My model of demographic contraction will have to be tested against further pieces of empirical evidence. For the time being, the preponderance of the documentary sources are consistent with the notion that for several generations following the Antonine plague, Roman Egypt (or parts thereof) experienced limited growth in *per capita* output facilitated by significant population loss.[94]

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Acknowledgements
I would like to thank Richard Saller, Peter Temin, Peter van Minnen and Yan Zelener for their comments on an earlier draft of this paper. I have greatly benefited also from detailed critiques by three anonymous referees.

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matic evidence and falling prices in the fourteenth century," *Economic History Review* 27 (1974) 1-15; Farmer (supra n.38) 441-42. Credit could not compensate for the lack of cash because its availability was tied to that of bullion: P. Nightingale, "Monetary contraction and mercantile credit: later medieval England," *Economic History Review* 43 (1990) 560-75.

94 *Postscript*: In a forthcoming paper, J. Greenberg challenges Duncan-Jones' contention that numerous samples of documentary evidence reflect the impact of the Antonine plague. To my mind, his critique further underscores the need for comprehensive deductive models that reduce the likelihood of *ad hoc* reasoning.

The effects of plague: model and evidence

Roger S. Bagnall

In the foregoing article, W. Scheidel builds on earlier work, most notably that of R. P. Duncan-Jones in *JRA* 9, to offer a model for the predicted effects of the Antonine plague and to argue that the model fits the evidence from Roman Egypt reasonably well within the limits of the quantity and quality of the latter. In his second footnote, he encourages critical response, suggesting that it "may either corroborate or undermine my interpretation." The following pages are intended as a contribution to that discussion, but with lesser ambitions than either corroborating or undermining the model as a whole. They offer some of both, in fact, but more in the direction of undermining it.

There are three reasons for not claiming too much at this point and not offering any general conclusion (as I do not). The first is that I do not have any fixed views on the degree to which the plague was the prime mover behind the changes visible in late 2nd- and 3rd-c. Egypt.[1] In the absence of any concerted attempt to formulate and test other hypotheses about the engines of social and economic change, it is hard to say if the degree of fit of evidence to model is impressive or not. The most obvious counter-candidate is the increased municipalization of Egypt during just this period, especially from A.D. 200 onward. It would be useful to generate a model of economic change from this force and see if it is equally capable of accounting for the evidence.

A second reason for circumspection is that it would take much more time than has been at my disposition to look at all of the evidence with the critical care it deserves. Scheidel's data come almost entirely from H.-J. Drexhage,[2] a work of considerable utility but not executed with a high level of critical care (as has been noted by D. Rathbone); nor has Scheidel himself done much to look critically at the character of the evidence and how far that might require more caution in its use. Some soundings of this sort appear in the following remarks, but it must be emphasized that they are only the beginning of an investigation.

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1 This may come as a surprise to the reader who has remarked Scheidel's polemical engagement with my remarks on *P.Oxy.* 4527 (above, pp. 112-13). I shall return to this text later, but for the moment I may simply note that it was scepticism — not yet laid to rest — about the quality of the evidence for the effects of the plague, not adherence to another hypothesis, that lay behind my earlier comments.

2 H.-J. Drexhage, *Preise, Mieten/Pachten, Kosten und Löhne im Römischen Ägypten bis zum Regierungsantritt Diokletians* (St. Katharinen 1991).

Finally, there are, in my view, some problems with the model itself that cannot be dealt with adequately under present time constraints and that are at least as significant as the difficulties presented by the evidence. An important one is that one of the model's underlying assumptions is strong doubt about the possibility of real *per-capita* economic growth in a society subject to natural fertility, although Scheidel does not quite deny it altogether. This is, of course, a central question in ancient economic history. My view of it is quite different from Scheidel's, but the point would require its own article (or book).

