

**Epidemic Smallpox, Roman Demography, and the Rapid Growth of
Early Christianity, 160 CE to 310 CE**

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Introduction and Motivation for Research

Between the second and the sixth centuries CE, a series of epidemic diseases struck the Roman world, causing a significant demographic decline and contributing, along with many other factors, to the decline of the Roman empire. A number of commentators have linked these plagues to specific events and trends in Roman history, such as the crisis of the third century, or have considered the plagues within debates about Roman demography or economy. But less attention has been paid to the potential importance they may have had for another important process ongoing at the time of the plagues: the rapid growth of Christianity. Conveniently for such an investigation, Constantine's conversion occurred at almost the same time as the last mention of a plague in historical sources (310 CE, by Eusebius), after which the Roman empire apparently did not suffer another major epidemic until the outbreak of bubonic plague in 541 CE in Egypt and Constantinople, by which time the process of Christianization was largely complete. Several interesting hypotheses of a possible plague-Christianity interaction have been proposed in the past, primarily by William H. McNeil in *Plagues and Peoples* (1976), and sociologist of religion Rodney Stark, in *The Rise of Christianity* (1996). The hypotheses advanced by these scholars focused on the alleged existence of a differential rate of mortality between Christians and pagans during the plague, due to Christian community solidarity and ostensibly superior nursing practices. This paper briefly explores their hypotheses, but finds them inadequate and instead employs a different approach to the problem by focusing on the ability of the Roman population and the Christian subpopulation to mount effective fertility responses in the wake of the mortality crisis. Due to the widespread Roman practice of sex-selective infant exposure, this paper argues, the Roman population had a skewed sex ratio, favoring men over women, a factor which inhibited the

population's ability to recover through increased fertility in the wake of the plague.

Christians, on the other hand, like their Jewish counterparts, did not practice infant exposure, and so had a sex ratio which was either neutral—with roughly equal numbers of men and women—or which may have slightly favored women, if the literary sources describing Christian success in converting large numbers of women can be believed. Whether the sex ratio was neutral or favored women over men, the Christian population would not have suffered the same fertility-dampening effect suffered by the Roman population, and it was thus capable of a more robust fertility response. The Christian population was thus able to accelerate its growth relative to the overall Roman population in the wake of the Antonine plague of 165 CE and in the wake of the second wave of plague in 189. It is important to note that this growth was *relative* to the overall Roman population, because the plagues themselves did not cause growth of any kind, but merely reduced the population of all groups. On top of the underlying demographic growth of the Christian population, conversion would have added even more to Christian numbers, and it is reasonable to expect that these conversions would have favored younger converts over older ones, as is often seen in new religious movements today. As a result, the age structure of the Christian population would have been tilted toward the younger end, and the Christian population would have had more individuals of reproductive age than its total size would suggest, permitting a higher level of reproduction than was possible for the Roman population as a whole. Underlying demographic growth thus worked synergistically with conversion to produce the rapid growth of Christianity during the first three centuries, up to the time of Constantine, at which point I conclude my analysis. The key to this process is the underlying demographic expansion, and the key to the demographic expansion is the problem of imbalanced sex ratios produced by sex-selective infant exposure.

The Great Pestilence

In about 165 CE, an epidemic swept the Roman empire. The literary sources are relatively sparse, but from several histories of the fourth century we gather the following story: an army campaigning in Mesopotamia contracted the disease in Seleucia (while desecrating a temple of Apollo, naturally), and the disease was brought by the army back to Rome; from Rome it proceeded to infect the whole world.¹ The story these histories tell is impossible to verify, but fortunately we do possess a contemporary account, by Galen, which gives an impressionistic but essentially reliable picture:

After I had passed another three years at Rome, when the great pestilence began, I swiftly returned to [Pergamum] my native city.²

When I reached Aquileia [in 168 CE], the plague grew fiercer than ever, so much so that the Emperors immediately went back to Rome with a few soldiers, while we the majority had difficulty in surviving. Most of us died, not merely from the plague, but because the epidemic was happening in the depths of winter.³

It is from this account that the name ‘Great Pestilence’ is derived, though more commonly it is referred to as the Antonine Plague. In dealing with infected soldiers from the army camp at Aquileia, Galen was able to gain direct experience with the plague, and recorded observations of his patients in enough detail that modern scholars have been able to derive a symptomatology from it, which has proven somewhat more useful than has Thucydides’ famously befuddling description of the Athenian plague. Galen says that an exanthem covered

¹ Ammianus Marcellinus, *Roman History* 23.6.24; *Historia Augusta*, Verus 8 and Marcus Aurelius; Eutropius, *Breviarium historiae Romanae* 8.12; Orosius, *History Against the Pagans*, 7.15.5-6.

² Kuhn, 19.5, translation by Duncan-Jones (1996).

³ Kuhn 19.15-18, translation by Duncan-Jones (1996).

the body, which turned black in the late stages due to remnants of blood in the blisters, and eventually the blisters scabbed and fell away, leaving scars; he also says that patients suffered fever, bloody stools, and diarrhea.⁴ Galen's description of these symptoms enabled Littman and Littman in 1973 to diagnose the Antonine plague as smallpox with a high incidence of the hemorrhagic form. The presence of rash over the whole body rules out bubonic plague, because buboes would have been concentrated in the groin and armpits. Furthermore, the rash was pustular rather than flat, meaning that it could not be typhus (which would have a flat rash). Typhus also would have produced a fine powdery desquamation, versus the scabs which Galen describes. Furthermore, Galen's black stools coincide with the gastrointestinal bleeding of hemorrhagic smallpox, and diarrhea is also a common smallpox symptom. On the basis of their knowledge of the virulence of smallpox, Littman and Littman estimated a mortality rate of 7 to 10 percent across the entire empire. Cities and armies, due to their high population density and connectedness with other population centers, would have been hardest hit, with a mortality rate of 13 to 15 percent according to the Littmans' estimate.

The Littmans' diagnosis of smallpox has remained by far the majority opinion among scholars, though the incidence of hemorrhagic smallpox (which is far deadlier than the ordinary form, producing a 90 percent mortality rate in those infected, as opposed to 30 percent),⁵ has not attracted significant attention. The consensus among scholars is that this smallpox epidemic struck a virgin population, i.e. one which had no smallpox immunity from prior exposure. The expected mortality is thus quite high, in line with the catastrophic

⁴ Littman, R.J. and M.L. Littman, "Galen and the Antonine Plague," *The American Journal of Philology* 94, no. 3 (1973).

⁵ "Smallpox Disease Overview," Centers for Disease Control and Prevention, <http://www.bt.cdc.gov/agent/smallpox/overview/disease-facts.asp>.

mortality described in all the literary sources, and an economic decline should also be expected. In a groundbreaking 1996 paper, R.P. Duncan-Jones argued that just such an economic decline was indicated by measurable economic changes which can be seen in various kinds of non-literary evidence, especially from Roman Egypt.⁶ He found that in the decades after 165, there was a decreased area and duration of land leases in Egypt, a decrease in land rents, a decrease in documentation such as army diplomas, and a decrease in Egyptian coin production. He also found similar effects in Rome and Italy, including decreases in inscriptions, public building projects, buildings financed by the emperor, brick and marble production, and Roman coins.

Duncan-Jones was at first supported by Walter Scheidel (2002), who argued that the economic changes induced by the plague in Egypt were comparable to those seen after the Black Death, i.e. land costs went down, rents went down, and wages went up, as follow-on effects to decreased population pressure and a decreased ratio of labor to available land. Scheidel's findings thus further corroborated the view that the plague had a very large demographic impact.⁷ The arguments of Duncan-Jones and Scheidel have subsequently been undercut, however, by various criticisms leveled by Roger Bagnall and others,⁸ and in his

⁶ Duncan-Jones, R.P., "The impact of the Antonine plague," *Journal of Roman Archaeology* 9 (1996), 108-36.

⁷ Scheidel, Walter, "A model of demographic and economic change in Roman Egypt after the Antonine plague," *Journal of Roman Archaeology* 15 (2002), 97-114.

⁸ Roger Bagnall, "The Effects of the Plague: Model and Evidence," *Journal of Roman Archaeology* 15 (2002): 114–20; Christer Bruun, "The Antonine Plague in Rome and Ostia," *JRA* 16 (2003): 426–34; James Greenberg, "Plagued by Doubt: Reconsidering the Impact of a Mortality Crisis in the 2nd c. A.D.," *JRA* 16 (n.d.): 413–25; Christer Bruun, "The Antonine Plague and the 'Third-Century Crisis,'" in *Crises and the Roman Empire: Proceedings of the Seventh Workshop of the International Network Impact of Empire, Nijmegen, June 20-24, 2006*, ed. Olivier Hekster, Gerda de Kleijn, and Danielle Sloopjes (Leiden: Brill, 2007).

contribution to *L'impatto della "peste antonina"* (2012), Scheidel concedes that the currently available economic evidence is insufficient to determine the extent of the plague's impact.⁹ However, Scheidel defends his earlier paper's conclusion against Bagnall's criticisms, arguing that land rents did indeed fall in Egypt. He also continues to support his earlier conclusion that wages rose, but he moderates the degree to which they rose, calling the difference a "shortfall" relative to what might be expected based on a knowledge of the Black Death. This shortfall, Scheidel believes, can be explained by institutional arrangements which prevented Egyptian workers from taking advantage of the labor shortage. Scheidel compares the difference between the effect of the Black Death and of the Antonine Plague to the difference between the effect of the Black Death in England, where the resultant labor shortage "finished off the manorial system and raised real incomes for workers," and the effects of the Black Death in Mamluk-administered Egypt, where workers did not enjoy increased wages, but rather were burdened by increased rents. This example shows how the institutional configuration that a plague encounters has as much to do with the economic outcome as does the mere change in the supply and demand equation. Scheidel suggests that a similar, but less absolute, dampening of the wage-inflationary effect of the demographic decline was maintained by Roman Egyptian landlords. On the other hand, data shows that such a check was not maintained against workers during the Justinianic plague, when wages very clearly did rise. In summary, then, the interpretation of economic data from Roman Egypt and elsewhere, first sparked by Duncan-Jones in 1996, continues today, though there

⁹ Walter Scheidel, "Roman Wellbeing and the Economic Consequences of the Antonine Plague," in *L'impatto Della "peste Antonina,"* ed. Elio Lo Cascio (Bari: Edipuglia, 2012).

seems to have been a moderation in the magnitude of economic changes detected in the late second century, and a decrease in the degree of weight placed on its various interpretations.

The uncertainty over the interpretation of this economic data has not, however, led to a widespread adoption of the minimalist interpretation of the plague. The minimalist view, first put forward by J.F. Gilliam (1961), holds that the demographic effect of the plague was small and that both the ancient literary sources and many modern historians have exaggerated its significance.¹⁰ Though this view is supported by a minority, the consensus maintains that the plague had a large demographic effect.¹¹ The consensus is based primarily upon the identification of the disease as smallpox; if it was indeed smallpox, and was introduced to a population which had no immunity, as all the evidence suggests, then a high level of mortality must have occurred. Several attempts to mathematically model this mortality have been made on the basis of the modern experience with smallpox. The first to do so was Yan Zelener in 2003.¹² His model begins by assuming that the Roman population was growing in the first two centuries CE. He argues that, rather than accepting that the population of the Roman empire was static at 50-60 million (the canonical but uncertain figure), it should be assumed that the population of the empire could have been growing during the peace of the first and second centuries CE. With a population of 60 million in the reign of Augustus, he argues, and a Mediterranean carrying capacity of 100 million (based on an estimate of what the carrying capacity was in 1500 CE, before any developments in basic technology began to increase the

¹⁰ James Frank Gilliam, "The Plague under Marcus Aurelius," *American Journal of Philology*, 1961, 225–51.

