10 *Rome, Taxes, Rents and Trade*[†]

KEITH HOPKINS

In 1980, Keith Hopkins presented a general model of the interdependence of movements of taxes, traded goods and money in the Roman empire.[‡] This bold attempt to construct a comprehensive explanatory framework for the expansion of market exchange in the wake of Roman conquest attracted considerable attention as well as criticism. One and a half decades later, Hopkins restated and refined his position in an article that took full account of the debate triggered by his original argument. Published in an academic journal of limited circulation, this important contribution is now being made more readily accessible. Hopkins, who held Moses Finley's chair at Cambridge, stands out among ancient historians for his professional background and research in both classics and sociology. As a social scientist, he has consistently preferred deductivist arguments and comparativist analogy over positivist and inductivist readings of ancient source material. After successfully redefining the terms of the debate in Roman population studies, Hopkins soon turned to the study of the Roman economy, devising sweeping models of urbanisation, the spread of chattel slavery, and the expansion of trade that have left their mark on the field.[§]

Here and elsewhere, he takes a mildly developmental approach, allowing for a measure of real economic growth in response to the increasingly efficient coercion of surplus from subject populations. Moving beyond the empirical analysis of ancient source material and generalisations from particular texts and artifacts, Hopkins seeks to identify the principal forces driving economic development and commercial exchange in the Roman empire, and to explore the logical implications of their interactions. Less constrained by disparate and often inadequate sources and the absence

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[‡] 'Taxes and Trade in the Roman Empire, 200 BC–AD 400', *Journal of Roman Studies* LXX (1980), 101–25.

[§] 'Economic Growth and Towns in Classical Antiquity', in P. Abrams and E. A. Wrigley (eds), *Towns in Societies: Essays in Economic History and Historical Sociology*, Cambridge: Cambridge University Press, 1978, pp. 35–77; *Conquerors and Slaves: Sociological Studies in Roman History, 1*, Cambridge: Cambridge University Press, 1978; 'Models, Ships and Staples', in P. Garnsey and C. R. Whittaker (eds), *Trade and Famine in Classical Antiquity*, Cambridge: Cambridge Philological Society, 1983, pp. 84–109.

of ancient statistics than more traditional modes of enquiry, this approach sheds light on underlying mechanisms that would otherwise remain obscure. This strength, however, has also been considered a weakness by scholars who have emphasised apparent discrepancies between predictions generated by the model (such as the reciprocity of tax and trade flows) and samples of empirical evidence, above all coinage. Richard Duncan-Jones in particular, in a series of studies of the movement of coins and goods, has repeatedly challenged crucial elements of Hopkins's argument.[¶] In this paper, Hopkins responds to these criticisms, discusses methodological issues, and defends a revised version of his model. The only comprehensive attempt to explain the dynamics of the Roman imperial economy currently available, this restatement will remain the starting point for further research for the foreseeable future.

INTRODUCTION

The purpose of this article is to review some working-methods in Roman economic history, to make some propositions, to analyse their implications, and to understand the criticisms levelled against these methods and conclusions. In some sense, the article is unashamedly apologetic, but it is also, I hope, revisionist and self-critical. Inevitably, I made errors in my previous work, and it seems only sensible to acknowledge some of them. And yet in the end I still want to reassert the broad utility of models in ancient economic history, and the basic validity of the specific model of the Roman economy and of the main conclusions, which I put forward nearly twenty years ago.[1] In sum, I want to go over some old arguments, assess criticisms, confess some faults, and break new ground.

Some of the greatest fun I have had in Roman history has been in constructing models. A model is, roughly speaking, a simplification of a complex reality, designed to show up the logical relationships between its constituent parts. The utility of models in Roman history, as I see it, is twofold. First, models allow us to perceive the structures or repeated patterns which lie behind the superficial flow of indi-

[¶] R. Duncan-Jones, *Structure and Scale in the Roman Economy*, Cambridge: Cambridge University Press, 1990, Ch. 2; *Money and Government in the Roman Empire*, Cambridge: Cambridge University Press, 1994.

[1] 'K. Hopkins, Taxes and Trade in the Roman Empire, *JRS* 70 (1980) 101ff. Two synoptic reviews of the current state in the field are intelligently incisive: William Harris, 'Between Archaic and Modern', in W. V. Harris ed., *The Inscribed Economy* (Ann Arbor, 1993) 11ff and E. Lo Cascio, 'Forme dell'economia imperiale', in A. Schiavone ed., *Storia di Roma* (Torino, 1991) 313ff especially 351ff. The book of Hans-Ulrich von Freyberg, *Kapitalverkehr im römischen Kaiserreich* (Freiburg im Bresgau, 1989), represents a real advance in sophistication, and is now essential reading: see the discussion by J. Andreau, *L'Italie d'Auguste à Dioclétian* (Rome, 1994) 175ff.

vidual actors and events, which fill the pages of traditional Roman narrative histories. Secondly, models allow us to construct whole pictures, into which the surviving fragments of ancient source material can be plausibly fitted. The model is a sort of master picture, as on the front of a jigsaw puzzle box; the fragments of surviving ancient sources provide only a few of the jigsaw pieces.