Perhaps even more troublesome to my mind is the sense that Scheidel's model of the effects of the plague is so powerful that it is capable of explaining any evidence at all. This point comes most saliently to the fore on p. 112, where we find a 'heads-I-win, tails-you-lose' situation. No matter what the interpretation of *P.Oxy.* 4527, Scheidel says, it will be consistent with his model, in part because one may allow an indefinably elastic lapse of time before some of the effects of the decline in population have their full impact. This may be true, but if it is, then almost any phenomenon observable over a half-century or more after the plague may be attributed to it without fear of disproof. The model then becomes incapable of challenge from the evidence, and ceases to be a hypothesis capable of falsification. This may be cheering to its proponents, but it tends to discourage the kind of testing that Scheidel has encouraged. Even less does it become possible for one to test alternative hypotheses or the possibility that the loss of population from the plague is one of several major developments that interactively generate the 3rd-c. Egyptian economy (which *a priori* seems to me the most likely reality).

Single-cause explanations of historical change, particularly deterministic ones, are inherently suspect. Proponents of the importance of the plague, no doubt because of earlier scepticism about the very fact of the plague in Egypt,[3] have tended to write as if it occurred in a vacuum. Characteristically, Scheidel tries (p. 110) to attribute all of the political, social, and economic change of late 2nd-c. Egypt to the plague. This is not a correct view, if only because we can trace some of these developments long before the 160s, but this subject too is too large to argue here. It is, however, worth calling attention to the fact that even so specifically demographic a matter as the depopulation of Delta villages is not solely the product of the plague, either directly or indirectly; it had begun before the reign of Marcus Aurelius, as N. Lewis pointed out.[4]

What follows, then, is a series of reflections on particular elements of the evidence, without an attempt to use them for any overall test of Scheidel's model, or of any other.

Wheat taxes

P. van Minnen has taken issue[5] with the interpretation of *P.Oxy.* LXVI 4527 that I offered in this journal (*JRA* 13 [2000] 288-92). I suggested that this papyrus pointed to a level of wheat tax collection in the Herakleides division of the Arsinoite nome (the Fayyum) in 184/5 that was much the same as it had been at the pre-plague peak. Van Minnen, by contrast, argues that restoring line 15 of the papyrus to refer to the entire period from Pachon through Mesore (roughly May through August), instead of to Mesore itself, would suggest a drastic shortfall in tax receipts, leading to the view that "the agricultural economy of Egypt was in deep trouble at the time". Scheidel naturally prefers this outcome, although he carefully points out that his model can accommodate either van Minnen's view or mine.

Unless the missing left side of the papyrus comes to light, it is not possible to disprove an exercise of this sort in prose composition; but, equally, it is not evidence. We do in fact have

3 It is perhaps worth repeating that for all my scepticism about claims to observe the effects of the plague, I have no doubt that the plague actually affected Egypt.

4 N. Lewis, "A reversal of a tax policy in Roman Egypt," *GRBS* 34 (1993) 111-12 (repr. in his *On government and law in Roman Egypt* [AmStudPap 33, 1995] 367-68).

5 P. Van Minnen, "P.Oxy. LXVI 4527 and the Antonine plague in the Fayyum," *ZPE* 135 (2001) 175-77; he is cited by Scheidel (n.92 above) with a "cf." and no indication that he disagrees with Scheidel on this point.

good evidence that the Roman government reacted fairly rapidly in the wake of the depopulation of Delta villages in the 160s to reduce local tax assessments, bringing them in line with contemporary realities.[6] Even the assessment, then, would suggest that the government still thought there was a reasonable likelihood of collecting an amount equal to the peak yield of the area before 160. Whether they succeeded in 185 in doing so (and many causes could have interfered with their success) we do not have clear evidence to show, but behavior in the Delta makes it likely that the target was in general realistic. If so, my point remains.

Money

Scheidel has now declared that "Rathbone's suggestion that the gradual introduction of a debased tetradrachm, together with other monetary adjustments, 'helped to fossilise these price rises into the new higher but stable bands of prices which lasted to around 274', offers what appears to be the only feasible solution".[7] In a note, he remarks (against Rathbone) that "it may not be true that coin issues following the debasement of 176/77 were small ... Even so, it seems doubtful whether coin debasement in the late 170s could have resulted in full-scale inflationary price-adjustment by the 190s." No grounds for the doubt are offered. The monetary history of the 4th c. shows clearly that nearly instantaneous responses in price levels to monetary change were not only possible but regular. Perhaps the most striking instance comes in the enormous leap in prices between 351 and 353, with lasting effect, but it is possible to trace the history of debasement of the coinage throughout the first half of the century and see price responses within a year or so, sometimes within months — as fine a resolution as our documentation allows us to identify.[8]

Neither Rathbone nor Scheidel has yet offered any evidence that debasement was not the operative mechanism. Van Minnen[9] has opted in favor of debasement driven by government budgetary requirements:

> In a way, then, the Antonine plague caused not just a rise in wages, as expected, but also a rise in prices, but in both cases, I think, indirectly, through the medium of the debasement of the currency. Whether wages rose even more than prices is another matter.

