THE ANTONINE PLAGUE AND THE 'THIRD-CENTURY CRISIS'

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Introduction: the Antonine plague

This paper will discuss two broad topics, the plague under Marcus Aurelius and the development of the Roman empire from the late second century onwards, and the relations between these two phenomena. The English word 'plague' is here used in the general sense of 'potentially lethal epidemic disease'. I do not want to imply that we are dealing with the 'bubonic plague' caused by the *yersinia pestis* bacillus (discovered or identified in 1894),¹ as today no one knows for certain what disease spread through the Roman world from 165 C.E. onwards, regardless of much speculation on the matter.²

The role of the plague among the causes of the 'third-century crisis'

The 'third century crisis' is in itself a debated topic, as is made abundantly clear in other contributions in this volume. To save time and space, I will simply take it for granted that changes affected the Roman world from the reign of Marcus Aurelius onwards which in certain

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¹ S.K. Cohn Jr., *The Black Death Transformed* (London 2002), 1. Cohn incidentally convincingly refutes the common notion that the European Black Death was bubonic plague, as do S. Scott and C.J. Duncan, *Biology of Plagues: Evidence from Historical Populations* (Cambridge 2001). The Black Death was likely a viral infection.

² Cf. W. Scheidel, 'A model of demographic and economic change in Roman Egypt after the Antonine plague', *Journal of Roman Archaeology* 15 (2002), 97–114, especially 99 "If the Antonine Plague was indeed a highly virulent form of smallpox".

ways were detrimental to the stability of the Roman Empire. Several rulers of the Severan dynasty can be blamed for various actions, but arguably the roots of the problem went deeper, i.e., to the economic, social and political foundations of the Roman world. Some scholars have thought that the Antonine plague affected these foundations so deeply that Rome started to decline after the reign of Marcus Aurelius. The discussion in this paper will focus on the decades preceding and immediately following Marcus' reign, down to the end of the Severan dynasty; thus the military anarchy of the mid-third century will not concern us here.

The interest in the effects of the Antonine plague is not new in Roman history. While it played no role in Gibbon's work, already Niebuhr considered it to have had serious effects on the Roman empire, especially in the cultural sphere.³ Another notable scholar with a similar view was Seeck.⁴ The title of Boak's work on manpower shortage signals a similar approach,⁵ while Mazzarino considered the plague and the wars under Marcus as the origin of the economic crisis of Rome.⁶

Rostovzeff, on the other hand, considered the plague on a par with foreign wars, poverty, and rebellion, and vehemently denied that depopulation would have constituted a factor in the weakening of the empire. He saw the roots of the crisis in a social upheaval in which the soldiery destroyed the bourgeois elite of the Roman world.⁷ The recently discovered notes from Mommsen's lectures on Roman imperial history from 1883 show him to have been similarly brief on the plague and its effects.⁸ He, like Rostovzeff later on, for the most part blamed political events for the budding crises under the Severans: "Lastly, there were the evil effects of incessant military insurrections.

³ B.G. Niebuhr, *Lectures on the History of Rome* III (London 1849), 251.

⁴ O. Seeck, *Geschichte des Untergangs der antiken Welt* I (Berlin 1910³), 398–405.

⁵ A.E.R. Boak, *Manpower Shortage and the Fall of the Roman Empire* (Ann Arbor 1955), 19.

⁶ S. Mazzarino, *La fine del mondo antico* (1959, reprint Milano 1988), 156–157.

⁷ M.I. Rostovzeff, *The Social and Economic History of the Roman Empire* (Oxford 1957²), 371, 374–375, and at 495: "Now, no political aim was at stake: the issue between the army and the educated classes was the leadership of the state (...) Such was the real meaning of the civil war of the third century. The army fought the privileged classes, and did not cease fighting until these classes had lost all their social prestige and lay powerless and prostrate under the feet of the half-barbarian soldiery".

⁸ Th. Mommsen, *A History of Rome under the Emperors* (Th. Wiedemann (ed.), translation C. Krojzl, London and New York 1996), 342.

How is a state to thrive when it changes its rulers by force every five years on average?"⁹

In the past decades the 'third-century crisis' has been the object of several monographs. The view of contemporaries such as Herodian and St. Cyprian have been analyzed by Alföldy.¹⁰ Other contemporary sources, such as some of the *Oracula Sibyllina*, were once discussed by Mazzarino,¹¹ and have received major attention from Strobel, who argues that the Eighth Oracle was written around 175 C.E. in Asia Minor.¹² It is interesting to see that, among the many signs of impending doom, the author of the oracle singles out famine and war, but pays very little attention to disease, which really ought to have devastated many communities in Asia Minor for a decade already, if the worst scenarios of the Antonine plague are to be believed. Indeed no major consequences are attributed to the plague by Strobel, who is altogether reluctant to talk about a 'third-century crisis'.¹³ Similarly Christian Witschel argues that the empire was so diversified that it is wrong to talk about a 'third-century crisis', while there were "numerous smaller crises which occurred regularly in pre-industrial times, such as failed harvests, famines, plagues, earthquakes, and the revolts which could result".¹⁴

In two recent authoritative collective enterprises the picture is more varied. First, in his contribution to the *Storia di Roma*, Elio Lo Cascio attributes great importance to the plague (both the Antonine one and a number of subsequent epidemics): the death-rate rose to 20% over a twenty-year period, and it would have taken the empire over seventy-five years to recover this loss of manpower, even if no other crises had intervened (which they did). From here stem the problems in recruiting

⁹ Mommsen 1996, op. cit. (n. 8), 345.