All this may still seem heresy to some. The dominant orthodoxy among traditional ancient historians is still induction; they move from evidence to conclusion, or at the very least that is how they prefer to present their arguments. For some Roman historians, even that is going too far; for a significant conservative minority, the primary focus is on the acquisition and display of knowledge, used often enough as a defence against arguing propositionally. For such people, speculation is a term of abuse, and they sometimes affect to sneer at models as though they are frivolous and irresponsible.[2]

Actually, there is ample room for both compromise and overlap between model-builders and inductivists; their positions are more complementary than opposed. After all, deduction like induction is partly a rhetorical pose. The model-builder, if he/she is going to have any chance of success, has to know much of what the sources tell us. The pure inductivist, if he/she is going to be understood outside a narrow range of committed specialists, has to think through what the implications are of his/her, detailed arguments. Quite a few conservative historians nowadays have a nodding, if uneasy, acquaintance with deductive arguments, model-building, and comparative evidence.[3] The gap between the various schools is not as wide as it once was. That said, I still think that the style and shape of my arguments on Roman economic history are distinctively different from the norm; and so, their assumptive frame is not always understood, and

[2] A recent and amusing case, *exempli causa*, is in the excitingly innovative book by Roger S. Bagnall, *Egypt in Late Antiquity* (Princeton, 1993). It is self-consciously inductivist, and presents a huge amount of well-documented detail. The care put into the detail is then used to legitimate the generalising inferences, which are often disproportionate to the evidentiary base. Per contra, Bagnall explicitly denies the utility of models (p. 310), and then proceeds to present a rather good simplifying model of the Egyptian agricultural economy.
[3] Harry Pleket, who would I think call himself a conservative historian, provides a wealth of comparative data in his long and interesting discussion of Roman economic history: 'Wirtschaft', in F. Vittinghoff ed., *Handbuch der Europäischen Wirtschafts- und Sozialgeschichte* (Stuttgart, 1990) vol. 1, 25–160. But his comparisons illustrate more often the possible than the probable. I agree with him completely that the agriculture of the Roman empire was a patchwork of varied agricultural practices and productivity, and that the area under cultivation and total product probably grew in the first two centuries AD. But his broad conclusions (pp. 78–9) on average agricultural productivity (yields of 8–10 times seed for Italy) seem much too optimistic, if only because he fails to fit them into a matrix of cognate probabilities (e.g. population and demand). See below.

their utility and its limits are not always appreciated. This is a slightly shame-faced, perhaps also two-faced apologia.

A MODEL OF THE ROMAN ECONOMY: FIVE PRINCIPLES OF CONSTRUCTION

Five interconnected principles form the frame for the model of the Roman economy constructed here. The first principle is that we are dealing here primarily with logical relationships, for example with the necessary interconnections between rents, taxes and trade, more than with their precise measurement. We are dealing with arguments, which order facts, more than with the facts themselves. But awkward facts, and arguments about disputed facts, can be very helpful, since they increase puzzlement (the search for new solutions), induce humility (the acknowledgment of failure to find them), and push us to clarify the exposition of logical relationships (some objections arose because the old model was not clearly explained). Facts and arguments together allow us to see more clearly how the Roman economy worked.

The second principle is that the larger the problem, and the larger the universe which it covers, the smaller the chances of large proportionate error. By this, I mean that it is easier and more profitable to draw up a model to cover the economy of the whole Roman empire, over a longish period, than to investigate a single town or province, at a single point in time. For example, if we investigate the population size of a single town in Roman Britain, or of a single province, we can be wildly wrong. But if we are careful in our construction of a model on a large scale, with luck and good judgement, some of our errors, and some local or temporary fluctuations, should be self-cancelling.

There is a corollary to this second principle which deserves a brief discussion, partly because it goes against the grain of much scholarly historical practice. A network of estimated probabilities deals only with probabilities. Because of our imperfect knowledge, each argument by itself may be weak, but in an elegantly constructed model, the imperfect, but complementary arguments prop each other up; the principle of construction is similar to a Red Indian wigwam. The area circumscribed by the arguments considered as a set should cover and account for much of the surviving relevant evidence. Or put another [way], constructing a model is just one out of several possible heuristic tactics. We make a simplifying assumption, to see where it leads us, without facing up initially to all the complexities of the real

world. It is as though, in order to guess the weight of an elephant, you first imagine it to be a solid cube.

The third principle is that it is very much easier and more profitable to construct a model in the field of economic history than in other fields, because we are dealing here with finite variables. For example, Roman population size and money supply may be unknown quantities, but their unknownness lies within guessable limits. So as a first approximation, we might guess that the total population of the Roman empire was probably between 30 million and 120 million people; and the more probable limits are much narrower.

The fourth framing principle helps us set narrower limits to probabilities. After all, we are concerned with networks of economic factors which affect each other. For example, population size affects total product, especially in a predominantly agricultural society, and total product in turn affects the amount of tax which a government can easily raise. The more people, the more food they produce, the more possible it is for labour to be specialised, the more taxes can be raised. So the second principle runs: the choice of a value for any one factor affects the range of probable values of all other cognate variables. Or put another way, it does not make much sense trying to find out about x, unless we think of the impact of our findings about x on the interrelated variables $yzabc$. Once stated, this principle is obvious; but it is surprisingly often forgotten.[4]

The fifth principle is only an extension of the fourth principle. It restates the compatibility or coherence theory of truth: the conclusion which we reach is more likely to be accepted if it is compatible with whatever else we wish to believe. At first hearing, this may sound too reductionist and too cowardly. Surely scholars should pursue truth straightforwardly, without servile obedience to what everyone else believes? Perhaps. But I suspect that the life chances are short for a conclusion which no one believes to be true. In history, as distinct from philosophy or science, the tautology is a favourite figure of argument; to be sure it should not be a simple tautology of the type

'good generals win battles', but a complex tautology, elegant and economical in its construction, fitting many of the known facts, and not easily controvertible.

The proof or disproof of the model cannot be accomplished by illustration. For example, if I put forward the proposition that, in general, height and weight, education and income correlate highly, and for particular reasons, it is no proof of the proposition if I produce an exemplary fat giant, or super-rich professor. Nor is it a defeat of the proposition if someone produces one thin, or even several thin, undereducated millionaire Welsh dwarfs (I call this the Welsh dwarf gambit). Finally, it is not helpful to claim that the data are too diverse to make generalisation possible.[5]

Three problems then: diversity, exceptions, and proof/disproof. Diversity is relatively easy; after all, generalisations were invented to cope with diversity. Exceptions, whether extreme cases (which are sometimes called outliers) or problematic marginal cases, pose quite different problems. At one level, they helpfully remind us that the world is not so carefully ordered that everything fits into a single, and necessarily imperfect, intellectual scheme. At another level, as we shall see, exceptions, and the objections which various scholars have raised about those exceptions, force us to revise the model, or make us clarify its workings, or recognise the limits within which it holds true.[6] All that said, I now think that a good model can be defeated properly only by other means: for example, by showing that its assumptive frame is wrongly conceived, or even better by showing that an alternative model will cover more evidence, more elegantly, with fewer moving parts, and more persuasively.