Even if debasement was the means, van Minnen is right to point out that it remains of compelling interest to see how far the prices of different items took similar or divergent courses in the 170s and later. I shall turn now to look more closely at a few of the critical areas.

Land prices

Scheidel's model predicts that "the value of land and of its products falls, as do real rents." His table of agricultural land prices shows a much greater rise in the price of expensive land (over 600 dr. per aroura in price) than of inexpensive land, with the median rising from index 100 to 151 (median) or 175 (mean) for the higher-quality land, compared to rising to 118 (mean) or 117 (median) for the lower-quality land. This categorization is presented as a simplification of the analysis presented by R. Alston.[10] It is, however, just as arbitrary a categorization as Alston's. First, it is worthwhile using the year 170 as the dividing line (rather than dividing by centuries), given the use of the plague as the driving force; this, after all, is what Scheidel himself does (p. 103) in discussing wheat prices, and consistency of practice is helpful for the sake of comparability of results.

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6 See Lewis (supra n.4) for a convincing demonstration.

7 D. W. Rathbone, "Prices and price formation in Roman Egypt," in J. Andreau *et al.* (edd.), *Économie antique: prix et formation des prix dans les économies antiques* (Entretiens d'archéologie et d'histoire, Saint-Bertrand-de-Comminges 1997) 216.

8 See my *Currency and inflation in fourth-century Egypt* (BASP Suppl. 5, 1985) 43-44, for the dramatic shift of A.D. 351-353. An update to the price tables in that monograph appears in *P.Kell.* IV, 225-29.

9 Van Minnen (supra n.5) 176-77.

10 R. Alston, *Soldier and society in Roman Egypt: a social history* (London 1995) 108, Table 6.2. Cf. my criticisms in *JRA* 10 (1997) 509 n.12.

Second, one must separate out the prices that are for unproductive land being sold off by the government, which are generally an order of magnitude below the prices of productive land. There are 14 such prices in all, 6 before 170 and 8 after.[11] Third, the only other objective discriminant is the differentiation between arable land and garden land (fruit orchards and vineyards).[12] There are a few cases in which the exact character of the land is uncertain because of damage to the papyrus, but they do not affect the outcomes very much. Scheidel, like others, notes the great difficulty of comparing land prices. For this reason, while giving mean and median figures like Scheidel, I have included two scatter plots, from which the reader may get a fair notion of the spread of the data-points.

The small number of cases of garden land look as follows:

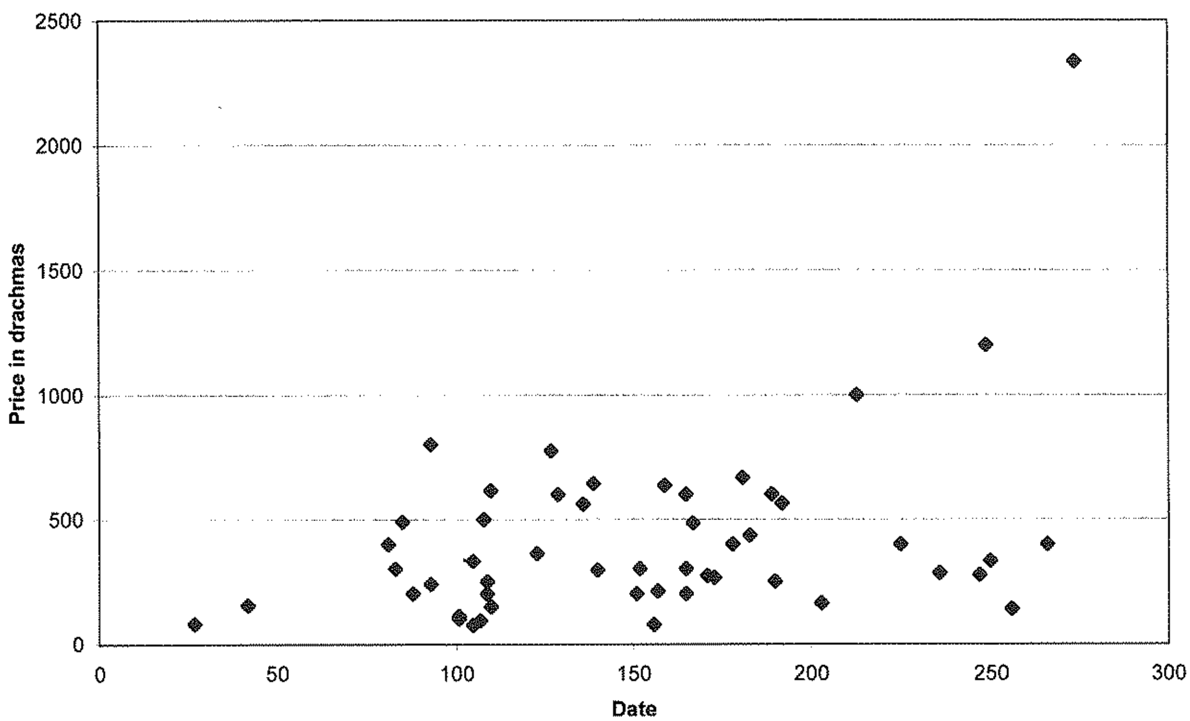
Period	*Refs.*	*Mean*	*Index*	*Median*	*Index*
Up to 170	4	686	100	572[13]	100
After 170	3	800	117	800	140

The number of cases is too small for one to place too much confidence in these figures, but they suggest that, in general, garden land did appreciate in nominal terms after 170, even if less than the rises in wages and in the prices of produce.

For the arable land, the situation looks as follows:

Period	*Refs.*	*Mean*	*Index*	*Median*	*Index*
Up to 170	33	343	100	300	100
After 170	19	548	160	400	133

PRICES OF ARABLE LAND



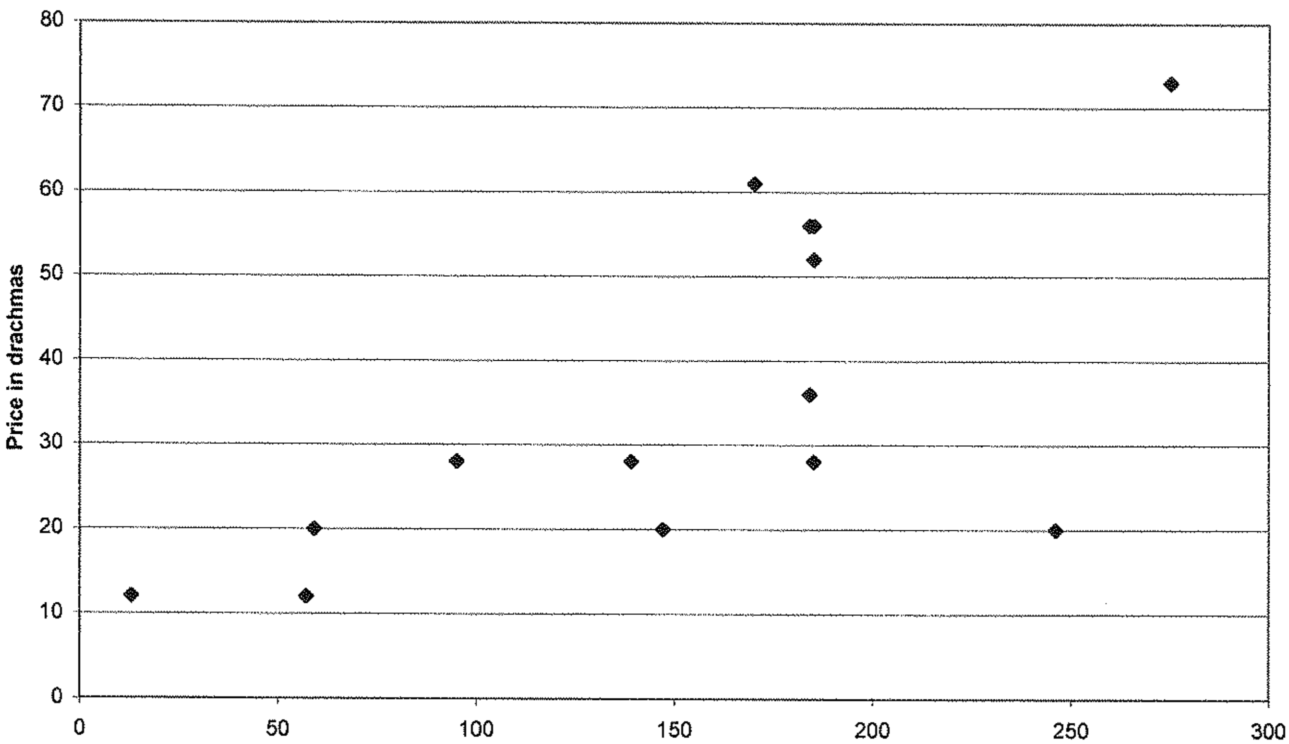
11 I have used the same table in Drexhage (141-54) that Scheidel used, but with additional data for the Oxyrhynchite from the table in J. L. Rowlandson, *Landowners and tenants in Roman Egypt. The social relations of agriculture in the Oxyrhynchite nome* (Oxford 1996) 320. In the discussion below, I have omitted texts that can be dated only to a century, or where the price per aroura is highly uncertain.