¹¹ W. V. Harris, "The Great Pestilence and the Complexities of the Antonine-Severan Economy," in *L'impatto Della "peste Antonina"*, ed. Elio Lo Cascio (Bari: Edipuglia, 2012).

¹² Zelener, Yan, "Smallpox and the Disintegration of the Roman Economy after 165 AD," PhD dissertation (Columbia University, 2003).

carrying capacity of the land to modern levels), then population could have grown until it reached its capacity or until it was reduced by disease or another factor.¹³ Not only is such growth in the first two centuries CE conceivable, Zelener argues, but it is almost required by the economic and archaeological evidence, which record very high levels of long-distance trade and general material prosperity in the period. Such prosperity is impossible to explain, Zelener believes, solely on account of non-demographic factors such as growth in agricultural productivity or reallocation of resources away from the military and toward peaceful activities.

Using this assumption of a growing Roman population as his starting point, Zelener constructs a model which mimics the density dependence and wave-like nature of smallpox, a disease which typically returns at periodic intervals after an initial outbreak. Zelener's model iterates every 10 days (approximately the incubation period of *Variola major*, the virus responsible for smallpox), and tracks not only the infected population as the virus spreads, but also the portions of the population which are either immune or susceptible to infection. The most significant unknown variable is what Zelener calls the "coefficient of transmission"—i.e. the probability of infection of a susceptible individual when they interact with an infected individual. A second variable is the "infection rate", a measure of the number of people with whom an infected individual has sufficient contact that he or she might infect them. Together, these two variables govern the speed with which the disease spreads through the population. Having thus created this basic configuration, Zelener then generates a variety of alternative models of varying levels of mathematical complexity. While his earlier models depict the empire as if it was homogeneous with regard to density and static with regard to

¹³ Ibid., 72-8.

mobility, his later ones become more complex, breaking the empire down into four “zones,” from Zone 1, which includes that parts of the population which are most densely concentrated and mobile, i.e. the cities and the army, to Zone 4, which includes the most remote parts of the empire, such as Britain. Having introduced this complexity, the simulation shows that after the initial wave of the epidemic, a new equilibrium state is not reached for 10 to 15 years, and the epidemic proceeds in a complex or even somewhat chaotic manner—the initial outbreak is not the most deadly, but only the first in a series of relatively small outbreaks of varying sizes which spread throughout the empire. This kind of model shows how important variations in density and mobility are to the outcome of an epidemic, and also provide a sobering reminder of the complexity of the real world, and of the difficulty inherent in modeling the non-linear processes which characterize it.

Zelener’s model is also notable for its assumption of 25 percent smallpox mortality—a significant increase over the 7-10 percent mortality proposed by the Littmans, and even over the 13-15 percent mortality that the Littmans claimed for cities and armies. All these figures are below the 30 percent mortality rate of smallpox itself because of the way smallpox strikes a population: in a smallpox epidemic, less than 100 percent of the population is infected—some being spared by chance, and some by immunity—and out of this fraction of the population which is infected, 30 percent die; the overall death toll is thus smaller than the 30 percent mortality rate which the CDC’s number would suggest. Zelener is able to justify his higher mortality rate in part because he adds the problem of density dependence; in this view, smallpox seeks out pockets of population density (perhaps towns or army camps) which are dense enough to support an epidemic; after striking such a pocket of density, it moves on. But those who were infected and survived the disease are thereafter immune to infection, meaning

that during an epidemic the pool of the vulnerable is constantly shrinking, but that during the subsequent period of quiescence the number of the vulnerable is continually being augmented by new births or immigrants from uninfected areas. After a time, therefore, a fresh outbreak is possible. The plague thus occurs in waves, striking an area and then striking it again several years later. The incidence of mortality for the empire as a whole would thus look like a graph stretched out in time, on which there are numerous spikes of mortality as the disease finds pockets of sufficient density but insufficient immunity and then exhausts those pools of susceptible people and subsides for a time.

Zelener's model is interesting and sophisticated, but I believe it harbors a problem, at least insofar as the estimation of a resultant mortality rate is concerned. The problem, simply stated, is that it confounds individuals belonging to different generations. If, over a span of twenty or thirty years, the plague returns to the same area once or twice and causes additional mortality beyond its first victims, many of these new victims will be children of the survivors of the former outbreak; thus their mortality cannot simply be added to the mortality of the earlier generation. The total mortality would have to be somewhat less than one would expect if one simply added the death tolls from each of the epidemics, though I must confess I do not know mathematically how one would arrive at a precise figure. I think therefore that Zelener's 25 percent mortality rate might have to be moderated somewhat, unless it can be shown that other follow-on demographic effects would have produced further population contraction in addition to the mortality of the disease itself (a model created by Paine and Storey (2006), which I analyze later in this paper, does exactly that).

The problem with estimating the mortality rate does not end here, however, because the Littmans also commit an error, I think, in arriving at their 7-10 percent mortality rate.

They claim that in a smallpox epidemic it is typical for 60-80 percent of the population to catch the disease, and that it is typical for 25 percent of the infected to die.¹⁴ If one does the math, however, a 25 percent mortality rate for those infected compounded with a 60-80 infection rate would result in 15-20 percent dying. Why, then, do the Littmans provide a lower figure? Because, it seems, they are actually estimating mortality based on several modern smallpox epidemics. They cite 9.6 and 9.8 percent mortality rates from two eighteenth century outbreaks in Edinburgh, 16-18 percent in Mexico City in 1779/80, 7 percent in Mexico City in 1797/98, and 7-8 percent in West Prussia in 1874. While these modern data are interesting and do provide some context for considering the ancient Roman experience, it needs to be remembered that the Roman population had no immunity to smallpox—which certainly can not be said of any of these eighteenth and nineteenth century cities. The eighteenth century European cities in this data set may also have been effected by the growing use of smallpox inoculation, which began in the mid eighteenth century, and the West Prussian case may have been effected by Edward Jenner's cowpox vaccine which began to be used in the early nineteenth. I think that it can definitely be stated, therefore, that these modern outbreaks are not a good parallel to the ancient Roman empire; a virgin population would have suffered much more severely from the first outbreak in the 160s, and perhaps also from the subsequent outbreak in 189. Out of all these figures, I think that the 15-20 percent mortality rate is a good starting point—this is the number I calculated based on the Littmans' estimate of 60-80 percent infection and 25 percent mortality among the infected; however, I would raise this to 18-24 percent, which is the same number recalculated on the basis of the CDC's figure of 30 percent mortality for those who catch the disease. It is also perfectly

¹⁴ This differs somewhat from the CDC estimate of 30 percent mortality for those infected.

conceivable that this figure could be as high as Zelener's 25 percent or even higher, but I think that, in the absence of a strong argument for the higher figure based on specific predictions from Zelener's epidemiological models, then the more conservative 18-24 percent figure is preferable as a working estimate. It should be remembered, of course, that even the lowest of all these estimates provides for breathtaking numbers of deaths, so that the specific number is not something to get too bogged down in. Any of these estimates provides a mortality situation which produces significant demographic effects—effects which are sufficient for the purposes of the argument I am making in this paper. Despite my criticisms, and whatever the precise mortality figure may turn out to be, I think that both Zelener and the Littmans provide important tools for thinking about the Antonine plague and for constructing a believable picture of its effect on Roman demography. Zelener's model is important because it provides a range of hypothetical pictures of how this mortality might have played out in the decades after 165, and because it does so with a mathematical and epidemiological sophistication absent from earlier analyses. The Littmans' analysis is also important, because it provides a more conservative picture grounded in the sources and developed with an eye toward the smallpox experiences of the eighteenth and nineteenth centuries.

Hypotheses of a Plague-Christianity Interaction

Despite the extensive scholarship on the Roman plagues, which I have summarized above, the possibility of a connection between these plagues and the growth of Christianity has been investigated by only two scholars that I know of. The first was William H. McNeill, who advanced two hypotheses in his 1976 book *Plagues and Peoples*, which together

provided a mechanism by which the plague may have benefited Christians demographically.¹⁵ First, he argues, the Christian duty of caring for the sick may have significantly reduced the mortality rate of fellow Christians during the Antonine plague. This hypothesis is based on the presumed effectiveness of basic nursing practices—or even the simple provision of food and water—in reducing smallpox mortality. As a result of having a lower mortality rate, Christians would have lost fewer of their number to each wave of the plague than would their pagan neighbors. Second, McNeill argues, Christians provided such care not only to their own members, but also to outsiders, and as a result they would have won the sympathy and perhaps even the conversion of some of those pagans whose lives they had saved.

Having suggested these possibilities, McNeill did not investigate further. No one else seems to have touched on the subject until 1996, when sociologist Rodney Stark attempted to show a connection as part of his book on Christianity's expansion.¹⁶ Beginning from McNeill's suggestions, and extending them with an application of sociological theory and touch of what Stark calls "the arithmetic of the possible," i.e. speculative numbers,¹⁷ Stark advanced two hypotheses of particular interest. First, he argued that the Christian social system was better able to cope with the catastrophe of the plagues than was the pagan social system, because Christians possessed greater community solidarity and because they had developed a system of social services, including care for the sick, which went beyond what was available to their pagan neighbors. Together, Stark argues, community solidarity and care

¹⁵ McNeill, William H., *Plagues and Peoples* (New York: Doubleday, 1976), 121.

¹⁶ Rodney Stark, *The Rise of Christianity: A Sociologist Reconsiders History* (Princeton: Princeton University Press, 1996).

¹⁷ Stark, 4.

for the sick would have resulted in a lower Christian mortality rate during plagues.¹⁸ Stark's second hypothesis is based on his first; he argues that because the plague would not only have killed people, but would also have destroyed social bonds, it follows that, if pagans had died at a greater rate than Christians, then the bonds between pagans would have been destroyed at a greater rate than Christian-pagan or Christian-Christian bonds. This would have meant, Stark argues, that the bonds of a given pagan to other pagans—bonds which might have restrained him or her from converting to Christianity—would have been lost more frequently, while a greater number of bonds to Christians would have survived, thus leading to an increased likelihood of conversion. This hypothesis is based on a theory of religious conversion largely developed by Stark himself, which holds that religious conversion usually follows social networks, being spread from one person to his or her close contacts, such as family members, friends, and coworkers, rather than through missionaries knocking on the front door, as it were. I find this idea compelling, though perhaps not specifically in connection with the idea of differential mortality rates; I will revisit the social network theory of religious conversion later in this paper, to argue for a slightly different idea of how conversion augmented the demographic expansion of the Christian population.

Stark continues by showing arithmetically how his hypothesized differential mortality rate could have effected the growth of Christianity. First he calculates the rate of growth of Christianity, estimating that it began with perhaps 1,000 followers in 40 CE and that it grew over three centuries into an empire-wide population of about 5 to 7.5 million in the year

¹⁸ Stark, 74-5.