A BRIEF REVIEW OF THE MODEL

My present purpose is to figure out the interconnections between several salient features of the Roman economy. As a preliminary, let us deal with four factors:

a) the total population of the Roman empire
b) average agricultural yields
c) the Gross Product of the Roman empire
d) the state budget, i.e. the total tax income of the Roman state.

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[4] For example, Roger S. Bagnall and Bruce W. Frier, *The Demography of Roman Egypt* (Cambridge, 1994) 56, by extrapolating from isolated and puzzling data, propose levels of urbanisation in Roman Egypt which are three times the levels found in Europe in 1800. Richard Duncan-Jones is also, in my view, quite often guilty of this error. For example, in *Structure and Scale in the Roman Economy* (Cambridge, 1990) Chapter 12, he argues for the importance of taxes in kind, without analysing the patterns of their consumption. In Chapter 2 of the same book, and in *Money and Government in the Roman Empire* (Cambridge, 1994), Chapter 12, he argues for the restricted local circulation of both money and pots, without considering the macro-economic implications of his conclusions. No doubt each author would defend his conclusions by saying that this is what the evidence shows; but no; it is what *their interpretation* of patchily surviving testimony shows.

[5] There is another common variant of these false arguments: the citation of a single exceptional exemplar. I call this the Mt Everest gambit.
[6] See the seductive self-awareness of Albert O. Hirschman, *A Propensity to Self-Subversion* (Cambridge, Mass., 1996) Chapter 1.

Specialists in Roman economic history know all too well that the exact size of each of these factors is unknown. Our information about each is scanty, fragmentary, and often suspect. That has not prevented its repeated use. For example, the geographer Strabo (17.1.13), citing his near contemporary Cicero, reports that the state income of Egypt in the middle of the last century BC was 300 million HS.[7] Yet the tax income of the whole Roman empire in the first century AD has been estimated as being in the region of 650–900 million HS per year (see below). Is it likely that Egypt alone generated 35–45% of the revenue of an empire covering the whole of the Mediterranean basin and beyond? It seems doubtful, and in the circumstances, it seems safer to distrust Cicero's figure.

This simple procedure illustrates my main point here, which is innocent enough. Even when we are given precious information by an ancient source, we have to think whether it is reasonable, probable, compatible with what else we know about the Roman empire. That does not seem too contentious, but its corollary is quite interesting. As Collingwood observed long ago, it is our critical intelligence which is prior to the sources; history is based on critical intelligence and reconstructive imagination plus sources, not on the sources alone (in spite of the many authenticating footnotes filled with citations from sources).[8] Or put another way, it is not the *evidence*, referred to in hushed tones, which dictates our conclusions, but our *selection* and *interpretation* of the evidence.

If we agree that the plausibility of ancient sources has to be checked, how are we to do it? When I first learnt ancient history, the dominant method in England seemed to reflect the English class system; sources were examined and ranked in order. Ancient history at Oxford, I remember reading the claim in a reference for a young scholar applying for a job in Cambridge, is taught only from first-class historical sources. Nowadays we are perhaps not so sure about rank and order; even inferior sources are widely admitted to represent views which are useful for historical reconstruction. But there are other methods. One is the comparative method, recently displayed to considerable effect, for example by Wim Jongman and

Harry Pleket.[9] It is a very useful method; we can compare what we know about later economies with what we know about the Roman economy. But strictly speaking, comparative data show only what was possible in other societies, not what happened in Rome. Comparisons have to be used cautiously, suggestively, not imperialistically. And they are particularly useful to indicate how Rome was different.[10]

The method I want to illustrate now is an exercise in co-ordinate thinking. Let me begin with three simple and unproblematic propositions:

1) Tax is a proportion of Gross Domestic Product (GDP).
2) We do not know the GDP of the Roman empire, but we can think of a *minimum*, which its *actual* GDP must have exceeded.
3) This *minimum* GDP must equal Population × (Minimum Subsistence plus Seed).

Taking one year with another, the population as a whole must eat enough to survive, and overall must grow enough seed to supply food for the next year. This self-maintaining sufficiency is, of course, not predetermined, and indeed may not always have happened. But the minimalist, stable assumption is a simplifying and useful heuristic tactic. After all, it seems more sensible to assume a hungry and stable population first, before we add the more speculative finesses of assuming a rising (or falling) population. To estimate minimum subsistence needs, we can rely on fieldwork by agricultural economists specialising in the modern third world.[11] They reckon that humans can survive and reproduce if they consume a minimum of 250 kg wheat

[7] Duncan-Jones (1994: 46 and 53, albeit with some reservations) accepts this income for Egypt, even though he estimates the total revenue of the whole Roman empire one hundred years later at 670 million HS.

[8] R. G. Collingwood, *An Autobiography* (Oxford, 1939) 79ff, for a scathing criticism of ancient history's 'scissors-and-paste men' who are not really historians at all. See similarly his *The Idea of History* (Oxford, 1946) 257ff or the revised edition, edited by J. van der Dussen (New York, 1993). Alas, scissors-and-paste people still dominate the ancient history profession, and some even pride themselves for it.

[9] Pleket *op. cit.*, see above note 3, and W. Jongman, *The Economy and Society of Pompeii* (Amsterdam, 1988). Of the two, Jongman is much more analytically sophisticated in his use of comparisons.