¹⁰ G. Alföldy, *Die Krise des Römischen Reiches. Geschichte, Geschichtsschreibung und Geschichtsbetrachtung. Ausgewählte Beiträge* (Stuttgart 1989). The author refers to plagues on several instances, but does not discuss the nature of the crisis much.

¹¹ Mazzarino 1959, op. cit. (n. 6), 38–39.

¹² K. Strobel, *Das Imperium Romanum im 3. Jahrhundert?: Modell einer historischen Krise?* (Stuttgart 1993), 57.

¹³ Strobel 1993, op. cit. (n. 12), 340–348: the worst period for Rome came in the 260s, there were some other difficult moments after ca. 250, but on the whole one should avoid labeling this transitional period ('Übergangsphase') a time of crisis.

¹⁴ C. Witschel, 'Re-evaluating the Roman West in the 3rd c. A.D.', *Journal of Roman Archaeology* 17 (2004), 251–281, especially 254 ("smaller crises"), 273 (conclusion); based on idem, *Krise – Rezession – Stagnation? Der Westen des römischen Reiches im 3. Jahrhundert n. Chr.* (Frankfurt a.M. 1999).

soldiers, the settlement of barbarians inside the empire, and a wide-ranging social and economic crisis.¹⁵

Second, in volumes 11 and 12 of the revised *Cambridge Ancient History* the Antonine plague receives some attention. Bruce Frier writes “The Roman empire was not dealt a mortal blow, but the sudden population drop ushered in, or immensely complicated, a host of social and economic problems”,¹⁶ while Mireille Corbier is cautiously agnostic.¹⁷

The Antonine plague in Egypt

A vicious epidemic spread from the East to Rome, Italy and western parts of the Roman world in the wake of Lucius Verus’ Parthian campaign (161–166 C.E.). There is no doubt about this, but there is a current debate about how serious the plague in reality was. Several rounds of this debate have been published in recent issues of the *Journal of Roman Archaeology*. Scholars have been debating the extent to which this epidemic disease affected the population, the society and the economy of the Roman world.

Walter Scheidel, in his most recent contribution on the topic in the *Journal of Roman Archaeology*, argued that the Antonine plague had a major, not to say a catastrophic effect on Egyptian society. Scheidel restricted his analysis to Egypt, although he also referred to some data from Rome and Italy that, he argued, provided substantiation for the dramatic changes he thought he could identify in Egypt after 165 C.E..¹⁸ We shall turn to the evidence from Rome and Italy below, after first briefly considering the situation in Egypt.

Papyrological experts have entered the debate, in particular Roger Bagnall, who is the author of several acute contributions.¹⁹ In 2002 he presented an evaluation of the same data that Scheidel had used

¹⁵ E. Lo Cascio, ‘Fra equilibrio e crisi’ in A. Schiavone (ed.), *Storia di Roma* II.2 (Torino 1991), 701–731, especially 710–716.

¹⁶ B.W. Frier, ‘Demography’, *CAH*² 11, 787–816, especially 816. Compare E. Lo Cascio, ‘General Development’, *CAH*² 12, 131–136, especially 136, referring to “the inability of families [of the ruling class] to reproduce, especially from the years of Marcus, when epidemic outbreaks introduced periods of high ‘crisis’ mortality.”

¹⁷ M. Corbier, ‘Coinage, Society and Economy’, *CAH*² 12, 393–439, especially 398 on the Antonine plague, and the ‘Plague of St. Cyprian’: “All of these clues should, of course, be followed up, but it is hard to reconstruct the full picture”.

¹⁸ Scheidel 2002, op. cit. (n. 2), 98.

¹⁹ First in R.S. Bagnall, ‘*P. Oxy* 4527 and the Antonine plague in Egypt: death or flight?’, *Journal of Roman Archaeology* 13 (2000), 288–292. In support of Scheidel: P. van

to substantiate the claim that the development of prices and wages in Egypt followed the model relating to Europe in the period after the Black Death (*ergo*, in Scheidel's view, the Antonine plague must have been equal in intensity to the Black Death).²⁰ Bagnall had at his disposal just over fifty sources with information on land prices for the three first centuries C.E. (mostly dating to 80–200 C.E.), an amount of data that probably will not impress many modern historians. Yet here, as so often in ancient history, the well-known dictum of Sir Ronald Syme comes in handy: "One uses what one has, and there is work to be done".²¹ Bagnall's overall verdict was the modest claim that he had "lesser ambitions than either corroborating or undermining the model [of Scheidel, Chr.B.] as a whole", while offering "more in the direction of undermining it".²² In general, Bagnall's contribution was much concerned with how to construe tables properly and how to present and interpret the statistical evidence, as well as with Scheidel's use of figures found in earlier research, which he simply reproduced "with no critical examination".²³ None of these features are unimportant, it seems to me.