12 Using 600 drachmas as the break-point, apart from other difficulties, fails to account for the fact that this sum was worth less in the second period than in the first.

13 Midpoint between the third and fourth cases, 467 and 676.

The post-170 data for arable land include some problems, however. These cases fall at the upper end of the spectrum and represent land that is not ordinary grain land but has (in the case of *P.Oxy.* XLIX 3498) considerable capital installations for irrigation, or a valuable reed bed (*P.Oxy.* XIV 1636). The third highest per-aroura figure belongs to *P.Amh.* II 96, again a case of artificial irrigation. If the top two or three are left out of account, the mean drops considerably. The median, however, remains unchanged. That fact reflects the one unmistakable change in the period after 170, which is the greater dispersion of land values — something the scatter plot shows unmistakably. Indeed, one gets the impression that ordinary land drifted down in value over time, with only those farms in which substantial investments had been made increasing in value.

PRICES OF UNPRODUCTIVE LAND



Finally, the sales of unproductive land by the government are interesting. It is curious that this is the only category of land in which the prices — presumably set by the government itself — drift upward with some consistency. This phenomenon may point to official recognition that a very different level of prices now prevailed in the economy as a whole.

A couple of other points are worth observing: 1. Even looking at the medians, and taking the limited amount of data for garden land into account, there is no sign that garden land has risen in value in comparison to grain land; 2. The price series for land, however difficult to interpret, is not subject to the breaks in the period 160-190 that several other parts of our data are.

Taken together, the results of this analysis seem to me compatible with Scheidel's model, particularly if one complicates it with cross-winds from shifting patterns of élite investment driven by changes in patterns of residence and demands on resources for participation in the municipal life of the 3rd c.

Rents

The volume of data here, from land leases, is relatively large. Scheidel has stratified it by provenance and period (1st c., 2nd c. to 165, 3rd c.). The figures have an appearance of solidity.

But it is not clear that this appearance is to be taken at face value. It is often hard to compute the rents in artabas of wheat per aroura, because the leases are in many cases complex, with crop rotation specified and part of the rent paid in cash or in another commodity. For this reason, one must take a careful look at the apparently dramatic rise in rents in the 2nd c. down to 165. For example, Scheidel sees the following development for the Oxyrhynchite:

Years	*References*	*Mean*
44-99	8	5.41
103-165	13	7.82
205-262	14	5.89

Clearly we are dealing with a substantial rise in the rental value of land during the 2nd c. compared to what it was worth in either the preceding or the following period. Or are we? Unfortunately, Scheidel's figures come straight from Drexhage's table with no critical examination.[14] Let us look at a sample case, *P.Oxy.* XXXVIII 2874, from A.D. 108. In this lease, 24 arouras are leased for a period of 4 years, with wheat rents of 139 artabas in the first year and 134 artabas in the succeeding years, plus half of the straw (ignored in both Scheidel's reckoning and in mine for the sake of simplicity). Drexhage computes a rent in year 1 of 11.58 artabas per aroura. Now it is true that this is the *rent per aroura of land planted in wheat,* because half of the land is specified to be planted in wheat and half in arakos clover each year. But in that case one is setting the rent on the other half of the land at zero: the true rent is arrived at by dividing the total rent by the total arouras, and it is half of this.