[10] This point is meant to deal with the objection which I have often heard voiced that you have to compare like with like. I can see no justification for this limitation. Part of the utility of comparisons lies in contrast; (some) apples are sweeter than (some) oranges. The Roman imperial economy was less sophisticated than the Chinese imperial economy in the seventeenth century. For a fruitful discussion of the economy of the Chinese empire, with repeated references to Rome, see Mark Elvin, *The Pattern of the Chinese Past* (London, 1973).

[11] Colin Clark and M. Haswell, *The Economics of Subsistence Agriculture* (London, fourth edition 1970) 57ff and 135. I should stress that these are actual minima, not optimistic minima prescribed by welfare agencies (such as the FAO) in order to boost foreign aid. I purposely use a rounded figure (250 kg wheat equivalent per person/year) in order to express the necessarily rough character of such calculations. Minimum intake depends on age, weight, climate, energy expended etc. The average food intake is equal to about 2000 calories per person/day, with about 15 kg wheat equivalent per person/year for a) clothing, plus an equal amount for b) heat and housing. At this rate of consumption, many/most people would feel hungry and lethargic.

equivalent per person/year; I stress the word *minimum*; we are deal-
ing with an underemployed population surviving on minimum
subsistence. This minimum survival ration includes some small
allowance for clothing, housing and heat. The concept 'wheat equiv-
alent' or 'rice equivalent' reflects the predominance of cereals in the
diet of hungry third-world countries, and also allows for inter-
cultural comparisons across time. Needless to say, it does not mean
that any population, or that Romans, ate only wheat. Nor does it
imply that all, or even that most, inhabitants of the Roman empire
necessarily lived at the level of minimum subsistence. These are only
provisional, simplifying calculations, useful for establishing a base
line.

We are now in a position to move to the next stage of our argu-
ment. Let us guess, without any commitment for the moment about
the truth of our guesses (though each of them is defensible and quite
close to scholarly conventions), that:

a) the population of the Roman empire was 60 million,
b) *minimum* subsistence was 250 kg wheat equivalent per person/
   year,
c) average yields were 4 × seed, i.e. that one quarter of gross
   agricultural product had to be saved each year in order to grow a
   similar crop in the next year.[12]

Now all these guesses can be put together in a single matrix, so
that we can see the implications of each separate guess for all the
other guesses. Let me emphasise that the objective here is *not* to esti-
mate the GDP of the Roman empire, but to use the minimum GDP
as an intermediate element in a network of calculations.

We have already asserted (see 3 above) that *minimum* GDP =
Population × (Minimum Subsistence plus seed). Using the estimates
cited above a–c), *minimum* GDP therefore equals:

60 million (Population) × (Minimum Subsistence = 250 kg wheat equivalent
plus Seed = 83.3 kg) = 15 million tonnes plus 5 million tonnes seed =
20 million tonnes wheat equivalent.

We can now return to proposition 1), and factor in price. Let us
also guess that: d) farm-gate prices for wheat averaged 3 sesterces per

[12] For average wheat yields of barely four times seed in first-century Italy, see Columella
3.3.4. On sowing rates for wheat at 4 modii per iugerum see Varro (1.44), 4–5 modii per
iugerum Columella (2.9, 2.12 and 11.2.75), and 5–6 modii per iugerum (Plin. *HN* 18.198).
Actual sowing rates obviously varied. Assuming an average sowing rate of 5 modii per iugerum
= 130 kg/ha seems reasonable enough. For a discussion, see best M. S. Spurr, *Arable Cultivation
in Roman Italy* (London, 1988) 41–88, or R. P. Duncan-Jones, *The Economy of the Roman
Empire* (Cambridge, 1982) 33ff.

modius of 6.55 kg = 450 HS per tonne.[13] If so, then *minimum* GDP
at 20 million tonnes was worth 9000 million HS. But tax is necess-
arily a proportion of this minimum gross product. If, as a final guess
in this matrix of guesses, we estimate the average tax rate as 10% of
minimum gross product, then total tax revenues = 2 million tonnes
wheat, at 450 HS per tonne = 900 million HS.[14] Need I stress that all
these figures are, and can be, only rough orders of magnitude? They
are better treated as metaphors than as reliable statistics.

DOUBTS AND USES

The exact size of each element in this simple equation is unknown
and untested. And yet, in spite of that, the conclusion is, in a curious
way, both compelling and useful. Let me explain why I think so.

The only known element in the Roman state budget is the total
cost of the army; perhaps 'known' is an exaggeration, but there is in
broad terms a rough agreement among modern scholars that the
army cost say 450–500 million HS in the middle of the first century.[15]
But several Roman writers tell us or imply that army costs were the
single most important item in the state budget.[16] And besides, other

[13] On wheat prices, see conveniently Duncan-Jones 1982: 145–6 and 345–6, with some
minor adjustments since it is now commonly agreed that the artaba equalled 4½ not 3⅓ Roman
modii, so that wheat prices in Egypt in the first century were probably often below 2 HS per
modius. Wheat prices obviously varied significantly, according to season, year and place (see
below note 64). 3 HS per modius as an average farm-gate price can only be a crude simplify-
ing assumption.

[14] It is easy to see why some traditional historians object to this form of reasoning. It seems
so unhistorical. Duncan-Jones dismissively calls it 'a priori not empirical' (1994: 46). But then
his estimates are based on army costs plus a series of arbitrary and unsubstantiated guesses,
including a complete guess that Vespasian increased total revenues 'perhaps by as much as
20%', which becomes 20% in his statistical table (1994: 33–46). Anyhow, mutual recrimin-
ation apart, our results are roughly similar: his 670 million, rising to 804 (*sic*) million HS in
about AD 80; mine is 900 million HS. Willy-nilly, we are playing a similar game, with similar
results. The real difference is that his reasoning is consciously inductive, mine is what I prefer
to consider as matrix thinking. And all of us in economic history need some such thinking to
check on the plausibility/utility of a) untrustworthy ancient sources and b) modern scholars'
competing estimates.