A set of data that so far has not been used in the debate about the Antonine plague in Egypt concerns the reports of failed flooding of farmland by the Nile ('abrochia'). The material has been conveniently collected by Wolfgang Habermann and the almost 70 reports have the following chronological distribution:²⁴

Minnen, 'P.Oxy LXVI 4527 and the Antonine plague in the Fayyum', *Zeitschrift für Papyrologie und Epigraphik* 135 (2001), 175–177.

²⁰ R.S. Bagnall, 'The effects of plague: model and evidence', *Journal of Roman Archaeology* 15 (2002), 114–120; cf. n. 17 above. For the use of the Black Death and its aftermath as a model, see Scheidel 2002, op. cit. (n. 2), 100–101, 109.

²¹ R. Syme, *Roman Papers* II (Oxford 1979), 711. There is more material that can be put to use, though. One should note the remarkable fact that the all-encompassing statistical survey of the remaining papyrological material (some 35,000 texts) presented by W. Habermann, 'Zur chronologischen Verteilung der papyrologischen Zeugnisse', *Zeitschrift für Papyrologie und Epigraphik* 122 (1998), 144–160, has not played any role in the argument of those who propound dramatic consequences of the Antonine plague. Habermann presented the surviving sources from the 2nd century (p. 151–152): evidence from the Arsinoite nomes peaked in the 150s C.E.; in the 160s it returned to the level of the 140s. The material from all the other nomes peaks in the 110s, and is then roughly evenly spread until 200 C.E. Obviously a more detailed analysis of the material might be worth while.

²² Bagnall 2002, op. cit. (n. 20), 114.

²³ Bagnall 2002, op. cit. (n. 20), 119.

²⁴ W. Habermann, 'Aspekte des Bewässerungswesens im kaiserzeitlichen Ägypten I: Die "Erklärungen für nicht überflutetes Land" (Abrochia-Deklarationen)', in K. Ruffing and B. Tenger (eds.), *Miscellanea oeconomica. Studien zur antiken Wirtschaftsgeschichte Harald Winkel*

Table 1: ‘Abrochia’-reports from Egypt
(from Habermann 1997, op. cit. (n. 24))

Year	Number of Reports	Year	Number of Reports	Year	Number of Reports
158	1	190	4	208	2
163	4	195	3	209	2
164	11	201	1	212	1
168	5	202	4	219	2
169	2	203	2	224	1
170	2	204	5	226	1
171	1	206	1	240	2
				245	4

It is easy to discern a pattern here. The reign of Marcus Aurelius was exceptionally heavily affected according to the reports on uninundated farmland, while further concentrations appear in the early 190s and in the first five years of the 3rd century. It would surely be tempting to connect these reports to the Antonine plague and its sequels – on the grounds that the rampant plague would have prevented work on the dikes and other operations necessary for an even flooding – were it not for the fact that the first peak in our data comes already in 163–164 C.E. Since the plague as far as we know did not reach Egypt before 165 C.E., this removes the ‘abrochia’-reports from the discussion, except for the fact that the difficulties in irrigating their farmland that Egyptian peasants experienced in the period 163–171 C.E. must surely be taken into account when debating the reason for changes in the Egyptian economy and population during those years.

The Black Death as a model for crisis

It is notable that Scheidel does in fact not connect the woes of Egypt (or Italy) to the ‘third-century crisis’, even though he postulates a ‘lasting consequence’ of the plague, in combination with later plagues under Commodus and in the third century.²⁵ He does, however, use the European Black Death from 1348 onwards as a model for the investigation of the Antonine plague in Egypt, and this comparative

zum 65. Geburtstag (Pharos IX, St. Katharinen 1997), 213–283, especially 223–226. In the presentation to follow I exclude a handful of reports that cannot be securely dated to a particular year.