When all 13 rents are re-computed to determine the average total rent per aroura of the wheat land under lease over the term of the lease (converting cash and other crops at their value relative to wheat), the average for this period comes out, on my reckoning, to 5.51 artabas per aroura, not 7.82.[15] The entirety of Scheidel's Table 1 must be re-computed on sound principles; no conclusions drawn from the table as it stands have any value.

Wages and commodity prices

Scheidel's model predicts a rise in real wages in the aftermath of the plague, that is, a rise in wages faster than that in the prices of agricultural products. His tables purport to show such a development, with the index of rural wages (2nd c. =100) rising to (depending on region) between 242 and 292, while commodity prices rise only to 178 (olive oil), 180 (wine), or 219 (wheat), in all cases using the means. There are, however, good reasons to be mistrustful of any conclusions drawn from a supposed spread between incomes and prices, and Scheidel recognizes the difficulties in the enterprise. For one thing, the choice of period to use as the basis for comparison to figures after 190 is critical. Rathbone chose[16] to compare longer spans of pre-170 prices than Scheidel has done, gaining data and thus likelihood of solidity but sacrificing any possibility of detecting intermediate developments. Using his indexes, one would find wheat advancing to 223 and wine to 238, but wages (here comparing mid-2nd to mid-3rd c.) advancing only to 188.[17] This would demonstrate a pattern the reverse of what Scheidel thought he could identify.

14 It is regrettable that Scheidel used the table in Rowlandson (supra n.7) 332-49 only to supplement Drexhage, whereas recomputing the rents on the basis of Rowlandson's list might have yielded sounder results.

15 The figure could be slightly higher or lower with different assumptions about the treatment of the non-wheat payments, but the effect would be trivial.

16 D. W. Rathbone, "Monetisation, not price-inflation, in third-century Egypt?" in C. E. King and D. G. Wigg (edd.), *Coin finds and coin use in the Roman world* (Berlin 1996) 321-39.

17 The last is calculated by averaging his span of 1 dr. to 1 dr. 3 ob. for rural wages of the mid-2nd c., for a mean of 8.5 obols (using the 7-obol drachma).

Time-span is not the only difficulty here. Much more intractable is the fact that a large proportion of the figures for commodities and wages come from a relatively small number of accounts originating in large estates, above all the Appianus estate documented in the Heroninus archive (comprising 71% of the Middle Egyptian data for the 3rd c.); and, as Scheidel notes, there are no Upper Egyptian data for the 3rd c. to allow comparison. One remark will give an idea of the slenderness of the foundations. If one excludes from the data both the Heroninus archive and one Memphite(?) account with extremely high figures,[18] there remain only 25 observations for the 3rd c., and they yield a daily average of 14.1 obols, or an index of 199 against Scheidel's 2nd-c. base for Middle Egypt. That is less than the index advance for wheat, by far the single most important expense in a family's budget. In other words, we would have a drop in real wages. I would not wish to claim that there was in reality such a drop for the average Egyptian; I make the point only to indicate the feebleness of the basis on which the real wages are claimed to have risen. Much more could be said in a similar vein about the 2nd-c. evidence, also highly archival (the accounts of the archive of the descendants of Patron ["archive of Laches"] dominate Drexhage's listings) and susceptible of distortion. Whether there is any way of overcoming these deficiencies in the evidence is questionable. Perhaps separating out large estates from the remainder would help, but it is questionable that there are enough figures *not* from such estates to give any insight into broader pay practices. It seems to me that one must look with a more critical eye on the wage and price evidence before claiming that "what really matters is that each of these measurements confirms the same basic trend: real wages for agricultural labour invariably appear higher in the 3rd c. than in the 2nd". It is not merely a question of the scale of this trend — it is its very existence that is highly dubious; and because this is surely the most important point on which Scheidel claims confirmation for his model from the papyri, it should make one uneasy about large claims for it.

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18 This is *BGU* I 14. It is by no means clear that the figures there are daily wages.