[15] We know the notional size of the Roman legionary army and the pay of legionaries, but
there is some debate about how many auxiliaries (non-citizen soldiers) there were at different
periods and about how much pay auxiliaries received. M. A. Speidel, 'Roman Army Pay Scales',
JRS 82 (1992) 87ff, argued plausibly that auxiliaries received ⅚ of legionary pay. R. Alston,
'Roman Military Pay from Caesar to Diocletian', *JRS* 84 (1994) 113ff, is radically agnostic.
But there is not a huge room for manoeuvre. Duncan-Jones (1994: 36) estimates 493–554
million HS for army pay and retirement benefits in the mid-first century (the higher estimate
if auxiliaries were paid retirement benefits). This can be only a rough order of magnitude.
I suspect there was systematic underpayment and some considerable fluctuations in numbers.
I personally therefore still prefer an estimate of 450±50 million HS.

[16] 'Our present revenues are insufficient to provide for the army and everything else' (Dio
52.6). And the early third-century jurist Ulpian explains tribute 'as being clearly called tribute
because it is attributed to soldiers' (Digest 50.16.27).

scholars have come to similar conclusions about the size of the Roman state budget, though by more inductive methods. Duncan-Jones, for example, estimates it as 670–804 million HS in the mid-first century AD.[17] My provisional estimate is 900 million HS. Given the uncertainties in such estimates, I personally regard these results as roughly similar – in the same ball-park. So the arbitrarily chosen figures for population size, average yields, wheat prices and tax rates, placed in conjunction, produce a roughly plausible result, of the correct order of magnitude.

What is wrong? There is one obvious and large omission. The *actual* Gross Domestic Product of the Roman empire must have exceeded the *minimum* Gross Domestic Product [Population × (Minimum Subsistence plus Seed)] by a very considerable margin. Many Romans ate much more than mere subsistence. A considerable minority of Romans did not produce their own food, but lived in towns, ate food produced by slaves or peasants, and produced goods or provided services (legal, religious, educational) for sale; others were peasants who produced goods for sale in their spare, non-agricultural time.[18] We have to include their total (though unknown) product in the Roman empire's actual GDP. It is difficult to estimate this *actual* Gross Domestic Product of the Roman empire, though occasionally a scholar has tried.[19] At least it seems reasonable, given the high level of urbanization in the provinces around the Mediterranean Sea in the first two centuries AD, accounting for say 10–20% of the total population, to imagine that non-agricultural production also accounted for more than a fifth and less than two fifths of total produce. After all, towns concentrated higher value labour and services, so that average product there would have been higher than in the countryside. And so, for these two reasons (1) widespread consumption above minimum subsistence, and (2) sizeable non-agricultural production, the *actual* GDP was significantly higher,

perhaps between a third and a half higher, than our minimum GDP.[20]

These speculative calculations suggest some positive conclusions and negative exclusions. By exclusions, I mean here that these calculations can be used to exclude as improbable some estimates advanced by reputable scholars. The first positive conclusion which leaps to the modern eye is that the Roman tax-rate was low.[21] If 10% of the *minimum* Gross Domestic Product would have produced as much or more tax (900 million HS) than the Roman government spent (Duncan-Jones estimates a budget of 670 million HS in the middle of the first century AD), then taxes must have been significantly less than 10% of the *actual* Gross Product. If we factor in our rough guess about the *actual* GDP as 50% above *minimum* GDP, then it looks as though taxes were levied at about 5–7% of *actual* GDP, to raise 700–900 million HS per year in tax in the mid-first century AD. At the very least, this model of the Roman economy raises the serious suggestion that Roman tax-rates were, objectively speaking, low.

Let us now turn to two exclusions. Some scholars have argued that the population of the Roman empire grew significantly above 60 million people, or that average yields were higher than 4 times seed, or both.[22] Let me try to show that both these conclusions seem improbable. If the total population of the Roman empire, because of general prosperity, grew to and stayed at about 100 million, and if

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[17] Duncan-Jones 1994: 33–46, and see note 14 above. See also note 19 below.
[18] For a quick description of the growth of the Roman economy under the impact of imperial expansion, see K. Hopkins, *Conquerors and Slaves* (Cambridge, 1978) Chapter 1. For a sophisticated summary and revision see E. Lo Cascio, 'Forme dell'economia imperiale', in A. Schiavone ed., *Storia di Roma* (Torino, 1991) vol. 2, 313ff esp. at 328ff. For a balanced survey of the economy in the first two centuries AD, see Peter Garnsey and R. Saller, *The Roman Empire* (London, 1987) 43ff.
[19] Surprisingly, serious academic estimates of the GDP of the Roman empire do exist. See R. W. Goldsmith, 'An Estimate of the Size and Structure of the National Produce of the Early Roman Empire', *The Review of Income and Wealth* 30 (1984) 263ff. His *Comparative National Balance Sheets* (Chicago, 1985) adds nothing relevant here. In detail, his method seems error prone, since his dominant tactic was to make estimates based on the average of all previous scholarly guesses. But there are some very useful hints for ancient historians, based on his knowledge of other pre-industrial economics. See note 20 below.