²⁵ Scheidel 2002, op. cit. (n. 2), 108.

perspective is a major aspect in all recent scholarship on the Antonine plague. As is well known, the effects of the Black Death were beneficial for those individuals who survived and for the following generations, insofar as real wages tended to rise. There was almost everywhere a lack of labourers, and thus wages rose quicker than prices (there was less demand – even if at first prices were high, when production broke down completely), while land rents decreased, as there were fewer peasants to work the land.²⁶

Against this background of the Black Death model, one might even say that it is no surprise if no connection is made between the Antonine plague and the troubles of the third century, as the plague could be said simply to have carried out a necessary 'Malthusian' purge. The empire should have been expected to recover and rise, stronger than ever, as in the 1300s, when the plague struck not only once, but repeatedly and during a long period. Yet the Black Death and its sequels did not prevent the Italian Renaissance from taking hold, nor did it prevent the new ideas and modes of behaviour from spreading, or the Italian city-states such as Florence and Milan from growing to become some of the leading financial powers of the world (or even political and military ones).²⁷

Now, while Scheidel's statistics from Egypt seem to adhere to the expected outcome in many instances, he acknowledges that the model does not quite apply: *per capita* real income does not seem to have risen. This, it seems to me, again provides food for thought.²⁸

Doubts about the seriousness of the Antonine plague

At this point we shall return to the plague in Italy and Rome, the heartland of the empire. Scheidel's 2002 article elicited two critical

²⁶ Brief resumes in, for example, Lo Cascio 1991, op. cit. (n. 15), 711–713; Scheidel 2002, op. cit. (n. 2), 100. There are regional differences and the model has also been challenged, but the general trend seems clear enough, see J. Hatcher, 'England in the Aftermath of the Black Death', *Past & Present* 144 (1994), 3–35, especially 32–35.

²⁷ When students of the Black Death sometimes state that it took more than a century for Europe to return to the pre-plague conditions, they refer to population levels, not to standard of living or general economic strength.

²⁸ Scheidel 2002, op. cit. (n. 2), 109. Lo Cascio 1991, op. cit. (n. 15), 715–716, also provides some answers to why the scenario played out differently: in his view, the dominating role exercised by the upper classes, supported by the imperial government, prevented the masses from benefiting.

responses, one from James Greenberg of the University of Chicago,²⁹ and one from myself.³⁰ Greenberg used more statistical calculations and more sophisticated tables than Scheidel and Richard Duncan-Jones, the scholar whose work had inspired Scheidel's study, and argued that Scheidel's figures cannot be said to prove what they seem to show: namely, that the Antonine plague had such dire consequences during the succeeding decades. In addition, Greenberg and I both independently reached the conclusion that one cannot prove the effects of the plague by using such one-dimensional tools as Duncan-Jones and Scheidel had marshalled. The same holds true for some inscriptions that have received attention in the most recent past.³¹

However, Greenberg never asked one fundamental question: namely *how* Scheidel arrived at his figures in the first place. Accepting all the data presented by Scheidel, he fell victim to the 'power of numbers and statistics'. My own approach was in part different: ever the positivist, I looked at the primary data Scheidel used, which to be sure he had taken over from the work of other scholars (obviously fully acknowledging this). I believe I was able to show that the figures were often inaccurate, that the real numbers which can be derived from the sources present a rather different picture, and that as long as we use the method of Duncan-Jones and Scheidel in evaluating epigraphic evidence, we will be unable to prove that the plague had any dramatic negative effect in Rome and Italy.³²

I should reiterate my firm belief that there was an outbreak of the plague in Italy after 165. Yet I do not think that we can take our late literary sources at face value when they claim that it was the worst ever or that the mortality was enormous.³³ More sophisticated and holistic methods must be devised for using the epigraphic evidence, which is

²⁹ J. Greenberg, 'Plagued by doubt: reconsidering the impact of a mortality crisis in the 2nd c. A.D.', *Journal of Roman Archaeology* 16 (2003), 413–425.

³⁰ C. Bruun, 'The Antonine plague in Rome and Ostia', *Journal of Roman Archaeology* 16 (2003), 426–434.

³¹ C.P. Jones, 'Ten dedications "To the gods and goddesses" and the Antonine Plague', *Journal of Roman Archaeology* 18 (2005), 293–301; *idem*, 'Addendum to *JRA* 18: Cosa and the Antonine plague?', *Journal of Roman Archaeology* 19 (2006), 368–369.

³² Bruun 2003, op. cit. (n. 30), 427–434 (misinterpreted data); 434 (need for a holistic approach).

³³ *Historia Augusta*, *Vita Marci* 13.2, 21.6, Orosius, and Eutropius. See Greenberg 2003, op. cit. (n. 29), 423; he is right that Gilliam 1961, op. cit. (n. 34) already provided an exhaustive evaluation of these late sources, concluding that they cannot be trusted to be accurate.

certainly important, before we can be certain about the effect of the plague.

To my mind, Gilliam's cautious investigation into the value of the literary evidence on the plague is still the most valuable we have. He concluded that 1% to 2% of the population of the empire may have died in the 160s.³⁴ Duncan-Jones went over the same evidence again in 1996 in perhaps his most substantial contribution to this debate, reading much more into the same texts,³⁵ but his argument does not quite convince.

On the other hand, with so few data, there may be a temptation to forego the primary sources altogether and simply work with comparative models, be they demographic or economic. Models are obviously good to think with, but still I believe that ancient history stands or falls with its primary sources.