300.¹⁹ If these figures are accurate, it represents a change which would have required a constant growth rate of 40 percent per decade.²⁰ Using this as a rate of conversion, Stark illustrates the effect of reduced plague mortality by creating a hypothetical city of 10,000 people in which there are initially 9,660 pagans and 40 Christians (0.4 percent Christians) in the year 160, and shows how a combination of religious conversion (at 40 percent per decade) and reduced mortality would have greatly accelerated Christian growth, relative to a ‘control’ scenario in which conversion was the sole driver of growth.

I do not believe, however, that the details of Stark’s mathematics in their present form are entirely valid. I see several areas for potential improvement. First, I think that the increase in Christianity’s numbers due to plague should constitute part of the predicted 40-percent-per-decade growth, rather than being additive to it. The 40 percent growth rate is calculated based on beginning and ending population figures, from 30 CE to 300 CE, and the plagues fell within this period, so their effects should be included as part of the 40 percent growth per decade predicted by the beginning and ending population figures. The important thing is to isolate the effect which the plagues by themselves might have had, without conversion. A second problem is that Stark assumes two plague events, in 165 and 251, each of which resulted in a 30 percent mortality rate for the Roman population. A more realistic model would have had an 18-24 percent mortality event in 165, followed by smaller mortality events in 189, 251, and 310 CE. To Stark’s credit, he made it very clear that his model city was

¹⁹ Stark derives his figures for the Christian population size from the estimates of: Erwin R. Goodenough, *The Church in the Roman Empire*, The Berkshire Studies in European History (New York: Henry Holt and Company, 1931); Michael Grant, *The History of Rome* (New York: Faber & Faber, 1978); Ramsay MacMullen, *Christianizing the Roman Empire: (A.D. 100-400)* (New Haven: Yale University Press, 1984).

²⁰ Stark, 4-6.

purely a hypothetical exercise intended to illustrate the effect of differential mortality in a plague, rather than to show an actual historical process; and his hypothetical city does illustrate that such an effect could potentially be very large.

However, the most basic problem is that McNeil and Stark's assumption of lower mortality rates for Christians remains unproven. To arrive at his conclusions, Stark assumes a 30 percent mortality rate during the plague for the Roman population, versus a 10 percent mortality rate for the Christian population, on the belief that nursing alone could reduce mortality by $2/3$. Unfortunately, this remains an assumption throughout Stark's analysis—he does not attempt to prove that such a reduction in mortality by nursing is possible. It is plausible to imagine that nursing could have resulted in some degree of improvement in mortality rates, but to prove that it did—and to know what degree of improvement could be expected—it would be necessary to do further investigation into the modern experience with smallpox. Specifically, it would be necessary to ask whether physicians have noted that nursing can have such a large impact on smallpox survival, without inoculation, medicine, or other advanced care. Even if such an effect has been noted, the argument would still be a difficult one to make, because a second question immediately presents itself: how many pagans were properly cared for, and how many were abandoned with no nursing care? We get various hints in the sources of ancient plague sufferers being abandoned to die miserably (in Thucydides, for instance), and it seems that flight was indeed a well-respected strategy in times of plague—apparently employed by Galen, for instance, who fled from Rome to Pergamum, and later by the emperors who fled from Aquileia to Rome, just to provide two instances from the 160s. But certainly there must also be counter-evidence to such flight and abandonment; certainly there must have been many pagans who did care for the sick, and

many Christians who fled. The issue is further confounded by the question of how the mortality rates of those who stayed and cared for the sick would have been effected by their proximity to the disease—by staying, did they die more frequently, versus those who fled and saved themselves? Quantifying the degree to which all these phenomena occurred would, of course, be impossible. It seems, therefore, that the difficulties with the differential-mortality hypothesis are insurmountably large, and thus there seems to be a need for an alternative model, if any link between the plagues and the growth of Christianity is to be posited at all.

What could such an alternative model look like? Instead of thinking in terms of the numbers killed by the disease itself, it may be profitable to think in terms of the recovery or non-recovery of the population after the plague. Rather than a differential mortality rate, there might have been a differential fertility rate. I believe that it is reasonable to hypothesize that such a differential fertility rate could have existed. The practice of child exposure, combined with a preference for sons over daughters, probably led to an imbalanced sex ratio in the Roman population. Had such an unbalanced sex ratio existed, I think it is safe to infer that, whatever the extent of the imbalance, it would have been an impediment to the fertility response that would have been necessary for the population to rebound after a massive mortality event. Furthermore, it is well attested that Christians were vehemently opposed to the practice, as I will show later in the paper, so it is reasonable to suppose that they did not suffer from a similar imbalance, and would thus have had an easier time recovering in the wake of the plagues. The difference between these two rates of population growth may have contributed substantially to Christian numbers relative to the Roman population as a whole, and may help to account for the rise of Christianity in the first three centuries CE.

Evidence of Infant Exposure

In order to support this alternative model, it will be necessary to show not only that infant exposure was a common practice and that it frequently involved sex selection, but also to show that it had a hand in shaping Roman demography. Both exposure and sex selection are sensitive issues by their very nature, and hard for historians to deal with, because even the most clinically dispassionate investigator will find it difficult to treat the phenomenon in a detached and non-judgmental way, and perhaps also because of a desire to see Greco-Roman culture in a more positive and ‘civilized’ light than would be implied by an admission of the widespread practice of such cruelties. In spite of these difficulties, both issues have received a fair amount of attention, especially since the 1970s, as ancient historians have become more interested in questions of social history. Inevitably, this increasing interest caused child exposure to become the occasion of some disagreement among historians. Donald Engels (1980) attempted to minimize the importance of infant exposure, and denied that substantial sex ratio imbalances were possible. He maintained this position on the basis of his belief that the Roman birthrate must already have been in equilibrium with the non-infanticidal death rate, and that significant levels of infanticide would thus have pushed the population into demographic contraction.²¹ However, William Harris (1982) showed that such reasoning is fallacious and that there is no theoretical barrier to believing that infanticide could have been practiced extensively.²² Harris argued that any number of scenarios could have played out depending on a range of demographic variables: e.g. infanticide could have reduced growth to

²¹ Donald Engels, “The Problem of Female Infanticide in the Greco-Roman World,” *Classical Philology* 75, no. 2 (April 1, 1980): 112–20.

²² W.V. Harris, “The Theoretical Possibility of Extensive Infanticide in the Graeco-Roman World,” *The Classical Quarterly*, New Series, 32, no. 1 (January 1, 1982): 114–16

stability, or reduced stability to decline (as Engels had argued), or its effect on population growth could have been small due to compensation by other factors, such as the low age of Roman girls at marriage—but he showed that the practice of exposure by no means necessarily led to population decline, and that it should not therefore be ruled out on theoretical grounds.

Beyond this one moment of controversy, it seems that a durable consensus has been achieved in the field, to the effect that infant exposure was widely practiced in the Hellenistic and Roman world, and that sex selection was common as well.²³ The consensus is most thoroughly and convincingly defended by Harris in a second article in 1994, in which he chronologically contextualizes child exposure within the history of the gradual growth of opposition to it by Jews, Stoics (and non-Stoic pagans), and Christians over the course of Roman history.²⁴ Although the development of this consensus has largely settled the question of infant exposure, sufficient uncertainty continues to surround the question of sex-selective exposure and sex ratios, that an in-depth examination of the available evidence will be necessary here.²⁵

²³ Of the earlier general treatments of the subject, the most significant all share the consensus view: A. Cameron, “The Exposure of Children and Greek Ethics,” *The Classical Review* 46, no. 3 (July 1, 1932): 105–14; P. A. Brunt, *Italian Manpower, 225 B.C.-A.D. 14* (London: Oxford University Press, 1971), 146-54; Sarah B. Pomeroy, “Infanticide in Hellenistic Greece,” in *Images of Women in Antiquity*, ed. Averil Cameron and Amélie Kuhrt (Wayne State University Press, 1983), 46, 68-70, 164-70. Only Parkin distances himself, judging the evidence to be too weak to generalize about the phenomenon: Tim G. Parkin, *Demography and Roman Society* (Baltimore: Johns Hopkins University Press, 1992), 95-98.

²⁴ W.V. Harris, “Child-Exposure in the Roman Empire,” *Journal of Roman Studies* 84 (November 1994): 1–22.

²⁵ In addition to Parkin’s skepticism, Walter Scheidel, for instance, has recently argued that sex ratios were less unequal than has often been thought: Walter Scheidel, “Greco-Roman Sex Ratios and

One reason to believe that sex-selective exposure of female infants was relatively common comes from John M. Riddle's important analysis of abortion and contraception in antiquity.²⁶ Although previous historians had not credited ancient physicians with a knowledge of efficacious abortifacient or contraceptive agents, and had thus not considered these methods of family limitation significant in the ancient world, Riddle argued persuasively that the herbs recommended by ancient medical writers such as Soranus and Dioscorides indicate that they possessed a greater level of accurate medicinal and herbal knowledge than had formerly been attributed to them. Riddle also argues that this knowledge on the part of male physicians probably derived from an active folk tradition maintained primarily by women. If this is correct, it would mean that such information was not confined to the ivory tower, as it were, but was available to those who might want to avoid or terminate a pregnancy. Riddle's argument has had a significant effect on scholarly opinion, and it is now generally believed that effective contraceptives and abortifacients were relatively easily accessible to women in antiquity. This, in turn, necessitates a change in the terms of the discussion over child exposure and sex ratios, because it is reasonable to conclude that if women in antiquity could practice family limitation by means of oral contraceptives or abortifacients, then they would have had little motivation to practice child exposure on a wide scale unless they had some ulterior motive to do so. Besides the desire to dispose of deformed or illegitimate children, the only motive which may easily be found is the desire to select the

Femicide in Comparative Perspective," *Princeton/Stanford Working Papers in Classics*, 2010, <http://www.princeton.edu/~pswpc/pdfs/scheidel/011003.pdf>.

²⁶ John M. Riddle, *Contraception and Abortion from the Ancient World to the Renaissance* (Harvard University Press, 1994); see also John M. Riddle, *Eve's Herbs: A History of Contraception and Abortion in the West* (Harvard University Press, 1997).

sex of the child. It is thus likely that contraception and abortion would have been used as the preferred tools of family limitation, while infant exposure would have been used primarily for sex selection, since it cannot be assumed that deformity or even illegitimacy would have been so common as to account for the degree of exposure which the sources imply. This line of reasoning thus bolsters the idea that infant girls could have been exposed on a relatively large scale.

Having outlined the terms in which child exposure has been discussed previously, it is necessary to look at the evidence which supports my hypothesis that child exposure was performed frequently enough to have produced an imbalanced sex ratio for much of the Roman population. I will begin with one of the most well-known literary references, a poem of the third century BCE by Poseidippus,²⁷ in which exposure, sex selection, and the economic motives that parents may have had for both practices are all woven together and neatly summarized:

Everyone, even if he is poor, rears a son,
But exposes a daughter, even if he is rich.