[20] Goldsmith 1984 (see note 19 above) reckons that total actual agricultural product (net of seed) was about 50% above minimum subsistence needs, and that non-agricultural product probably equalled at least 40% of GNP, and that average Roman living standards were significantly lower than in England in the seventeenth century. His final estimate of average product in the Roman empire in mid-first century AD at 380 HS or 125 modii per head is 3 times my estimate of minimum GDP, and to me seems too high. It implies a tax-rate of only 3%, and emphasises the weakness of the Roman government. On his side is the argument that a similar split between subsistence, above subsistence agricultural product and non-agricultural product has been found in other under-developed states.
[21] See the review by P. A. Brunt of Lutz Neesen, *Untersuchungen zu den direkten Staatsangaben der römischen Kaiserzeit* (Bonn, 1980) in *Roman Imperial Themes* (Oxford, 1990) 324ff.
[22] K. J. Beloch, 'Die Bevölkerung im Altertum', *Zeitschrift für Sozialwissenschaft* 2 (1899) 620, revised his earlier estimate of the population of the Roman empire from 54 million in 28 BC (in *Bevölkerung der griechisch-römischen Welt* 1 886: 507) to 100 million, plus or minus 20 million, in about AD 200. E. Lo Cascio, 'The Size of the Roman Population', *JRS* 84 (1994) 113ff, deals primarily with the population of Italy. He aims to reject Beloch's still standard view that Italy's population at the end of the last century BC was in the region of 5–7 million (the old view was strongly supported by P. A. Brunt, *Italian Manpower* (Oxford, 1971 and 1987) 121–30. If the Beloch–Brunt view is rejected, then I assume that Lo Cascio wants the Nissen–T. Frank estimates reinstated, and the population of Italy is estimated at 14–16 million. I cannot help wondering if apologetic national pride is partly responsible for such inflated estimates. For example, C. Jullian thought that Gaul's population in the last century BC was 20 million, and may have doubled in the first two centuries AD, thanks to the 'long famous fertility of Celtic women', *Histoire de la Gaule* (Paris, 1920) vol. 5, 25–8. In either case, the empire's population is presumably estimated at well above 60 million.

because of general prosperity GDP rose proportionately with the population, then tax-rates averaged $^6\!/_{100} = \frac{3}{5}$ of the level argued for in the previous paragraph. Those who argue for a population of 100 million are implying an average tax-rate of 3–4% of *actual* GDP, and that with average yields of four times seed. If average yields were higher, then tax-rates were even lower. But if taxes were so low, the Roman government should have faced little difficulty in raising tax-rates, or in increasing army rates of pay.[23] But rises in army rates of pay were rapidly followed by difficulties with the coinage system, and were usually resolved by debasement (more precisely, by reducing the silver content of the coinage, or its weight, or both). If the Roman empire ran on a 3–4% tax-rate, it should have been able to increase revenues in an emergency. Any enemy attack would have induced a minor tax hike, and a massive increase in central government resources. This did not happen. It is therefore easy to conclude that the empire did not have at its disposal a population of 100 million people.

Other scholars have argued either that Roman wheat yields were significantly higher than four times seed, or that they were higher in important areas of Roman agriculture, in addition to the well-known exception of Egypt, irrigated by the Nile.[24] Once again, I am concerned here as much with the intellectual shape of the argument as with the (im)plausibility of the results. Let us proceed by supposing the high yield advocates are right. What are the implications of their assumptions? We can examine the argument, both macro-economically at the level of the whole economy, and micro-economically at the level of the single farm.

First, macro-economically. Let us take the example of Italy. Roughly speaking, experts argue that Romans in Italy cultivated perhaps 10 million hectares, about 40% of the surface.[25] If yields

[23] One way round this difficulty would be to argue that government revenues were so low, and could not be raised, because of the obstructive power of the aristocracy who depended on rents. I too think that rents and taxes were competitors for a limited surplus, but see the emperors in the first two centuries AD as having an edge over the aristocracy. But such perceptions are matters of judgement and of competing probabilities.

[24] For example, Pleket 1990: 78–9 (cf. note 3 above). On agricultural yields in Egypt, see, for example, A. K. Bowman, *Egypt after the Pharaohs* (London, 1986) 17–18. With several qualifications, specialists in ancient Egypt often argue for average yields there on irrigated land of about 10 times seed.

[25] See the slightly different, consumption-led calculations of Italy's total produce by Brunt, *Italian Manpower* 1971 and 1987: 126, following Beloch. He also assumes an average yield of four times seed, and an average consumption of wheat of about 250 kg wheat per person/year, and assumes that the city of Rome was fed from imports; he also thinks that Roman farmers even at those yields left half their fields fallow. I think that this calculation underestimates consumption by draught animals and horses, and also perhaps consumption

averaged six times seed (650 kg wheat/ha net of seed), then Italy alone produced enough food to feed 26 million people, or roughly speaking almost half of the population of the whole empire.[26] But we know very well from a myriad of sources that Italy and Rome in particular were importers of food. Again even with half the cultivable land fallow, the assumption of widespread high yields looks mistaken.

We reach the same critical conclusion if we think micro-economically. Let us speculate about single farms.[27] If, for the sake of example, a peasant had a farm of 5 hectares (20 iugera), and if yields averaged six times seed, then crops equalled 650 kg net of seed (sowing at 130 kg/ha = 5 modii per iugerum; see note 12 above). The farm then yielded 3250 kg wheat per year. Roughly speaking, since a family of four persons at minimum subsistence requires 1000 kg wheat equivalent per year, this assumption implies that one third of the Roman population could have fed two thirds living in non-agricultural occupations. But it is generally agreed that the non-agricultural sector of the Roman population amounted to barely 10–20% of the whole. And even if the high yield farmer restricted his output by keeping half his land fallow, he could still support one third of the population in non-agricultural occupations. No. It looks as though the conclusion of widespread high yields in Roman farming (outside irrigated areas such as Egypt or small favoured locations) is mistaken. Above all, the intellectual moral is that tentative isolated results should be scrutinised in terms of their broader implications.

by Italians over and above minimum subsistence. But the differences between us are a salutary reminder of the tentative nature of such calculations and of their roughness. They can provide rough orders of magnitude only.

[26] Italy's actual population was probably in the order of 5–7 million in the beginning of the first century AD. It may have risen during the first two centuries, then declined in the third century and risen again in the fourth century. We know from post-mediaeval demographic histories that there were broad long-term waves of population growth and decline, but our data from antiquity are rarely good enough to be able to quantify such changes. For short- and long-term population swings, see E. A. Wrigley, *Population and History* (London, 1969) 62ff. The best introduction to ancient demography is T. Parkin, *Demography and Roman Society* (Baltimore, 1992).

