinople, which was considered the ‘City of the Mother of God’ in the 7th century at the latest: in 626, Mary is even said to have faced the Avar and Slavic besiegers of the capital in person.\textsuperscript{95} However, this development was demonstrably accelerated in a special way in the 6th century,\textsuperscript{96} and the relevant testimonies repeatedly refer to the plague pandemic as a context. Particularly revealing in this connection is Justinian’s transfer of the feast of Hypapante (also known as ‘Marian Candlemas’ or ‘Presentation of the Lord’) from 14 to 2 February – just in the plague year 542. The transfer was – roughly simplified – accompanied by the conversion of an original Feast of Christ into a Feast of Mary with penitential character and, as can be seen from a Marian legend referring to it, was expressly done as a measure to appease the plague.\textsuperscript{97} This procedure was later considered so successful that the designated Pope Gregory I, who himself had spent several years in Constantinople and was therefore informed about the ‘successful’ coping mechanisms against the epidemic there, copied it in view of the severe plague epidemic in Rome in 590.\textsuperscript{98} Significantly, the ‘Black Death’ in the late Middle Ages also led to an intensification and differentiation of Marian devotion.\textsuperscript{99} The deep veneration of the Mother of God by the population of the capital and the empire, which also emerges in other testimonies (e. g. in the erection of Theotokos churches, such as the Nea in Jerusalem, consecrated in 543),\textsuperscript{100} distinguishes the medieval, the Byzantine Constantinople at any rate clearly from its late antique predecessor, and its intensification should therefore definitely play a role in the context of the discussion about the transformation process from antiquity to the Middle Ages – with what inevitably also points to the plague.

\textsuperscript{94} Arentzen (2017).
\textsuperscript{96} Important in this context are the articles of Cameron (1978); 2000.
\textsuperscript{99} See Bulst (1996).
\textsuperscript{100} References in Meier (’2004), 584–586; on the Nea see also Trampedach (2015).
(b) Significant changes in religious practice in the 6th century can also be observed in other areas, especially in the worship of cult images. This is all the more astonishing as the Christians initially rejected any form of image worship, as they suspected the survival of pagan practices in it. Since the middle of the 6th century, however, an increasing number of written testimonies confronts us with venerated icons, both in the private and public spheres. The image of Christ that is said to have saved the city of Edessa from the Persians in 544 soon became particularly prominent; almost simultaneously, another image of Christ appeared in Kamulianai in Asia Minor, which quickly multiplied miraculously. Also the picture of Mary of Sozopolis attracted attention at that time, and one could point to other examples. If one takes a closer look at the circumstances described for the ‘appearance’ of all those images, a direct connection with the already mentioned severe catastrophes that struck the Eastern Roman Empire in the 6th century is suggested. Whole communities felt protected from foreign attacks by miraculous images, especially Edessa, the home of one of the most famous icons. According to this, it was mainly foreign attacks that gave decisive impulses to the cult of images, but the plague also seems to have been not without relevance. Saint Theodor of Sykeon, for example, is said to have been cured of the disease by an icon during the epidemic of 541/42. This makes it clear once again that the plague cannot be regarded as an isolated phenomenon; it drew its terrible impact in contemporary perception, among other things, from the fact that it was chronological – and, as many believed, causally – linked to other catastrophes and that, to emphasize this once again, it met with a very specific, eschatologically charged expectation.

(c) As already indicated, the phenomenon of a further intensified sacralization of emperor and empire should also be placed in this overall context. For whoever placed his salvation in the hands of new or until then not yet in the appropriate way evoked protective powers had obviously lost confidence in the traditional emergency helpers and intercessors – and among these of course also the emperor. For what help should one hope for from a ruler who himself fell ill with the plague like countless of his subjects? Cautious hints in the sources indicate that this led to criticism of the emperor, which in turn forced him to act: Not only did he demonstratively engage with the increasing Marian piety of the population by transforming the Hypapante into a Marian feast of penitential character, by institutionalizing the

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101 Details and references for the process that is sketched in the following are to be found in Meier (2004), 528–560.
103 Ps.-Zach. HE 12.4.
105 For the early cult of images von Dobschütz (1899) is still important; Kitzinger (1954); Cameron (1983). See also Brubaker/Haldon (2011).
106 VTheod. Syk. 8 p. 7 Ed. Festugiere.
107 Prok. BP 4.25,20; HA 4.1–3.
feast of the ‘Annunciation’ (Euangelismos) and by asking Procopius to remark in his praise of the imperial buildings that he was particularly eager that the historian should report in detail about the numerous Marian churches he had built. Above all he pursued a remarkable self-sacralization. Justinian himself now took on the role of holy men who, like Mary, were able to mediate between God and man, and propagated a similar way of life (asceticism) and similar characteristics (miracles) to those who helped in need. In ceremonial contexts his image was put next to the christian cross. And soon the image of Christ appeared, in the manner of a Roman emperor, next to Justinian. No one could now dare to accuse the emperor of lack of piety.