The poem is certainly a morbid exaggeration, but is probably all the more revealing for that reason. I take it as a commentary on, and perhaps also a criticism of, the general social acceptance of child exposure as a normal, widespread practice. If the phenomenon were not real and if the audience possessed no shared awareness of its reality, the exaggeration would not have produced its intended comedic effect. If the joke was successful with its audience, it was successful precisely because sex-selective exposure was practiced quite commonly—but not quite to the universal degree that the poet's exaggeration declares. The poem's mention of

²⁷ Poseidippus, Fragments II, Kock, quoted from Harris (1994), 4.

poverty as the force which impels a parent to expose a child, also makes this poem a good starting point, as is the barbed line aimed at the rich who had no such excuse; both these points will be seen in greater detail later.

Some, however, might suspect that Poseidippus' poem amounts to nothing more than a disturbing witticism and that its exaggeration is too great to glean any information from it about the social reality of exposure. For the benefit of such skeptics, we should also look at a second, equally infamous source, a papyrus discovered at Oxyrhynchus, which is strikingly similar to the poem:

Hilarion to his sister Alis, many greetings.... Know that I am still in Alexandria.... I ask you and entreat you, take care of the child, and if I receive my pay soon, I will send it up to you. Above all, if you bear a child and it is male, let it be; if it is female, cast it out. You have told Aphrodisias, "Do not forget me." But how can I forget you? Thus I'm asking you not to worry. The 29th year of Caesar, Pauni 23.²⁸

This little slice of life from Egypt in 1 BCE is so completely candid and direct, so revealing of daily life, and yet so similar in its attitude to the Poseidippus couplet, and similar as well as to the many references to exposure found in drama and fiction,²⁹ that I think it can be taken as evidence that poetic or fictional sources, such as Poseidippus' poem, cannot simply be written off as so much poetry and fiction; there is, in other words, a social reality to the phenomenon. That reality, however, is a difficult one to understand from a human standpoint. The letter exhibits an unsettling juxtaposition of Hilarion's pitiless indifference to the child if it is female, on the one hand, and his concern for Alis on the other. His attitude seems all too

²⁸ P.Oxy. iv. 744. <http://papyri.info/ddbdp/p.oxy;4;744>. Translation from APIS: <http://wwwapp.cc.columbia.edu/ldpd/apis/item?mode=item&key=toronto.apis.17>.

²⁹ E.g. Ovid *Met.* IX 678, or Lucian, *Hetairikoi dialogoi* 2.1. Many examples from New Comedy are catalogued by Gilbert Murray, "Ritual Elements in the New Comedy," *The Classical Quarterly* 37, no. 1/2 (January 1, 1943): 46–54.

similar to the callousness of Poseidippus's exaggerated gallows humor, but there is obviously no exaggeration and no humor in the letter. What kind of social world do we need to assume, then, in order to understand Hilarion's attitude? I think that three basic factors are at play. First, the business-as-usual tone of Hilarion's command to expose the child is only possible if there were a general social acceptance of the practice; Hilarion's request, in other words, must have been within the bounds of normal behavior, and what he was commanding his wife to do was not unusual or criminal. It is easy to believe that this was the case, given the array of literary sources which paint a similar picture, some of which I will examine later in this paper. Second, there must have been some sense in which infant children themselves were not regarded as real persons and were not considered a part of the social world and its sphere of protection until some later date when they achieved personhood and became the objects of affection and concern. This, too, is easy to imagine in an environment of high infant mortality, in which parents never knew whether their infant child would still be alive in a month or a year—an environment which must have produced a certain distance between parent and child as a psychological defense mechanism against the likelihood of death. Third, Hilarion must have had some strong motivation which led him to make the decision that a daughter was not at all worth rearing, but that a son certainly was. This motivation, I believe, can only have been economic in nature, as was alluded to already by Poseidippus, who weaves the pressures of both poverty and wealth into his poem.

I will return momentarily to the question of economic motive, because it is one of the most important aspects of the phenomenon, but before I continue I think it is necessary to question whether, by exposing the child, Hilarion was really condemning it to die. Was exposure necessarily a form of infanticide? There are a number of sources that would lead us

to believe that it was not. With regard to Roman Egypt, the evidence of Kopr-names (personal names which are thought to derive from *apo koprias*, ‘from the dunghill’) probably indicates the presence of foundlings, though conversely many who bore the name were probably the descendants of foundlings, thus producing an appearance of slightly inflated survival chances for an exposed child. It is unclear how the Kopr-names fit with another important piece of evidence from Roman Egypt, a second-century set of rules governing the administration of the Roman “special account” (*idios logos*) in which revenues were collected from the Egyptian people by a Roman official known as the *idiologus*.³⁰ One of the many harsh and confiscatory regulations the document contains is number 41:

If an Egyptian rears a child exposed on a dung heap and adopts him, a fourth of his estate is confiscated at death.

Seemingly the Roman administration thought that Egyptians whose heirs were foundlings did not deserve to pass on the whole of their estate, but it remains unclear why this would have been the case. Judging by the remainder of the document, however, it seemed that the Romans needed very little excuse to confiscate the estates of the Egyptians under their rule.

Presumably the law would have eliminated any desire to adopt a foundling as heir, but would not have prevented exposed children from being rescued and reared for other purposes, such as slavery or prostitution. It is clear that at least some foundlings were raised, not only because of the Kopr-names discussed above, but also because a further regulation of the *idios logos* (no. 92) declares that a child rescued from a dung heap would not be allowed to become a priest.

³⁰ Also referred to as the Gnomon of the Idios Logos. From the Berlin Papyrus No. 1210 (= *Select Papyri*, no. 206 = *FIRA*, vol. I, no. 99), translated in Naphtali Lewis and Meyer Reinhold, eds., *Roman Civilization: Selected Readings*, vol. 2, 3rd ed. (Columbia University Press, 1990), 298-302.

The rules of the *idios logos* are not the only case in which we have evidence of the Roman provincial administration's attempt to deal with the issues created by exposure and the subsequent survival of foundlings. In the first century CE, Pliny writes about the problem of foundlings in the province of Bithynia and Pontus, in a letter to the emperor Trajan:

A very considerable question, Sir, in which the whole province is interested, has been lately started, concerning the state and maintenance of what are called *foundlings* (θρεπτούς). I have examined the rulings of former Princes ..., but not finding any thing in them either particular or general relating to the Bithynians, I thought it necessary to apply to you for your directions....³¹

Pliny then mentions a half-dozen letters and edicts issued by past emperors on the subject. His description makes the issue sound like a significant one: the 'whole province' is interested, and there is a substantial legal tradition on the subject. Trajan's reply adds to this sense of importance when he says that "the question concerning free-born persons who have been exposed as infants and reared in slavery by those who took them up, has been frequently discussed..." However, Trajan finds no generally applicable law issued by former emperors, so he rules that "those who desire emancipation upon this ground should not be debarred from publically asserting their freedom, nor be obliged to purchase it by repaying the cost of their maintenance."³² Like the Egyptian regulation, it is difficult to imagine that such a ruling would be beneficial for exposed children, unless one assumed that an early death would be preferable to a life of slavery; essentially, Trajan is making it riskier to raise an exposed child, by opening the possibility that they could demand freedom later. Would this have exercised a dampening effect on the raising of foundlings? This is not an answerable question, but it is

³¹ Pliny, *Letters* x. 65. Translated in William Melmoth, trans., *Pliny: Letters*, vol. II, Loeb Classical Library (Cambridge: Harvard University Press, 1953).

³² Pliny, *Letters* x. 66.

worth thinking about. In any case, it is clear from the Pliny-Trajan correspondence as well as from the Egyptian evidence that not all exposed children died.

Another source which has been taken to suggest an even higher survival rate is Justin's mid-second century Christian apologetic in which he says that almost all children who were exposed, girls and boys alike, were raised for the purposes of prostitution.³³ The men who exposed these children would then unknowingly commit incest, he says, with their own children when they visited the local brothel. It must be remembered that Justin was writing in a rhetorical environment in which Christians were accused of sins worse than mere exposure of children, and he makes it seem that his statements constitute a defensive maneuver against such accusations. Probably his account should be taken a bit less seriously for that reason, though undoubtedly many children saved from exposure did end up in prostitution. In any case, his statement that almost all exposed children were saved from death and raised cannot be interpreted literally, not only because there is much other evidence that exposed children did die, but also because just two chapters later, Justin himself states that Christians feared to expose children because they might not be picked up and might therefore die, making the parents murderers.³⁴

In spite of these sources which point to some exposed children having been saved from death, the general assumption of the sources is that most exposed children would die, and as Harris points out, even those who were saved would often die within a few days as a

³³ Justin, *First Apology* 27.

³⁴ Justin, *First Apology* 29.

result of the experience, as was the case at foundling hospitals in later history.³⁵ Dionysius of Halicarnassus also indicates that death was the normal result of exposure when, in writing of the alleged law of Romulus concerning infanticide, he implies that exposure is the equivalent of the destruction of the child.³⁶ Likewise, various mentions of dogs and wild animals by ancient authors probably point to one of the more common fates that awaited the exposed children, not to mention cold and starvation.³⁷ In summary, then, while it is true that children were sometimes raised as foundlings, it is also true that many were not. The exact ratio is, of course, unknowable, but we can assume that the larger part probably died, as ancient authors themselves seem to have generally assumed.

Given such a range of outcomes—slavery, prostitution, a terrible death—what was it that impelled parents to subject their own children to exposure? As I mentioned earlier in connection with the letter of Hilarion and the poem of Poseidippus, it was very frequently an economic pressure which supplied the motive. In his letter, Hilarion's phrase "when I receive my pay" implies that, at the very least, he was not a rich man, and more probably he was living paycheck to paycheck, as it were. Plutarch also writes of poverty and exposure, when he says that poor men might decide not to rear their children for fear that a life of poverty or servility awaited them.³⁸ Such a life of servility could itself be a motive for exposure. While slavery and poverty were not at all the same thing in the Roman world, many slaves must have felt economic pressures similar to those felt by the poor, and this could sometimes

³⁵ Harris (1994), 10.

³⁶ Dionysius of Halicarnassus, *Roman Antiquities* II.15.2.

³⁷ Harris (1994), 8.

³⁸ Plutarch, *De amore proles* 5.

translate into a desire to avoid having children. Dio Chrysostom, for example, writes of the pressures experienced by slave families, and his account is interesting enough to quote here:

In the case of slave women ... some destroy the child before birth and others afterwards, if they can do so without being caught, and yet sometimes even with the connivance of their husbands, that they may not be involved in trouble by being compelled to raise children in addition to enduring slavery.³⁹

Apparently, when the duties of servility clashed with those of family life, slave women often balanced the equation by practicing family limitation. It is interesting, however, that in Dio Chrysostom's account, exposure appears to have been an illicit practice for slave women, whereas that rarely appears to be the case in other sources. I suspect this simply points to the fact that, as slaves, these women (and also their husbands) did not have the right to dispose of their children as they wished, but were obligated to do with them as their owners wished. Exposure would therefore have been practiced surreptitiously so as to avoid the double burden of slavery and childcare responsibilities.

However, poverty and slavery were not the only economic conditions which might lead to exposure. Wealth, too, seemed to inspire some Romans to expose their children. Musonius Rufus, a Stoic of the first century CE, states as much in his lecture entitled "Should every child born be raised?"⁴⁰ Musonius answers the titular question with a strong affirmative, citing Augustus and Zeus as his authorities, but states that for some wealthy Romans, the problem of the division of the family inheritance was an inducement to limit the number of their offspring in order to ensure that the family's wealth and position would be preserved for

³⁹ Dio Chrysostom, Discourse 15, vol. 2, trans. J.W. Cohoon (Cambridge: Harvard University Press, 1939).