[27] Calculations about the size, yield, agricultural practices, and consumption needs of the single farm/farming family are obviously complex and require a parametric model of their own. *Exempli gratia*, a 5 hectare = 20 iugera farm, if half was left fallow each year, and if the yield was four times seed (520 – 130 seed = 390 kg wheat/ha), would produce 975 kg wheat per year on average. That is less than minimum subsistence for a family, with no allowance for plough oxen, rent or taxes. A 5 hectare farm, wholly cultivated, with a yield of four times seed, would produce 1950 kg wheat net of seed, enough food for a family of four at subsistence, and an ox, and still be able to produce a sizeable surplus, which could be extracted as rent or tax, or consumed by the farmer. Less or more land, yield and fallowing would dictate both the farmer's living standards and his need to work for a landlord.

LOW TAXES AND ARISTOCRATIC SELF-ENRICHMENT

Why were Roman taxes so low? By low, I mean here less than 10% of *minimum* Gross Domestic Product, and only about 6% of probable *actual* GDP. Of course, I should stress right away that there is a fundamental distinction between taxes levied and taxes transmitted. The central Roman government was primarily interested in taxes transmitted, i.e. in the amount of taxes which eventually reached its own coffers.[28] The *actual* amounts of tax levied by tax-collectors on the ground were likely to have been significantly higher, and they were probably unfairly distributed; the powerless paid a disproportionately high share of the taxes, while the powerful, who supervised the collection of taxes or the distribution of the tax burden, understandably let themselves off lightly. So complaints about the heaviness of Roman taxation, or the cruelty of Roman tax-collectors, are quite compatible with an overall low level of transmitted taxes.[29]

All that said, an average tax burden in the rough region of 5–7% of *actual* Gross Domestic Product seems low. Why were taxes so low? Two answers come readily to mind. The first is genetic, the second structural. The genetic explanation is that the Roman empire came into being as an empire of conquest; its first taxes were devised to replace booty as a more effective method of covering the costs of war. The state's needs started at a low level; the rapid expansion of the empire in the last two centuries BC meant that the Roman state repeatedly had more revenues flowing in than ever before.[30] The

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[28] The Roman tax-system seems to have involved assessing towns at a particular sum, and then leaving local leaders to allocate the tax among their own land-owners. See *IG* v (1) 1432–3 for example, dating perhaps from the first century AD. It shows that the southern Greek town of Messene was assessed at 100,000 denarii (400,000 sesterces), divided among land-owners in strict proportion to their declared capital value (self-assessment itself as a system favours the socially powerful rich – who is to challenge their assessments?). Even so, it was outsiders, including Romans, who were particularly negligent in paying their dues.

[29] For a highly rhetorical report of cruelty by tax-collectors in first-century Egypt, see Philo, *de specialibus legibus* 3, 159, who describes a tax-collector torturing the wives and children of fugitive tax-payers; cf. Lactantius, *de mortibus persecutorum* 23. More generally, still see best A. H. M. Jones, 'Taxation in Antiquity', in A. H. M. Jones, *The Roman Economy*, ed., P. A. Brunt (Oxford, 1974) 151ff, and see similarly the hostile and anti-Roman comments by rabbis on Roman tax-collectors' greed, analysed by N. de Lange, 'Jewish Attitudes to the Roman Empire', in P. Garnsey et al. eds., *Imperialism in the Ancient World* (Cambridge, 1978) 255ff, now partly superseded by M. Hadas-Lebel, 'La fiscalité romaine dans la littérature rabbinique ...', *Revue des études juives* 143 (1984) 5ff.

[30] The classic overview of Roman state income and expenditure from 268 BC onwards is to be found in T. Frank, *Economic Survey of Ancient Rome* (Baltimore, 1933) vol. 1. Although Roman leaders knew and published statements about the total income of the Roman state, only fragments have survived. According to Plut. *Life of Pompey* 45, the income of the Roman state in 62 BC was 340 million sesterces, compared with an average estimated income in the first half

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conquerors provided only a sketchy service to the conquered: rudimentary corrective justice, supervision (of tax-collection), external defence, with occasional support for internal infrastructure (ports, roads, temples) and internal order. But the main provision was in external defence, divertable if needs be towards the ruthless suppression of rebellion.

The structural reason for low taxes is that the Roman conquerors had always operated a binary system of profiting from empire. By a binary system, I mean that the Roman state shared the profits of empire with its own citizens. As is well known, in the period of imperial expansion from about 200 BC–AD 14, conquering generals and provincial governors took a far greater (and increasingly larger) share than common soldiers. Equestrian tax-collectors and the relatively rich private investors in tax-collecting companies (who bid at auctions for the right to collect taxes in a district or province) profited more than private Roman citizens who stayed at home. But even ordinary citizens in Italy got some profit from empire. After 167 BC, citizen land-holders paid no tax on Italian land (except in emergencies); and citizens living in the city of Rome received a monthly dole of wheat at subsidised prices from 122 BC, and absolutely free from 58 BC. Finally, tens of thousands of Italian citizens emigrated from Italy to the provinces, especially in the second half of the last century BC, in order to get larger farms than they had had in Italy.[31] In short, Romans got richer by profiting from empire.

Two final elements in this complex process of self-enrichment need to be mentioned. First, especially after the Augustan settlement, upperclass Romans moved swiftly to occupy important tracts of provincial land. In Nero's reign, for example, six men of senatorial rank were alleged to own half the province of Africa. And, for example, rich Romans owned substantial estates in Egyptian villages.[32] How else could Romans maintain their ostentatious

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of the second century BC of 55 million sesterces (Frank 1933: 145). In sum, Roman state income had increased sixfold, and was to more than double again in the next century.