Justinian’s successors initially continued this sacral exaltation of the empire and thus institutionalized it. For Justin II, who ascended the imperial throne after Justinian’s death in 565, this can be clearly proven on the basis of Corippus’ eulogy. For the panegyrist the emperor is also a holy man. Under the later emperors, however, a regressive development is becoming apparent again, although a return to the conditions of Justinian’s early days was no longer possible. In this respect, the plague and its consequences for the emperor ultimately played a quite significant role in the process of the formation of the Byzantine empire.

The unfathomable dimensions of the disaster, its lack of explanation and the universally visible impact of the mass deaths had clearly shown their limits to the conventional emergency helpers of mankind, e.g. the Holy Men, but also to the representatives of the churches and – as indicated – not least to the government of the empire. With Mary and the icons, new protectors now joined the side of those affected by the plagues. In this respect the Christian religion – with a few exceptions – was ultimately not questioned in principle. But the forms of religious practice changed, new priorities were set, and this had serious consequences. These are connected with the keyword ‘liturgification’.

This is a phenomenon that is difficult to grasp analytically. The term refers to the fact that since the 540s religion has obviously played a much more important role than before, in all socially relevant matters. A considerable thrust of religious penetration of the whole of society – beyond the high significance of religion, which is already evident in late antiquity – is emerging at all levels that we can observe in our testimonies. The methodological problem of grasping religion in a quantifiable way can be solved by comparing texts of the same genre from the early and late Justinianic phase. While the former are still clearly oriented towards the classical-antique heritage and try to harmonize Christian religious contents with it, in

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109 Prok. aed. 1,3,1.
112 Cf. on this entire complex with further references Meier (2004), 608–638; (2016b).
113 Coripp. Laud. Iust. 1,175.
the later works those elements that are still in antique tradition increasingly recede into the background in favour of Christian symbolism. However, such a tendency can also be seen in other areas. Just compare the ceremony of the triumph over the Vandals in 534 with that of the triumphant imperial adventus in 559: While in 534 the reference to classical traditions was still part of the programme, in 559 there is no longer any trace of it; instead we observe a deeply religious ceremony.\textsuperscript{115} Imperial legislation also confirms this picture: the laws of the 530s are full of allusions and references to the great Roman past; since the 540s these elements gradually disappear. Even in art, corresponding tendencies can be observed. The art historian Ernst Kitzinger has repeatedly pointed out that Justinian’s reign, even from the perspective of his discipline, fell into two clearly distinguishable phases. Firstly, the attempt at a synthesis of classicist and Christian ideals; then, towards the end of his reign, the increasing emergence of rigid, religiously transformed Christian forms of expression.\textsuperscript{116} The development thus outlined finds its clearest expression in an act that has more than only symbolic significance: After 541, the consulate, the central office of the old Roman community, was no longer assigned. The plague had obviously pushed interest in it into the background.

Seen from today’s perspective, ‘liturgification’ seems to be a peculiar phenomenon that has the effect of petrification of an entire society. It was, however, of decisive importance for the continued existence of the Eastern Roman/Byzantine Empire, because it stabilized a society that had become staggered and simultaneously – which, of course, was not foreseeable at the time – equipped it for the struggle for survival against the Arabs since the 630s. It is important to remember the insecurity of the Eastern Roman population: The unpredictability of the plague, which, without any discernible logic, carried off the one, spared the other, completely wiped out one village, but left the neighbouring village untouched, must have driven contemporaries to despair.\textsuperscript{117} John of Ephesus wondered every morning whether he would live to see the evening.\textsuperscript{118} The different speeds at which the disease killed its victims also led to deepest uncertainty. Everywhere, therefore, the sources reveal desperate strategies to make what happened more plausible for contemporaries. They lie behind tendencies of collective repaganization\textsuperscript{119} as well as behind ideas according to which the plague has acted among people in the form of individual persons. Procopius reports that ghosts who had infected their victims by beating them were blamed for the transmission of the disease.\textsuperscript{120} John of Ephesus knows

\begin{footnotes}
\item \textsuperscript{116} Kitzinger (1984), 202 ff.
\item \textsuperscript{117} Prok. BP 2,22,8; Euagr. HE 4,29; Greg. Tur. Hist. 4,5. See Sallares (2007), 258–261.
\item \textsuperscript{118} Joh. Eph. in the Chronicle of Zuqnīn p. 80 Ed. Witakowski.
\item \textsuperscript{119} E. g. Joh. Eph. in the Chronicle of Zuqnīn p. 79–80 Ed. Witakowski.
\item \textsuperscript{120} Prok. BP 2,22,10.
\end{footnotes}
of ghost ships, manned by headless ghosts, which are said to have been seen in a city before the plague arrived.\textsuperscript{121} And again it is Procopius who tries to construct regularities by imagining the plague as a creature acting according to a plan and travelling through the countries, claiming, in the course of time, the due tribute from every region at its own discretion.\textsuperscript{122} Euagrius seeks to trace similar regularities by relating the episodic occurrence of the plague to the 15-year indiction cycles.\textsuperscript{123} The historian Agathias, on the other hand, considers the search for reasons for the ultimately inexplicable catastrophe to be futile, and Euagrius can only draw the fatalistic conclusion that the further course of events remains uncertain, for only God knows the causes and everything else.\textsuperscript{124}