⁴⁰ Musonius Rufus, Lecture 15, trans. Cora Lutz, vol. 10, *Yale Classical Studies* (Yale University Press, 1947).

subsequent generations rather than being divided between many offspring. However, Musonius is not content to treat the problem as a purely economic one. He also sees in it a moral dimension, and argues that neither wealth nor poverty is a sufficient reason to practice family limitation, because of the damage done to the common good by childlessness, and because those who fail to raise children fail to fulfill their duties to their ancestors. Other Stoics, such as Hierocles, echo these sentiments.⁴¹

A similarly moralistic dimension is explored by Tacitus in the *Germania*. Tacitus notes that the Germans considered it immoral either to limit the number of one's children or to kill children after birth, and he says that among the Germans childlessness holds no advantages.⁴² It is quite clear that Tacitus's comments should not be read as simple commentary on the Germans, but also as a commentary on Roman society. Though Tacitus does not specifically condemn child exposure or family limitation, his statement that the Germans did not do such things is embedded in a narrative which both praises German virtues and emphasizes the dissimilarity of such noble savagery from the 'civilized' life of the Romans. As such, his comments on exposure and family limitation should be taken as an implicit criticism of Roman reproductive practices, which are made to seem decadent by comparison. Good habits, Tacitus adds, go farther among the Germans than good laws do among the Romans. I take his 'good laws' to be the marriage laws of Augustus, consisting of the *Lex Julia* of 18/17 BCE and the *Lex Papia Poppaea* of 9 CE, which together were intended to shore up Roman morality by promoting marriage, punishing adultery, and

⁴¹ Strobæus, *Anthology* 4.75.14. Translated in Ilaria Ramelli, *Hierocles the Stoic: Elements of Ethics, Fragments, and Excerpts*, trans. David Konstan (Leiden: Brill, 2009), 79.

⁴² Tacitus, *Germania* 19-20. Tacitus also mentions that the Jews refuse to kill their children, both because they count it a crime, and because they seek to increase their numbers: *Hist.* 5.5.

discouraging celibacy. Though neither law deals with child exposure, they fit well into a sequence with the criticisms of the Stoics and of Tacitus because all spring from a similar anxiety concerning the deterioration of Roman morals and the inadequacy of Roman reproduction. Each of these sources in its own way adumbrates a different aspect of the reforming and moralizing spirit of the period.

The Augustan legislation failed to reform Roman society or to change the course of Roman demographic history, however, because the deeper economic causes which shaped Roman reproduction remained unchanged. In addition to poverty, the other major economic motive behind exposure was dowry. Of all the economic factors which impelled families to expose their children, I find the dowry to be the most interesting: it is less immediate than poverty, but in the long run probably had a greater hand in shaping the practice of exposure. When a daughter was born, her parents had to consider that eventually, in order to marry, they would need to provide a dowry. For this reason, a daughter was an expensive child to raise. Ovid makes this the motivating factor behind a story of exposure in the *Metamorphoses*.⁴³ Exploiting the obvious dramatic possibilities latent in child exposure, Ovid writes of Ligdus, a devout but poor man, whose wife is with child. Emotionally distraught but impelled by cruel necessity, Ligdus tells his wife:

“There are two things I pray to heaven for
 On your account: an easy birth and a son.
 The other fate is much too burdensome,
 For daughters need what Fortune has denied us:
 A dowry. ...”

⁴³ Ovid, *Met.* IX 960 ff., the story of Iphis and Ianthe. In Charles Martin, trans., *Ovid: Metamorphoses* (New York: Norton, 2004).

Ligdus goes on to command his wife that she should expose the child if it is a girl—and he is explicit that this would entail death—but to keep it if it is a boy. As the story unfolds it turns out that the child is indeed a girl, but she is saved when her mother tricks Ligdus into thinking she is a boy. Years later, on the eve of her wedding day, on which she is to be married to another girl—and thus on the eve of the trick being discovered—it is only by the miraculous intervention of the goddess Isis that disaster is averted, and she is transformed into a man. This supernatural resolution merely serves to underline in a vivid way the doom that awaited the many girls who were not so lucky as to catch the goddess' attention. The story is fantasy of the purest sort, but no less revelatory for that. Ovid shows, and his audience undoubtedly shared, a keen awareness of and interest in the phenomenon of exposure, and especially in its emotional aspects. But Ovid is also aware of the economic underpinning of exposure, namely, the dowry system and the coercion it exercised upon parents.

Since the size of the dowry was relative to wealth and social position, the situation Ligdus found himself in would not have been confined only to the poorer classes. Richard Saller writes that in classical Athens, dowries could range from under 5 to almost 20 percent of the family's wealth, whereas in Rome they ranged from 5 to 10 percent, and often included land.⁴⁴ It is not a very large stretch to imagine that such a requirement must have encouraged families to think twice before raising a daughter, though this might have been slightly less true for Roman as opposed to Greek families, since their dowries were slightly smaller. The connection between dowry and female infanticide is further substantiated by modern history.

⁴⁴ Richard P. Saller, "Household and Gender," in *The Cambridge Economic History of the Greco-Roman World*, ed. Walter Scheidel, Ian Morris, and Richard P. Saller (Cambridge: Cambridge University Press, 2007), 93-96.

In China prior to the twentieth century, dowry was one of the chief factors encouraging female infanticide.⁴⁵ Both practices were eliminated after the revolution in 1949, though the One Child Policy and the advent of ultrasound brought back imbalanced sex ratios in the 1970s. Likewise in India, the need to provide a dowry has been one of the main factors that has led to imbalanced sex ratios, and though the practice has been curbed somewhat by government opposition, imbalanced sex ratios persist to the present day especially in the northern and western states.⁴⁶ These examples illustrate the important role of dowry in the question of female infanticide, and there is no reason to suppose that the need to provide a dowry would not have had a similar effect upon families in the Greco-Roman world.

Though I have by no means exhausted all the literary evidence of the practice of infant exposure from throughout antiquity, I think I have reviewed enough to make the most important point, which is that infant exposure was a well-known and widely practiced phenomenon and that it was very frequently linked with the preference for sons over daughters. In light of Riddle's demonstration of the efficacy of the oral contraceptives and abortifacients known in the ancient world, it seems that sex selection was one of the major motivations for practicing infant exposure. Though it was deplored by some philosophers and literary elites, and though it helped to inspire laws and welfare schemes designed to promote marriage and childrearing, such as the *Lex Julia* and *Lex Papea Poppaea*, as well as the *alimenta* system which I will address later, all the measures to combat it were unsuccessful because of their failure to alter the fundamental economic motivations which lay behind it,

⁴⁵ Brigitte H. Bechtold and Donna Cooper Graves, "The Ties That Bind: Infanticide, Gender, and Society," *History Compass* 8, no. 7 (July 1, 2010), 713.

⁴⁶ Fred Arnold, Sunita Kishor, and T. K. Roy, "Sex-Selective Abortions in India," *Population and Development Review* 28, no. 4 (December 1, 2002): 759–85.

such as poverty and the dowry system. As such, exposure and infanticide of females remained a frequent practice throughout the entire period for much of the population of the Hellenistic kingdoms and the Roman empire, and there is no reason to believe that this substantially changed prior to the Antonine plague of the 160s.

Greco-Roman Sex Ratios

The evidence of infant exposure from literary sources is significant, but it does not exhaust the data at our disposal. I turn now to several data sets which provide information about sex ratios for certain populations around the Mediterranean in various periods. All of these data at least outwardly appear to show a shortage of females, so the primary question is whether they can stand up to critical scrutiny; if they can, then they may offer significant evidence in favor of the hypothesis that sex-selective infant exposure reduced the number of females in at least some Mediterranean populations. These sources have frequently attracted the attention of historians, and some historians have taken them as evidence of sex-selective infant exposure, but others have made strong arguments in the opposite direction, so this section of my paper will attempt to weigh these debates to see if any of the data sets support my thesis.

A hundred years after the creation of the Augustan marriage legislation, the concerns it embodied had not diminished, but had led to the creation of the imperial *alimenta* system under Trajan. As opposed to the moralizing tendency of the marriage law, the *alimenta* moved in a purely economic direction, promoting population growth by financially supporting the children of poor families in Italian towns. The most important evidence concerning the system

is from an inscription in Veleia, dating to 109-112 CE, which records in great detail the financial basis of the program as it was implemented in the town.⁴⁷ This inscription, also known as the Table of Veleia, is interesting because it records the number of boys and the number of girls who were to receive the grant, thus potentially providing some information about the sex ratio of the children in the town. The inscription says that 263 legitimate boys, 1 illegitimate boy, 35 legitimate girls, and 1 illegitimate girl would be supported. These numbers yield an extraordinarily high sex ratio of 733, i.e. 733 males for every 100 females. Considering that the sex ratio at birth should be about 105 in a natural population, these numbers, if they are taken at face value, would represent an extreme shortage of females. One possible interpretation is that female infanticide radically distorted the sex ratio of the children of impoverished parents in Veleia. However, the sex ratio is so extreme that other explanations must be sought.

Richard Duncan-Jones points out that the total number of children supported by the grant adds up to exactly 300, suggesting that this is a number which was decided upon by an administrator when the program was initiated.⁴⁸ He also points out that Veleia is a large enough town that there would have been more than 300 poor families living there. Thus there would have been competition to receive the grants, and it would be reasonable to assume that each family was limited in the number of children for which it could receive payments—probably to just a single child. Families with both male and female children would have been

⁴⁷ *CIL*, vol. 12, no. 1147 (= Dessau, no. 6675 = *FIRA*, vol. III, no. 116), in Naphtali Lewis and Meyer Reinhold, eds., *Roman Civilization: Selected Readings*, vol. 2, 3rd ed. (Columbia University Press, 1990), 256-7.

⁴⁸ Richard Duncan-Jones, "The Purpose and Organisation of the *Alimenta*," *Papers of the British School at Rome* 32 (1964): 123-46.

forced to choose whether to receive the grant for a son or for a daughter, and because the grant was for a sum of 192 sesterces per year for a legitimate boy, but only 144 sesterces per year for a legitimate girl (and slightly less for illegitimate children), the choice would not have been a difficult one. Not only did boys receive a 33 percent larger sum each year, but they might also have been expected to receive the grant for several years longer than girls, who married quite young. As a result, boys would have received a larger sum on a monthly basis, as well as a larger sum over the course of their childhood. We can therefore expect that the only families receiving *alimenta* for a daughter would have been families which had no sons eligible for the grant.

The data from the inscriptions show that 12 percent of the children were female, and on the basis of Duncan-Jones' argument, it is reasonable to assume that each child represents one family; thus, only 12 percent of the families receiving *alimenta* received it for a female child, meaning that 88 percent of the families had at least one male child. The question thus becomes: is it reasonable to think that 12 percent of the families had only female children? Is this percentage higher or lower than we might expect for a natural population? I am not equipped to answer this question here, but it would be an interesting avenue for further research. A computer model of the *alimenta*-receiving population in Veleia which could predict what these percentages might have been under various demographic regimes would probably be quite illuminating. As it stands, however, the Veleia data set cannot either support or undermine the hypothesis that female infanticide produced imbalanced sex ratios in Roman populations. It is interesting, though, to see what a large effect in the distribution of aid was produced by the inequality in the amounts given to boys and girls. It might even be argued that the *alimenta* was counterproductive, in the sense that it so pointedly favored boys over

girls that the program itself might have become yet another reason for parents to prefer male over female children, especially if they only had one child.