In what follows, I shall examine closely some of the evidence presented by Duncan-Jones and Scheidel for the serious effects of the plague in Italy, evidence that has not yet received the proper critical scrutiny.³⁶ My purpose here is partly methodological: to illustrate how allegedly authoritative numbers used in the debate about the Antonine plague really originated.

A case study: building inscriptions in Italy during the second century

Among the material presented by Duncan-Jones in 1996 (and then used by Scheidel in 2002) as proof of the ravaging of the Antonine plague were "Fig. 10 Italy: public buildings, A.D. 98–211 (non-imperial)", and "Fig. 11 Italy: imperially financed buildings, A.D. 98–211".³⁷

The bar-graph in Fig. 10 shows a steady decline in inscriptions per year in the period following Antoninus Pius, i.e., through the reigns of Marcus, Commodus, and down to Severus, whose reign is the poorest

³⁴ J.F. Gilliam, 'The plague under Marcus Aurelius', *American Journal of Philology* 73 (1961) 225–252 = idem, *Roman army papers* (Amsterdam 1986), 227–253.

³⁵ R.P. Duncan-Jones, 'The impact of the Antonine plague', *Journal of Roman Archaeology* 9 (1996), 108–136.

³⁶ Greenberg 2003, op. cit. (n. 29), 417–418, examines the statistical presentation critically without addressing the question of how the data was collected. The topic found no space in my own Bruun 2003, op. cit. (n. 30).

³⁷ Duncan-Jones 1996, op. cit. (n. 35), 127. Statues were excluded, which have little value when discussing 'building inscriptions'.

in terms of surviving evidence. Fig. 11 shows a complete blank for the period 161–192, i.e., no imperially financed buildings were constructed in Italy under Marcus and Commodus. The absolute numbers can roughly be gauged from the bars in Duncan-Jones' graph but they are nowhere mentioned in the 1996 paper. One has to turn to Duncan-Jones' *Structure and Scale* (1990) for confirmation, and there one will find the following results:³⁸

Table 2: Imperial and non-imperial building in Italy according to Duncan-Jones 1990, op. cit. (n. 38), 213

	Italy, building dedications only (non imperial)	Italy, building dedications only (emperors)
Trajan	4	4
Hadrian	11	10
Pius	15	7
Marcus	5	—
Commodus	2	—
Severus	1	3

Unfortunately not even Duncan-Jones gave references to the individual sources on which his bar-graph was based. There is a general reference to Hélène Jouffroy's work from 1986, which in some thirty pages records the evidence for public building in Italy during the second century up to the end of the reign of Commodus.³⁹

A perusal of the substantial lists in Jouffroy's book raises a number of methodological questions. According to my calculation, she included some 130 buildings built or repaired in Italy from Trajan to Commodus,⁴⁰ while Duncan-Jones' table above contains only half of that, a mere 38 non-imperial and 24 imperially financed public buildings, which gives a total of 62 items for a period of over 110 years. Anyone wanting to reduplicate Duncan-Jones' survey of Jouffroy's data (which ideally should be possible) faces serious methodological problems, having

³⁸ See R.P. Duncan-Jones, *Structure and Scale in the Roman Imperial Economy* (Cambridge 1990), 213 Appendix 2 for the figures.

³⁹ Duncan-Jones 1990, op. cit. (n. 38), 62; H. Jouffroy, *La construction publique en Italie et dans l'Afrique romaine* (Strasbourg 1986), 109–140.

⁴⁰ I did not count buildings that were merely registered as having been in existence ("attesté").

for instance to decide whether or not to include entries of the following types presented by Jouffroy:

- (1) "*Operae eius haec exstant... Caietae portus, Tarracinensis portus restitutio...* (Hist. Aug. Pius 8.2)" – where we only have a literary reference for building operations,
- (2) "*Volcei: [...ex tes]tamento Otacili Galli patris Caesare[um vetustate] conlapsum p(ecunia) s(ua)...* (CIL X 415), II^e siècle" – which gives only a very general date,
- (3) "*Corfinium: C. Alfius T.f. Maximus pecuniam legavit L. Herennio C.f. Rufo is aedem podium cryptae partem facienda curavit probavitq.*; CIL IX 3168, après 122" – where we only get a *terminus post quem*,
- (4) "*Mevania: vestiges d'un temple tétrastyle; Hadrien (C. Pietrangeli, Mevania...)*" – where the information is derived solely from archaeological material.⁴¹

Duncan-Jones declared that the bars in his graph recorded 'building dedications', which patently means that only epigraphical evidence could be included.⁴² Archaeological material such as (4) is consequently excluded, and so too presumably are literary sources such as (1), and evidence lacking a precise date. This essential information was lost in the transition and is no longer stated in the 1996 paper which only refers to 'public buildings' and 'imperialy financed buildings', nor hence in Scheidel's 2002 article.