If one looks at the historiography of those decades, a process emerges that can only be understood against the background of the general conditions outlined above: The classicizing Greek profane historiography, which we still encounter in the historical works of Procopius, begins to dissolve and converge with the church historiography into which it flows in the first half of the 7th century. A closer examination of the works in question reveals that the authors – beginning with Agathias – despair of their claim, derived from the tradition reaching back to Herodotus and Thucydides, to explain historical events plausibly by causal chains. Agathias, in particular, makes it clear that the events before him can simply no longer be explained.\textsuperscript{125} Everything is, as Euagrius states, handed over to the rule of God. But if this is so, then there is no longer any need for a classicist profane historiography. Accordingly, it ends – against this background, also as a consequence of the plague – in the early 7th century. At that time, however, ‘liturgification’ had penetrated all areas of Byzantine life, even warfare.\textsuperscript{126}

If one considers the latter aspects: the veneration of Mary, the cult of images, the sacralization of the emperor, the dissolution of the classicist profane historiography and – ultimately integrating all these phenomena – the comprehensive ‘liturgification’ of Eastern Roman/Byzantine society; if one also recalls the prominent role that the plague must have played in this (and for whose denial I still cannot find any plausible evidence) – then the question of the significance of this pandemic ultimately answers itself: It was to a large extent responsible for a cultural restructuring process that helped shape the transition from Late Antiquity to the early Middle Ages. I would like to emphasize that these are structural reconfigurations that can be made manifest by means of comparisons with previous decades, that are sometimes manifest in the sources, and that are even partially quantifiable (for example, the increase in processions, miraculous images, Marian feasts, etc.). I am convinced that the impact of the ‘Justinianic Plague’ must be pointed out with the greatest possible emphasis in this context, and less so because of the mortality it caused and the demographic and economic problems it created.

\textsuperscript{121} Joh. Eph. in the Chronicle of Zuqnīn p. 97–98 Ed. Witakowski.
\textsuperscript{122} Prok. BP 2,22,7–8.
\textsuperscript{123} Euagr. HE 4,29.
\textsuperscript{124} Agath. 5,10,5–7; Euagr. HE 4,29.
\textsuperscript{125} See Agath. 2,15,13; 5,4,3; 5,4,6, and, in particular, 5,10,7.
The short-, medium- and long-term effects of major pandemics cannot be determined only quantitatively, as the current Covid-19 pandemic clearly shows. Of course, demographic aspects, the number of people infected, the number of people who have died and those who have recovered are of great importance (although current events once again demonstrate the serious problems of counting numbers and the uncertainty of drawing conclusions from them). But far more important is the question of how societies change under the pressure of an epidemic disease. This too is impressively demonstrated in the present day – and this by a pandemic whose mortality rate, even if one takes a “minimalist position” for the ‘Justinianic Plague’, is still far lower than the number of victims in late antiquity. Anyone wishing to make a contribution to the complex discussion about the end of the Roman Empire and the transformation process that led from Late Antiquity to the Early Middle Ages cannot ignore the epidemic events of that time without comment. Purely quantitative surveys – regardless of whether they serve to exaggerate or relativize the pandemic – do not lead any further, because complex questions of cultural studies cannot be answered in this way; privileging superficially objective ‘scientific’ evidence leads us into biologisms and determinisms from which historical research has long distanced itself with good reason. This does not mean, of course, that the results of recent scientific approaches and the materials they offer are without value – quite the contrary. But they do not generate results of their own and do not themselves answer historical questions – let alone clearly and objectively. They rather require a careful, methodologically sensitive and critical interpretation. In order to achieve this, interdisciplinary efforts and, in particular, fundamental joint methodological discussions are necessary.\textsuperscript{127} Not only the present, but also the distant past is far more complex than a numerical value or the result of a DNA analysis might suggest.\textsuperscript{128}

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\textsuperscript{127} The reflections of Sessa (2019) on the current debates about the fall of the Roman Empire within the context of an environmental history offer a good starting-point for that.

\textsuperscript{128} See also Roosen/Curtis (2018), who deal with the Problem of an uncritical use of quantitative data or databases.


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The 'Justinianic Plague': An “Inconsequential Pandemic”? A Reply


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