Though the *alimenta* inscription at Veleia does not provide a useful sex ratio, there are other data which can be analyzed for sex ratios in a similar way. One of the most interesting data sets comes from the Anatolian city of Miletus: it is a set of inscriptions on the walls of the Delphinion, a shrine to Apollo, which document the enfranchisement of several hundred mercenaries and their family members, dating from the late third through the early second century BCE, but mostly between 228 and 220.⁴⁹ The mercenaries came primarily from Crete, as well as from the Ionian islands and elsewhere, and were settled in Myus, a city controlled by Miletus. What makes the data most valuable is that they adhere to a standardized format, generally listing the man, his wife, his sons, and his daughters; this format enables family ties to be reconstructed, so that we have much more than a simple count of undifferentiated people. Sarah Pomeroy, who collected and organized the data in a 1983 paper, says that the inscriptions are the most extensive body of evidence for Hellenistic Greek demography, and she takes them as strong evidence for female infanticide. In all, 766 people are recorded: 569 males and 193 females, for a sex ratio of 294. Only 215, or 27 percent, are children, a low number which can be explained by the fact that the adults are an unnatural population of immigrants, many of whom are unmarried soldiers. The children therefore belong to a subset of the adults, and the child-adult ratio is not as abnormal as it appears. Because the adults are a rather haphazard population, derived from many places, it is the children, belonging to a

⁴⁹ Data is collected in Sarah B. Pomeroy, "Infanticide in Hellenistic Greece," in *Images of Women in Antiquity*, ed. Averil Cameron and Amélie Kuhrt (Wayne State University Press, 1983), and Walter Scheidel, "Greco-Roman Sex Ratios and Femicide in Comparative Perspective," *Princeton/Stanford Working Papers in Classics*, 2010, <http://www.princeton.edu/~pswpc/pdfs/scheidel/011003.pdf>.

total of 91 nuclear families, which are more valuable for studying sex ratios. However, like the sex ratio on the Veleia inscription, this one is very high: 131 of the children are sons, and only 34 are daughters, yielding a ratio of 385.

Walter Scheidel, in a very useful but also highly skeptical 2010 paper on the subject of sex ratios, called this data set into question by breaking it down into age brackets. In the inscriptions, some girls are labeled *korē* (maiden), and many boys are labeled either *anēbos* (immature/‘beardless’) or *hēbon* (mature). Using these labels, Scheidel thought he detected a pattern of falling sex ratios as the children matured, indicating to him that younger girls were not being recorded—a frequent objection to attempts at calculating sex ratios from inscriptional evidence. However, while the concealment of females is a major problem which must always be considered in the analysis of data like these, I think that this objection should perhaps be used a little more sparingly than it often is. What Scheidel does not take into account is that there were probably strong motivations at play in this case which would have militated against the failure to record daughters. These inscriptions recorded the grant of citizenship to foreigners who had immigrated to Miletus, and thus served as important evidence of the immigrants’ new status in the community. Should other records have been lost, and should their citizen status ever have come into question, these inscriptions might have proved very important to those whose names they contained, and might also have proved very important to their descendants.

As Pomeroy notes, the inscription includes 18 unmarried adult women not attached to any man—perhaps widows of dead soldiers. Whatever their status, their inclusion probably means that the inscription was being used to record the citizen status of immigrant women and girls, just as much as immigrant men. Pomeroy also notes that in many other Hellenistic

cities, the marriage of citizens to non-citizens was banned, and the children of such unions would be labeled *nothoi* (bastards), as several of the immigrants in the Miletus inscriptions are. Such a law may have been in effect in Miletus as well, Pomeroy speculates, and even if not, the existence of these sorts of laws elsewhere points to the potential danger of leaving citizen status ambiguous. For this reason, and also simply because they were new and unknown in the community, the immigrant fathers would have had a strong motivation to remove any doubt concerning the status of their daughters, and would not have indulged their alleged biases by carelessly overlooking their daughters in the record of the family's enfranchisement. Also, it must be pointed out, as Pomeroy does (though Scheidel ignores her), that there is some ambiguity about the labels used to assign ages to the children. For a girl, status as *korē* probably ended around age fourteen, when she married. For a boy, however, *anēbos* status may not have given way to *hēbon* status until age 20, if Miletus followed Athenian practice, though the transition may have happened anywhere between 14 and 18 if biological puberty were the guide. The *anēbos* category might thus include boys from a wider range of ages than the *korē* category includes for girls, and it is thus likely that the sex ratio in Scheidel's youngest age bracket is inflated, and the sex ratio in the bracket for older children is understated, thus producing a more pronounced appearance of declining ratios.

It seems very clear that the Miletus data show a Hellenistic-era population with a strongly biased sex ratio. Out of the range of different sex ratios which it is possible to derive from the data (depending on who is counted), I think that the children of the 91 nuclear families is the most meaningful subgroup, so I would say that the sex ratio to use is 385. This is a very high ratio, so this group should by no means be taken as a 'normal' population, and

their demographic profile should by no means be generalized to other Hellenistic Greeks, let alone to Romans, but nevertheless the group does serve as a meaningful test case which proves that at least for some populations in the Mediterranean world, very high sex ratios were a reality and not merely a figment of the imaginations of historians misled by exaggerated literature.

Tombstones and other memorials might seem to be another tempting source of data, but they have largely been debunked by Keith Hopkins, who showed in a 1966 paper that the age structure implied by commemorative inscriptions is wildly incompatible with the age structure of modern populations.⁵⁰ He also argued that the sex ratios implied by the inscriptions were hopelessly confused by biases introduced by commemorative habits which tended to exaggerate the number of women who died when they were of childbearing age. Such women were likely to be married to living husbands who could commemorate them, but older women were commemorated much less often, because, having been married at a very young age, their husbands were likely to predecease them. It is worth noting that the sex ratios derived by Hopkins from the large number of inscriptions analyzed by MacDonnell (1913), are more conservative than the ratio from Miletus. The tombstones record 11,924 men and 8,834 women, and if they are broken into age brackets, the ratios range from a low of 104 for

⁵⁰ Keith Hopkins, "On the Probable Age Structure of the Roman Population," *Population Studies* 20, no. 2 (November 1, 1966): 245–64. Determination of age structure based on inscriptions was earlier attempted by: K.J. Beloch, *Die Bevölkerung der griechisch-römischen Welt* (Leipzig: Duncker & Humblot, 1886); W. R. MacDonnell, "On the Expectation of Life in Ancient Rome, and in the Provinces of Hispania and Lusitania, and Africa," *Biometrika* 9, no. 3/4 (October 1, 1913): 366–80; A. R. Burn, "Hic Breve Vivitur. A Study of the Expectation of Life in the Roman Empire," *Past & Present*, no. 4 (November 1, 1953): 2–31.

those aged 15-29, to a high of 173 for those aged 60-99, and thus appear fairly reasonable, but given the many difficulties, I will not attempt their resuscitation as a source in this paper.

Instead, I would like to move on to one final data set concerning sex ratios, this time from Roman Egypt. Roger Bagnall and Bruce Frier (1994) analyzed a very important set of 300 census returns from Middle Egypt, preserved on papyrus and dating mainly to the second and early third centuries CE.⁵¹ These returns provide some of the best data for Roman demography. With regard to the question of sex ratios, however, I find them to be rather strange, suggesting more questions than answers. The overall sex ratio of all 1,022 people in the returns is 112, which the authors correct to 110.4 due to some corrupt data which they omit. There is a very substantial difference between free and slave populations: a sex ratio of 120 for free, and just 50 for slaves.⁵² Another peculiar feature is that the sex ratio is high in metropoleis, 148.3, but quite low in villages, where there are 237 females but only 209 males, for a ratio of 88.2. The difference becomes even more pronounced when Bagnall and Frier look at 50 returns from families where one or both parents are 34 years of age or younger. In this group, those living in the metropoleis have children with a sex ratio of 209 (23 sons and 11 daughters), while those living in the villages have a ratio of just 55 (19 sons and 34 daughters).⁵³ This difference between village and city is quite peculiar. Bagnall and Frier suggest that it may be due to a dual concealment—girls being concealed in metropoleis for the same reason that is normally cited, i.e. no one bothered to acknowledge their existence, and

⁵¹ Roger S. Bagnall and Bruce W. Frier, *The Demography of Roman Egypt* (Cambridge: Cambridge University Press, 1994).

⁵² *Ibid.*, 92-4.

⁵³ *Ibid.*, 151-3.

boys being concealed in the villages when they began to reach taxable age. Frankly, this seems like a stretch to me, but I must admit to being biased against the oft-abused concealment argument after having seen it used to explain away so much data, and especially in this case where it is used in two opposite ways at the same time.

However, Bagnall and Frier are candid in admitting that concealment can not be the whole explanation, so they also suggest that sex-selective infanticide and exposure in the cities may have had a role. This seems somewhat more likely, especially in light of an explanation offered by Naphtali Lewis and cited by Bagnall and Frier,⁵⁴ which suggests that the metropoleis may have been home to a more Greek-influenced population, whereas the Egyptian peasants in the villages may have adhered to more old-fashioned mores. In this vein it is worth noting that literary sources mention that the Egyptians, like the Jews and Germans, had a tradition of raising all their children, rather than exposing them.⁵⁵ It is true that even if this does explain the high sex ratios of the cities, it does not explain the low ratios of the towns, but otherwise I find this a relatively satisfying explanation for an otherwise frustrating data set. It fits with the Miletus data—since both show Greek populations with high sex ratios—and it provides some continuity between the Hellenistic and Roman periods. It is far from the best thing that could be hoped for, but it is not entirely useless either.

In summary, after rejecting two of the four data sets I examined, due to their biases and other flaws, and after accepting the data from Miletus, and provisionally accepting the data from Roman Egypt, I find that the sex ratio data overall are moderately supportive of the

⁵⁴ Ibid., 152, n. 62; Naphtali Lewis, *Life in Egypt under Roman Rule* (Oxford: Clarendon Press, 1983).

⁵⁵ E.g. Strabo, *Geography* 17.824.

idea that sex-selective infant exposure could and did produce elevated sex ratios, at least for localized populations. The Miletus data are by far the most encouraging, and though a sex ratio of 385 is very high, there is no reason to believe that it is impossible for a small population. I argue that Miletus functions as a valuable ‘proof of concept’ which demonstrates that a biased sex ratio could and did exist. By necessity, this shows that a similar situation could also have existed in other places and times, though it by no means proves that it did. The major caveat is that most other populations, if they did have biased sex ratios, would probably be biased to a lesser degree than this atypical population of mercenaries. One of the conclusions I draw from this examination is that sex ratios could be quite variable from one population to the next and would have been governed by local economic and cultural conditions. Despite the tendency toward local peculiarity, none of the data I examined challenge the idea that sex ratios throughout the Roman empire tended to be high. In fact, out of all the data I examined, only the Egyptian villages showed any indication of a non-elevated sex ratio, and rural Egypt is precisely where one might have expected greater equality, on the basis of the literary claims that Egyptians did not practice child exposure. The picture derived from the literary sources, therefore, still stands.