That leaves the question, how to deal with buildings that are not precisely dated to a particular reign by Jouffroy. While my survey turned up about ten non-imperial or imperial buildings dated under Marcus (against five listed by Duncan-Jones), there are another sixteen that are dated 'mid-second century', 'last third of the 2nd c.', 'third quarter of the 2nd c.', 'before 200', and so on. This is not the right place for an in-depth and properly footnoted survey of Jouffroy's data – which is in any case in part outdated (see next paragraph) and in part less than completely accurate⁴³ – but it is important to realize the limitations of the information that Duncan-Jones extracted from Jouffroy's lists.

⁴¹ Jouffroy 1986, op. cit. (n. 39), 112 (1); 118 (2), (3) and (4).

⁴² Duncan-Jones 1990, op. cit. (n. 38), 62.

⁴³ For instance, inscriptions on *fistulae* have not been dealt with in a coherent way. Why is CIL 11.3548a–b (Centumcellae) included (p. 113), but not, for example, *Imp. Hadrianus Pyrgensis* (*Notizie degli scavi di antichità* (1960), 363) for Pyrgi. For a survey of all imperial *fistulae* in Italy see C. Bruun, 'Imperial Water Pipes in Roman Cities', in A.O. Koloski-Ostrow (ed.) *Water Use and Hydraulics in the Roman City* (Dubuque, Iowa

We are dealing with inscriptions only, and at that with 'dedications', a situation that seems to limit the material still further.⁴⁴

In any case, as far as epigraphical evidence for imperial building activity in Italy is concerned, there is now the more recent work of Marietta Horster. Her clear and well documented study focuses on urban imperial building activities in Italian towns.⁴⁵ It is interesting to compare Horster's figures (supplemented with additions by Géza Alföldy) with those presented above. If we exclude Ostia (as did Duncan-Jones), and evidence from *fistulae*, Horster's work reveals some ninety imperial building projects in Italy dated to a particular reign (with some fifteen more of uncertain date). The figures look as follows:⁴⁶

2001), 51–63. *CIL* 11.3793 = 6.1260 = *ILS* 290 refers to the Aqua Traiana which supplied the capital, not to a local aqueduct for Veji (p. 113).

⁴⁴ Nowhere in Duncan-Jones 1990, op. cit. (n. 38) is there a clarification of what is meant by a 'dedication'. It seems that a dedicatory formula *dedicatus/a/um* plus date and name was not required for a text to be counted by the author. Nor are readers made aware of what method was applied when a building was financed by an earlier emperor but dedicated by his successor.

⁴⁵ M. Horster, *Bauinschriften römischer Kaiser: Untersuchungen zu Inschriftenpraxis und Bautätigkeit in Städten des westlichen Imperium Romanum in der Zeit des Prinzipats* (Stuttgart 2001). As seen from the title, the study focuses on towns. Milestones are therefore excluded, and inscriptions relating to roadworks, including bridges, have apparently not been systematically studied, see 12, 296, 315 n. 299 (*CIL* 11.6622). Precisions and additions were supplied by G. Alföldy, *Journal of Roman Archaeology* 15 (2002), 489–498 (reviewing Horster 2001), and idem, 'Zu kaiserzeitliche Bauinschriften aus Italien', *Epigraphica* 64 (2002) 113–145.

⁴⁶ The following list retains the order in which the inscriptions are mentioned in Horster 2001, op. cit. (n. 45), 253–341, separately for each emperor (Ostia is excluded in order to create a better comparison with Duncan-Jones). Some inscriptions were added based on suggestions made by Alföldy (see the previous note), and some inscriptions which Horster did not include in her list of urban building inscriptions proper (pp. 76–96) have been included, because they do refer to some kind of public work (such as bridges). Augustus: *CIL* 10.1617, 10.4749, 9.540*, 11.6218, 11.5266; *AE* 1991, no. 666; *CIL* 11.720, 5.5027, 5.3325; Tiberius: *CIL* 11.3783, 11.3784, 5.4307, 5.6358; Caligula: *CIL* 11.720; Claudius: *CIL* 11.5; *AE* 1991, no. 666?; Galba: *CIL* 11.6187; Vespasian: *CIL* 14.3485, 10.1406; *AE* 1979, no. 170; *CIL* 10.1629, 11.5166, 11.3734, 11.598, 5.4212; Titus: *CIL* 10.1481 = *IG* 14.729; *AE* 1994, no. 413; *CIL* 10.1630; *AE* 1951, no. 200, *AE* 1902, no. 40; Domitian: *AE* 1994, no. 404; *EE* 9.609; *CIL* 11.368; Trajan: *CIL* 9.5746; *AE* 1987, no. 353; *CIL* 5.854, 9.4515; Hadrian: *CIL* 10.6652, 14.2216, 15.2460, 10.5649, 14.2797; *AE* 1982, no. 142a; *CIL* 14.2798 ?; *NSA* 1907, 127+658 f.; *AE* 1976, no. 114; *CIL* 10.5963, 10.4574, 10.3832, 10.463*, 9.4116, 9.5681, 9.5294, 9.5353, 11.5668, 11.6115, 11.6001, 11.5988; *AE* 1984, no. 390; *AE* 1946, no. 222, *AE* 1991, no. 694, *CIL* 5.2152; Antoninus Pius: *CIL* 10.3832, 10.3831, 10.1640–41, 10.103; *EE* 8.204; *CIL* 9.5353; *AE* 1984, no. 390; *CIL* 11.1425, 11.3363; Marcus: *CIL* 11.371; Commodus: *CIL* 10.6654?, 11.1665; Septimius Severus: *CIL* 10.5909; *AE* 1982, no. 153; *IGItal.* 1.22; Caracalla: *CIL* 11.2166?, 9.4960; *AE* 1968, no. 157. S. Segenni, 'Antonino Pio e le città dell'Italia (Riflessioni su *H.A.*, v. *Pii*, 8,4)',

Fig. 1: Imperial building projects in Italy: Augustus (9) – Tiberius (4) – Gaius (1) – Claudius (2) – Nero – Galba (1) – Vespasian (8) – Titus (5) – Domitian (3) – Nerva – Trajan (4) – Hadrian (24 + 1?) – Pius (9) – Marcus (1) – Commodus (1 + 1?) – Septimius Severus (3) – Caracalla (2 + 1?) – Individual emperors from Severus Alexander to Aurelianus (9 + 1?).

As is evident, Horster's research presents some differences compared to Duncan-Jones' table, but again the quantity of the material is not very large. The one outstanding feature is the enormous activity under Hadrian (some 30% of all the dated projects belong to his reign), but otherwise the material lends itself to a number of different conclusions, depending on the pattern one wants to see and the periods one construes. For example, one might wonder at the exiguous number of projects in the later Julio-Claudian period (only two in over thirty years after A.D. 37), at the record activity under Vespasian and Titus (at least thirteen projects in twelve years), at the passivity during the following almost four decades (only seven projects from 81 to 117), and so on. But of course these observations are arbitrary and different periodizations would produce different impressions; my point is to underline the fragility of this kind of proof by statistics. And here I will not even go into the question of the 'epigraphic habit', imperial self-glorification, *damnatio memoriae* and other essential factors that influence the composition of the epigraphic record. However, the dearth of projects under Marcus is still quite noteworthy, and the difference compared to his

Athenaeum 89 (2001), 355–405 contains a fuller survey of Pius's activities. That emperor in several cases (merely) dedicated what Hadrian had begun.

predecessor cannot be denied, although Pius in many cases had the advantage of finishing projects that Hadrian had begun.⁴⁷

Some observations by Horster are important in this context. The concept of 'Sättigung an Gebäuden', i.e. the possibility that local needs had already been satisfied, should not be forgotten when explaining fluctuations in public building.⁴⁸ Public building is certainly not always driven by rational causes, but after the surge under Hadrian the needs may have been less pressing (even though, ideally, repair works ought surely to have been undertaken in the 160s–170s on buildings erected under Hadrian or before). More importantly, Horster reaches the conclusion that no conscious imperial building policy can be discerned in Italy. The emperors mostly reacted to special needs of one kind or another.⁴⁹ Therefore, if Marcus' attention was taken up by his wars, as it surely was, it is only to be expected that there should be less public building in Italy sponsored by imperial funds.

To sum up so far: The information we have about the Antonine plague does not warrant the conclusion that it was of such a magnitude that by itself it would have had catastrophic consequences for the Roman world. There were other factors, though, that created problems for the empire: foreign enemies and long-term social and economic developments, for instance. The Antonine plague on its own cannot explain the 'third-century crisis', of whatever nature it was.

The plague and the debate about slavery in Italy

There is a further question for which the Antonine plague is also allegedly quite important. The research of Elio Lo Cascio has tied the plague to a specific aspect of the 'third century crisis' – the fate of slavery in Italy.

Scholars interested in determining the population of ancient Italy are engaged in two related debates which concern the overall population of

⁴⁷ See *CIL* 9.5353, 10.1640, 3832; *AE* 1984, no. 390.

⁴⁸ Horster 2001, op. cit. (n. 45), 243. This possible explanation for a decrease in inscriptions was mentioned, although not advocated, for North Africa by E. Fentress, 'African Building: Money, Politics and Crisis in Auzia', in A. King and M. Henig (eds.), *The Roman West in the Third Century. Contributions from Archaeology and History* (Oxford 1981), 199–210, especially 199 f., where other possible explanations are mentioned as well.

⁴⁹ Horster 2001, op. cit. (n. 45), 248–250; supported by Alföldy 2002, op. cit. (n. 45), 491–492.