The Christian Demographic Profile

Having examined the population of the Roman empire as a whole, I now turn to the Christian subpopulation, with the aim of determining its sex ratio at the time of the Antonine plague, and with the secondary aim of addressing the question of Christianity’s numerical growth in the centuries prior to 312 CE. The analysis of Christian attitudes to infant exposure must begin with the Bible and the Jewish tradition. The Bible contains many episodes and

laws which are at least obliquely related to the question at hand, though it never makes specific legal pronouncement on infanticide or exposure, or, for that matter, on abortion or other kinds of family limitation. Nevertheless there is sufficient material available that no Jewish or Christian apologist was at a loss for Biblical precedent. Child sacrifice, for instance, could be thought of as related to infanticide, and the near-sacrifice of Isaac by Abraham, prevented at the last moment by the presentation of an animal victim, might be interpreted as a precedent which forbids further child sacrifice. In the rest of the Bible child sacrifice is associated with Canaanite religion and is forbidden. Sacrificing a child to Molech, for instance, is punished by being stoned to death.⁵⁶ Child abandonment features prominently in the story of Moses, but there is a substantial mitigating circumstance, in that the story has Pharaoh condemn all the Hebrew boys to death, and describes his mother as trying to rescue him from this fate. Moses' exposure was thus an action taken *in extremis*.⁵⁷ While the story is certainly an example of child exposure, no Jew or Christian would have interpreted it as an endorsement of child exposure.

Several Jewish sources make explicit mention of infanticide and child exposure. One example is the *Sentences of Pseudo-Phocylides*, a Hellenistic Jewish text, which commands:

A woman should not destroy an unborn babe in the womb,
nor after bearing it should she cast it out as prey for dogs and vultures.⁵⁸

⁵⁶ Leviticus 20:1-4, 18:21; Deuteronomy 12:31, 18:10.

⁵⁷ Exodus 2:1-10.

⁵⁸ *Sentences of Pseudo-Phocylides* 185, translated in Walter T. Wilson, *The Sentences of Pseudo-Phocylides* (Walter de Gruyter, 2005), 187.

This sentiment is elaborated and placed within a context of religious law by Josephus and Philo, both of whom provide important evidence concerning Jewish attitudes in the first century CE. Josephus, in a polemical defense of Judaism, writes that Jewish law forbids abortion and infanticide, and that it commands that all children be raised.⁵⁹ He labels any woman who commits either abortion or infanticide as a murderer. Philo likewise condemns both practices, going into greater legal detail.⁶⁰ He cites Exodus 21:22, a somewhat ambiguous law which I interpret as punishing a man if he strikes a pregnant woman and causes a miscarriage; the man receives the death penalty if he causes death (presumably of the mother, though the wording is unclear), but if he does not cause death he must only pay a fine set by the woman's husband. The legal tradition summarized by Philo, however, diverges from my interpretation: it says that if the fetus had assumed its human shape by the time of the miscarriage, then the man would receive the death penalty, but otherwise he would be fined. This tradition, then, is interpreting the law as talking about the death of the fetus, rather than the death of the mother. It is thus a law which prohibits abortion, at least after the fetus has assumed its shape. Philo interprets this law as applying to infanticide as well, and he singles out child exposure as a form of infanticide, and thus as a violation of the law, and as murder.⁶¹

A number of Christian writers continue this tradition, typically within an apologetic framework. Justin is one of the earliest of these, writing in the mid-second century, but as I showed earlier, his testimony is difficult to interpret because it is complicated by his rhetorical

⁵⁹ Josephus, *Against Apion* ii.202.

⁶⁰ Philo, *Special Laws* iii. 108-109.

⁶¹ *Ibid.*, iii. 110-119.

struggle against those who accused Christians of sexual immorality. Justin seems to imply that children who are exposed, rather than dying, end up in brothels. That Justin cannot be taken seriously here is made clear later in the same text when he writes that Christians do not expose infants for fear that they might die, and make the parents murderers.⁶² In any case, the point is that Justin's view exists on a single continuum with the views of Josephus and Philo: infanticide is murder, and exposure is either infanticide or something worse, so both practices are strictly prohibited. Other Christian writers repeat extremely similar sentiments.

Athenagoras, writing under the reign of Marcus Aurelius, mentions both the use of abortifacient drugs and child exposure, calling both murder.⁶³ Clement of Alexandria, writing in the early third century, mentions exposure in a discourse on the importance of compassion for irrational creatures and for the young of animals. He does not omit to speak of murder, however, which by now has become a constant refrain among all these authors.⁶⁴ The same is likewise true of Tertullian, Origen, and Lactantius, writing in the third century through the early fourth century.⁶⁵ Tertullian's account is particularly interesting because it, like the *First Apology* of Justin, brings up the topic of infant exposure and infanticide as part of a defense against accusations that Christians engaged in shocking rites—in this case, child sacrifice and the drinking of blood. The exact same charge against Christians is also fended off by Marcus Minucius Felix in exactly the same way, i.e. by pointing to the pagans' practice of child exposure to show that it is not the Christians who engaged in shocking behavior, but their

⁶² Justin, *First Apology* 27-29.

⁶³ Athenagoras, *Supplicatio* 35.6.

⁶⁴ Clement, *Stromata* ii. 92-93; also ii. 18.

⁶⁵ Tertullian, *Apology* 7; also *ad Nationes* 16. Origen, *Contra Celsus* viii.55. Lactantius, *Divine Institutes* vi. 20.18ff.

pagan accusers.⁶⁶ The air was thick, apparently, with hurled insults and insinuations, and though a marked polemical character is more obvious in these three cases, in a broader sense all these Christian sources are apologetic and disputatious. Are they less trustworthy, then? I think that the rhetorical context in which these sources are embedded does make them slightly less trustworthy as guides to what Christian behavior actually was, but I think that they do at least reliably reveal the ideals that Christians held out for themselves. As Harris points out, there is no evidence that Christians practiced exposure, nor did Christians ever censure one another of practicing child exposure, which they likely would have done had it been a common practice.⁶⁷ These sources also reveal the legal framework within which the Jews and Christians interpreted infant exposure. By speaking of murder, it is clear that they have completely altered the Roman understanding of the infant—it is now fully a person, who is capable of being murdered and not just killed. They have also attenuated the power of the father over the infant, which under Roman law is practically unlimited.

While the writings of these apologists do not constitute proof that the Christians abstained from infant exposure, they do show that exposure was an area of concern to Christian ethics and was widely, perhaps universally, condemned by Christian leaders. It is reasonable to believe that this concern would have been translated into a change in the reproductive behaviors of the Christian population. If we accept that the early Christians shunned infant exposure, or at least that it was shunned widely enough that it became demographically insignificant, then it seems logical to assume that as a population Christians would have been free of the effects of sex selection, and thus would have had a sex ratio near

⁶⁶ Tertullian, *Apology* 9; also Marcus Minucius Felix, *Octavius* 30.

⁶⁷ Harris (1994), *op. cit.* (n. 24), 17.

105. However, this conclusion ignores the possible distorting effects of conversion. There is some evidence which suggests that early Christianity may have been especially attractive to women, and as a result of an unequal ratio among the converts, there may have been more women than men in the early Christian community.⁶⁸ Such a scenario meshes well with findings in the sociology of religion, which consistently show a higher degree of religiosity among women than among men.⁶⁹ If there were more women than men in the Christian community, there is no reason to suppose that these ‘excess’ women were unmarried. Marriages with pagan men probably would have produced some secondary conversions of the men, but more importantly it can be assumed that the mother’s influence over her children would have ensured that many of the children of such unions would have become Christian. In this way, a low sex ratio could have allowed more rapid growth than would otherwise have been possible.

In spite of these interesting possibilities, I do not want to overly emphasize the idea that Christians had a sex ratio below 105, both because it is quite speculative and because it is unnecessary for my argument. If it is true, then it makes my argument stronger because it increases the degree of separation between the demographic profiles of the Roman population and the Christian sub-population, and would enhance the demographic effect that I am

⁶⁸ Harnack has an excellent review of a wide array of sources which illustrate the large numbers and important roles of women in the early church: Adolf von Harnack, *The Mission and Expansion of Christianity in the First Three Centuries*, trans. James Moffatt, vol. II (London: Williams and Norgate, 1908), 64 ff.

⁶⁹ Alan S. Miller and John P. Hoffmann, “Risk and Religion: An Explanation of Gender Differences in Religiosity,” *Journal for the Scientific Study of Religion* 34, no. 1 (March 1, 1995): 63–75; Alan S. Miller and Rodney Stark, “Gender and Religiousness: Can Socialization Explanations Be Saved?,” *American Journal of Sociology* 107, no. 6 (May 1, 2002): 1399–1423.

seeking to show in my alternative model of the interaction between the plague and the growth of Christianity. However, my argument still works even if the Christian population did not have a low sex ratio, but merely had a neutral ratio of about 105. This more conservative set of starting conditions would produce a somewhat less dramatic effect, but since such conditions are more believable, this is the stronger argumentative posture for me to take. If the Christian population did indeed possess an exceptionally low sex ratio, then that is all the better for my argument, but I do not want to start by assuming such an extraordinary situation.

One final observation on the Christian sex ratio comes from Keith Hopkins, who points out that the Christian sex ratio would have tended toward the norm of the larger society as its own number became a significant fraction of the total population. This is a good point to keep in mind, but it does not effect my argument because even by 312 CE, the latest date that comes into my argument, no one supposes that Christians constituted much more than 10 percent of the population. However, in the fourth and fifth century, Hopkins' posited convergence of sex ratios certainly would have gradually occurred. I would add to his observation, however, that not only would the Christian sex ratio change to resemble the Roman sex ratio as the Christian population grew, but the Roman sex ratio would also change as Christianity itself modified the conditions of Roman religion, law, etc.

This leads me into the question of Christian numbers, which is the topic of Keith Hopkins' paper.⁷⁰ Hopkins argues, provocatively, that the number of Christians in the Roman empire was very small until the end of the second century. He is critical of the traditional, inductive method of counting Christians, practiced most notably by Adolf von Harnack, in

⁷⁰ Keith Hopkins, "Christian Number and Its Implications," *Journal of Early Christian Studies* 6, no. 2 (1998): 185–226.

which scholars “string together snippets of testimony from surviving sources”—sources which, Hopkins contends, often merely spoke a language of “hope, despair, and polemic” rather than of statistics.⁷¹ Some sources seem to indicate a very large Christian presence from early on—Pliny refers to emptied temples and abandoned pagan rites in a letter to Trajan,⁷² while in the mid-second century Tertullian makes it seem as though Christians were already a majority of the population.⁷³ These sorts of sources, Hopkins says, should lead us to call into question the inductive method of counting Christians, as well as the picture of Christian growth that scholars like Harnack derived from them, i.e. a picture in which 7 to 10 percent of the empire is Christian by 312 CE, or approximately 6 million people out of a population of 60 million. Rather, Hopkins claims, Christianity was a rapidly growing but still (in relative terms) very small movement until about the third century.

This is a problematic and arguable view of early Christian growth, because of its tendency to downplay many of the sources, but I discuss it here because I think it is plausible to believe that the early Christians would not have had any way to estimate their own numbers. In this paper I have employed primary sources to show that Romans widely practiced sex-selective infant exposure, and to show that Christians did not, but these are areas in which individual beliefs and experiences are significant in judging the nature and shape of a phenomenon. It is much harder to believe that early Christians could have reliably judged their own numbers, spread out as they were across the Mediterranean, and perhaps in hiding from persecution, or on the move from place to place, etc. The observations and

⁷¹ Ibid., 186.

⁷² Pliny, *Letters* 10.96-97.

⁷³ Tertullian, *Apology* 1, 2, 37.

opinions of ancient writers thus become less significant, and theoretical concerns gain ground. Against Harnack's count of 50 Christian communities listed in the sources in 100 CE, and 100 communities by 180 CE, Hopkins believes that there would have been many more communities, each probably quite small in size. These groups were spread out as a highly dispersed network of small cells which met in houses, and only the few communities which were located in large cities grew to a sufficient size that they possessed organized and professional clergy. This picture fits better, Hopkins argues, with the radicalism of the early movement, with its need to hide from persecution, and with the small physical size of the houses in which the communities would have met. Hopkins further posits—purely hypothetically—that the Christian growth rate might have approximated an exponential curve, albeit with fluctuations from one period to the next. It is this exponential growth curve which provides the basis for Hopkins' argument that Christian numbers would have remained low in absolute terms despite their rapid rate of growth during the first and second centuries, and that only in the third century CE would they have begun to constitute a very noticeable fraction of the Roman population. This conclusion is simply a property of exponential curves—most of their growth, measured in absolute rather than relative terms, comes at the end.

Hopkins readily admits that his picture of Christian growth is not one that he can prove, so I do not want to oversell it. But I do find it intriguing in relation to my model of plague-Christianity interaction because it places such a large part of Christianity's growth in the period of the plagues—the Antonine plague begins in 165 and returns in 189 CE, and several further waves of epidemic disease also strike the Roman world in the third and early fourth centuries, all during a time when Hopkins predicts that Christian growth would be reaching its largest levels in terms of absolute numbers. If his model is correct, then it

strengthens my thesis because it places more of Christianity's growth in the period of the plagues, but if, on the other hand, his model is incorrect, I do not think my argument is harmed. If a smaller portion of Christianity's growth occurred in the period between 165 and 312 CE, this would simply diminish the degree to which plague impacted Christian growth, rather than eliminating the plague as an important factor altogether. In other words, Hopkins' exponential model is not necessary for my thesis, but it is helpful.

Plague and Sex Ratios

I will now attempt to integrate the different strands of my research into a single argument. Since the ultimate question is how the sex ratio will effect the ability of the population to recover in the period after the plague, the proximate question must be how the sex ratio will effect the rate of growth of the population. I begin by conceptualizing the practice of (non-sex-selective) infant exposure as a 'drag' on the birthrate of Roman women. In order to expose a child, a Roman woman must first have a child; it follows that, in order to be able to expose a certain fraction of her children and still reproduce at replacement level, a Roman woman was required to have more children in proportion to the number she had exposed. This means that any given level of infant exposure will produce a drag on the birthrate of that population which must be compensated for. On the level of the entire population, if Roman women failed to compensate for exposure by having more children, then the Roman fertility rate would have fallen, perhaps below replacement level, and the Roman population would have begun to decline. Conceptualized in this (rather callous) way, the practice of infant exposure appears as a sort of inefficiency—a waste of time and reproductive effort which must be compensated for by having more children.

This is further complicated if the infanticide is sex-selective. By selectively exposing girls, the population is essentially removing mothers from the next generation. The drag effect is thus compounded, because mothers, far more than fathers, are the parents that limit and thus determine the rate of reproduction. The fewer women there are, the higher each woman's reproductive rate needs to be in order to replace the population. Exposing girls therefore ensures that the girls who are not exposed, and who grow up to become mothers in the next generation, each need to work harder to match the level of reproduction that both she and her missing sisters would have been responsible for had there been no exposure. All of this is simply to say that Christian women were at a significant advantage. Even in the peace and relative health of the first two centuries CE, a given Christian woman could either have had a lower gross level of reproduction than a given Roman woman and still have matched her net level of reproduction, or the Christian woman could have matched the Roman woman's gross level of reproduction and have ended up with a higher net level of reproduction. Christian social structure was thus configured to grow relatively easily compared to the Roman social structure. And this is on a purely demographic level of analysis; if one also considers that a tightly bonded Christian community may have provided a far better social safety net than was available to the average Roman, and may thus have alleviated some of the demographically limiting effects of poverty, etc., then the fertility differential could have been even greater. One further consideration is that of conversion: though it has not been a central focus of this paper, it must be remembered that Christians were supplementing their natural rate of increase with religious conversion during this entire period. It is likely that many converts were relatively young, meaning that the effect of conversion would have been to skew the Christian age distribution toward the younger end. As a result of this more favorable age structure,

Christians would have been able to sustain a higher level of reproduction than their total population figure would suggest, and this would have compounded with the effects of the more favorable sex structure of the Christian population to provide a substantial reproductive advantage. I therefore consider it plausible that Christians were reproducing more prolifically than other Romans even in normal times, when there was no plague, and were probably gaining on the Roman population due to natural increase in the period before the plague.

When the plague struck, however, this process was accelerated. The plague compressed more of these Christian gains relative to other Romans into a shorter space of time. To understand why this is the case, it is necessary to look at two recent papers on the plagues. In the most recent work on the Antonine plague, a growing consensus has emerged that to understand the demographic effect of the plague it is not sufficient merely to ask how many were killed, but to think about fertility and population growth in the years following the plague. In 2006, Richard R. Paine and Glenn R. Storey created a model of the impact of the Antonine plague on the Roman population in which they focused on the post-plague growth rate in combination with plague mortality, and they concluded that the plague's effect would have been quite significant and prolonged. They found that the first wave of the plague, corresponding to the 165 CE event, despite killing about 30 percent of the population, would merely "perturb" the rate of population growth, but would not entirely halt it. In other words, the size of the population shrank quickly when it was struck by the plague, but the rate at which it then proceeded to grow again was close to the rate at which it had previously been growing. The population thus could have been expected to recover to its former levels over time, had not the second wave, of 189, pushed the population growth rate into the negative. According to the model, forty years of negative growth followed, in which the population

continued to decline. This is because the second wave, though killing fewer people (thanks to immunity built up during the first wave), would disproportionately rob the population of people of reproductive age, who were born shortly after the first plague, with no immunity, and who were just reaching their peak reproductive years as the second plague hit. As a result, the Roman population would not have been able, according to Paine and Storey's model, to reproduce fast enough to rise back to its former level. However, taking migration into account, Paine and Storey believe that the city of Rome itself would have been brought back to pre-plague population levels relatively quickly; the long-lasting decline would have manifested mostly in the wider empire.⁷⁴

In a 2012 essay for *L'impatto Della "peste Antonina"*, Yan Zelener also discusses the importance of the fertility response in determining the plague's ultimate impact on the population. Zelener thinks that the "self-reinforcing decline in [smallpox] mortality" due to the disease's dependence on density in order to spread, would have resulted in less than the predicted 25 percent mortality in the first generation, but that further population decline would depend on the fertility response. Zelener argues that because the Roman empire had too few centers of population dense enough to support endemic smallpox, subsequent smallpox infection would have been epidemic, and thus the fertility response would not have been as great, for the same reason as in Paine and Storey's model. As a result, the eventual population

⁷⁴ Paine, Richard R. and Glenn R. Storey, "Epidemics, Age-at-Death, and Mortality in Ancient Rome," in *Urbanism in the Preindustrial World: Cross-Cultural Approaches*, ed. Glenn R. Storey, (Tuscaloosa: University of Alabama Press, 2006), 80. Paine, Richard R. and Glenn R. Storey, "The Alps as a Barrier to Epidemic Disease During the Republican Period: Implications for the Dynamic of Disease in Rome," in *L'impatto della "peste antonina"*, ed. Elio Lo Cascio, (Bari: Edipuglia, 2012), 188.

decline would have exceeded 25 percent, according to Zelener.⁷⁵ Both models, then, predict severe mortality and an inability of the Roman population to rebound in the aftermath of the 189 CE plague. If Paine and Storey's model is accurate, the 165 CE population level would not have been achieved again until 90 years later, assuming a favorable growth environment—which of course was not the case, since the Roman empire was just then descending into the military anarchy of the third century.

This demographic environment was one in which pagan Romans were vulnerable, and one in which the Christian population was pre-configured to be able to take advantage of that vulnerability. Because infant exposure was acting as a drag on Roman birthrates, and because sex selection had produced a sex ratio imbalance which compounded that drag effect by removing potential mothers from the population, Roman women were already being taxed with a higher reproductive burden prior to the development of negative growth following the 189 CE plague. In order to understand this situation I call upon the metaphor of elasticity: the Roman population was somewhat 'inelastic' in the sense that it was already 'stretched' by child exposure and sex selection, so that any increase in fertility would have come at the cost of an even higher reproductive burden on Roman women. The Christian population, however, with its more favorable age and sex structure, possessed greater elasticity and was thus better able to absorb the shocks of the mortality crises and was more easily able to increase its rate of reproduction in the years after 189 CE in order to compensate for the disproportionate mortality of reproductive-age individuals in that wave of plague. The decline in Christian

⁷⁵ Zelener, Yan, "Genetic Evidence, Density Dependence and Epidemiological Models of the 'Antonine Plague'," in *L'impatto della "peste antonina"*, ed. Elio Lo Cascio, (Bari: Edipuglia, 2012), 175-6.

population would not have been as steep, nor would it have lasted as long. Factoring in ongoing conversion at the same time, and Christians may not have been in negative growth for very long. Conversion, in a sense, can be compared with immigration; as Paine and Storey highlighted in their article, the city of Rome itself would not have suffered negative growth for very long, because it enjoyed a high level of immigration, and a similar situation would have prevailed in the Christian population. As a result of the negative growth of the overall Roman population, and as a result of the ability of the Christian population to more quickly and easily compensate for the assaults of the plague, the Christian population would have accelerated its growth relative to the Roman population during this period.

Conclusions

What I have presented here is obviously not a proof, and is not intended to be. It has been an exploration, and the result is an alternative model of plague-Christianity interaction in the second and third centuries CE, which I believe substantially improves upon the models suggested by McNeill and Stark. My conclusion is still merely an hypothesis—an interesting one, I hope—which I believe is plausible on the basis of the ancient testimony I have analyzed in the course of my argument. I believe I have shown that as a result of its practice of infant exposure, and particularly as a result of the exposure of infant girls, the Roman population suffered from an imbalanced sex ratio which, though sustainable during times of peace and health, proved unsustainable in a time of crisis mortality. In this respect, at least, the Roman social system was not as well adapted for harsh times, nor was it as prepared to rebound from demographic catastrophe. Christians, however, possessed a set of social norms which proved more durable in the new demographic reality—a set of norms which provided a demographic

‘elasticity’ which enabled a more robust fertility response after the plague. This demographic edge, even if rather slight, when compounded over several generations during the period of accelerating change in the post-plague period, resulted in substantial Christian growth relative to the larger Roman population within which it was embedded. It is perhaps ironic that the Romans, who won so many of their earlier victories on the basis of superior manpower, in the end were overcome by Christianity in what can only be described as a war of attrition.

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