Italy and the slave population of Italy (and the whole empire). Details concerning the debate are presented elsewhere in the volume; suffice it to say here that a crucial question is how to interpret the Augustan census figures, around 4 million in 28 B.C.E. and 4.9 million in 14 C.E.⁵⁰ This represents an astonishing growth since 70 B.C.E. (910,000). No demographic model can account for such an enormous growth by natural means. Is it therefore the case that the Augustan figures include women and children? Many scholars are of that opinion. Lo Cascio considers such a proposition impossible, with some good arguments, explaining the higher figures as the product of a more efficient census and new grants of citizenship.⁵¹

The high population estimate for imperial Italy proposed by Lo Cascio, some 12 million, has a certain relevance for another lively current debate, the one about the number of slaves in the Roman world. Prominent participants in this debate include Walter Scheidel and William Harris. Scheidel argues that slave breeding was the only way in which the slave population could have maintained itself demographically during the empire, when slaves may have constituted 10% of the population. Harris argues that for keeping the numbers of slaves stable other sources of supply were important and probably equally important as breeding: in particular infant-exposure, but also import across the borders, piracy, and so on. His estimate of the slave population is closer to 15%–20% of the total.⁵²

There are many uncertainties in these calculations and no model is completely satisfactory, as Lo Cascio showed in a paper published in 2002 in which he solved the problem of the apparently too high proportion of slaves by arguing that the total population of the Roman

⁵⁰ P.A. Brunt, *Italian Manpower 225 B.C.–A.D. 14* (Oxford 1971), 13–14.

⁵¹ E. Lo Cascio, 'Il census a Roma e la sua evoluzione dall'età "serviana" alla prima età imperiale', *Mélanges de l'École Française de Rome (Antiquités)* 113 (2001), 565–603, especially 591–592; more in detail E. Lo Cascio, 'The Size of the Roman population: Beloch and the Meaning of the Augustan Census Figures', *Journal of Roman Studies* 84 (1994), 23–40, especially 32: women and children were included in the provincial census, but they were taxpayers, unlike the situation in Italy. It is thought that in Italy all those who were *sui iuris* declared the women and children under their authority, but that does not mean that they were included in the count. The first provincial census (which some think influenced Roman practice) was not until 27 B.C.E. Lo Cascio's figures are supported by G. Kron, 'The Augustan Census Figures and the Population of Italy', *Athenaeum* 93 (2005), 441–495.

⁵² W. Scheidel, 'Quantifying the Sources of Slaves in the Roman Empire', *Journal of Roman Studies* 87 (1997), 159–169; W.V. Harris, 'Demography, Geography and the Sources of Roman Slaves', *Journal of Roman Studies* 89 (1999), 62–75.

Empire was in fact higher.⁵³ If scholars assume six million slaves under Marcus Aurelius, against 54 million free individuals,⁵⁴ then it may well be that the pool from which to recruit enough slaves to keep the servile population stable was not large enough. By assuming, however, that the free population was considerably larger, for instance comprising some twelve million in Italy alone, six million slaves in the empire at large will make up a much smaller group as a percentage of the total population, and the pool from which to recruit new slaves (foundlings, victims of kidnapping, etc.) is hence concomitantly larger.⁵⁵ As is evident, this is no *ad hoc* solution by Lo Cascio; it derives directly from his view on the size of the citizen body and the Italian population under Augustus and the succeeding dynasties.

Assuming a larger total population of the Roman world than some other scholars do is certainly one way of solving the problem with the stability of the slave population. It is a solution which also interestingly assigns less importance to slave labour during the first two centuries C.E. than is customary.

But one problem, it seems to me, is that the high population figures have to come down eventually. I doubt that one can argue for such a large overall population of the Roman world in the later 2nd century and during the difficult years of the 3rd century. In order for Lo Cascio's model to make sense, the numbers must decline, and this is where the Antonine plague is important. The plague provides a logical reason for why the large population of the first century B.C.E. is much reduced some two centuries later.⁵⁶

Here I come back to my conclusion in the previous section: what if, after all, one cannot show that the Antonine plague had such catastrophic consequences (including demographic ones) as is commonly assumed? If so, the 'high population model' may have to be revisited,

⁵³ E. Lo Cascio, 'Considerazioni sul numero e sulle fonti di approvvigionamento degli schiavi in età imperiale', in W. Sudor (ed.), *Études de démographie du monde gréco-romain* (Wrocław 2002), 51–65.

⁵⁴ Frier 2000, op. cit. (n. 16), 814 suggests a total population of 61.4 million in 164 C.E.

⁵⁵ Lo Cascio 2002, op. cit. (n. 53), 63; see *idem*, 'Il rapporto uomini-terra nel paesaggio dell'Italia romana', *Index* 32 (2003), 1–15, especially 9–10 for the figure of twelve million.

⁵⁶ See Lo Cascio 2002, op. cit. (n. 53), 6: "A risolvere drasticamente il problema interviene, negli anni '60, la pestilenza".

and, as a consequence, it becomes more difficult to argue for quite as large a population of slaves in Roman society as is sometimes done.

The Antonine plague indeed represents a crucial question at the intersection of debates about the 'third-century' crisis, the Roman population, and even the size of the slave population.

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