[31] On the increase in booty from empire and the progressive relative enrichment of the Roman elite, and the consequent migration of Italian peasants to Rome, other Italian towns and the provinces, see Hopkins 1978: Chapter 1.

[32] The allegation that six Roman land-owners owned half the province of Africa when the emperor Nero killed them is made by the encyclopaedist Pliny (*HN* 18, 35). A mid-second-century papyrus from the Fayum in Egypt (P. Bouriant 42), for example, lists estates which had once belonged to the senator Seneca, to the equestrian praetorian prefect Macro, to various freed imperial slaves and to members of successive royal families. In the late empire, the noble Christian lady Melania owned estates in six provinces (*Life* (Greek) 11–12, 19–21, 37), see Elizabeth A. Clark, *The Life of Melania the Younger* (New York, 1984); another aristocratic lady, Olympias, owned estates in four provinces (*Life* 5); see E. A. Clark, *Jerome, Chrysostom and Friends* (New York, 1979) 107ff.

luxury, except by profiting from the whole empire?[33] Secondly, especially under the rule of the emperors, the political economy of the Mediterranean basin became increasingly integrated.[34] Not only did rich Romans own land in all the provinces, but increasingly rich provincials became Romans. By the end of the second century AD, about half of all senators were of provincial origin; and two second-century emperors passed regulations that senators should have ⅓, reduced by the second emperor to ¼, of their total wealth in Italian land.[35] The mere existence of the regulation illustrates the extent to which senatorial investment in land had become provincial. Finally, in 212, most inhabitants of the empire became Roman citizens. The old dividing line between conquerors and conquered had been replaced by an empire-wide system of stratification between the more powerful (*honestiores*) and the unprivileged (*humiliores*).

The increased wealth of the Roman rich under the shelter of emperors' rule is generally veiled by a prevailing aristocratic ideology, which reports cruelty and persecution by emperors of individual aristocratic families, and laments the decline of aristocratic political power compared with its glorious republican past.[36] But the few surviving statistics on aristocratic wealth suggest significant increases in aristocratic wealth. Cicero in the middle of the last century BC wrote that a rich Roman needed an annual income of 100–600,000 HS per year, an income clearly implying a total fortune well in excess of the minimum capital required to become a senator (1 million HS). Pliny the Younger, himself a senator of middling wealth at the end of the first century AD, may have had an annual income of at least

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[33] The Roman imperial state unlike post-mediaeval European kingdoms offered aristocrats no reliable offices of profit at court. Aristocrats therefore had to meet the expenses of being courtiers basically out of their incomes from estates (including urban rents), from their mercantile activities (administered by their freed slaves), as well but less predictably from gifts from the emperor and profits from occasional but discontinuous office in the provinces.

[34] By political economy, I refer here to the extent to which the Roman state, by its policies, but probably unintentionally, affected the ownership of wealth. What I have in mind is that the peace and security of the state acted as a carapace protecting the interests primarily of the Roman ruling classes. For a similar broad phenomenon in past feudal European monarchies, see Perry Anderson, *Lineages of the Absolutist State* (London, 1974).

[35] On the regulations governing senatorial ownership of land, see Plin. *Ep.* 6.9 and SHA, *Marcus Aurelius* 11. On the process of integration in the Roman elite and their lowish rates of inheriting status, see K. Hopkins, *Death and Renewal* (Cambridge, 1983) Chapter 3 (by Hopkins and G. P. Burton). For some good, but mostly ill-founded, criticisms of our statistical reasoning, see Johannes Hahn and P. M. M. Leunissen, 'Statistical Method', *Phoenix* 49 (1990) 60ff, and now a convincing rebuttal by G. P. Burton, 'The Inheritance of the Consulship', *Phoenix* 49 (1995) 218ff.

[36] The conflict between successive emperors and aristocrats is the leitmotif of surviving Roman historians of the early empire. For example, the emperor Claudius is reported to have had 35 senators and more than 300 knights put to death (Suetonius, *Claudius*, 29, cf. *Tiberius* 54, Dio 67.9).

1.1 million HS; and the historian Olympiodorus reports that middling aristocrats in the west in the late fourth century AD had incomes of 1333–2000 Roman pounds of gold, roughly equivalent to 6–9 million HS per year.[37]

If these figures are roughly indicative, aristocratic fortunes had risen two- or threefold from the late Republic to the end of the first century AD, and had risen again five- to eightfold between AD 100 and AD 400. Such skimpy data can only be suggestive. But aristocratic enrichment is what we might have expected from general trends in the politicoeconomic integration of the empire. After all, Cicero and his contemporary senators had practically all their wealth invested in Italy, whereas the fourth-century elite controlled investments from all over the Mediterranean basin. Imperial government, for all its alleged cruelty to aristocrats, provided an effective shelter for their progressive enrichment.[38]

This marked increase in average individual aristocratic wealth should also be considered as an increase in aggregate aristocratic wealth. But in an economy with a limited surplus, rents and taxes were in competition. The aggregate wealth of the political aristocracy limited the state's capacity to act.[39] Let us consider again the figures

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[37] See Cicero, *Paradoxa Stoicorum* 45–9; for Pliny, see the detailed estimates by Duncan-Jones 1982: Chapter 1; for Olympiodorus, see R. C. Blockley, *The Fragmentary Classicising Historians of the Later Roman Empire* (Liverpool, 1983) vol. 2, frag. 41.2.; Olympiodorus claimed that middling senators had an annual income of 1000–1500 Roman pounds of gold; but if they, like the richest senators, had an additional income in 'grain, wine and produce ... equal to one third of the income in gold' (ibid.), then their total incomes averaged 1333 to 2000 Roman pounds of gold. I translate that (rather crudely) into sesterces by equating each pound of gold with 45 aurei each worth 100 HS; that was the minting rate in the second half of the first century AD. Duncan-Jones also lists the size of 29 fortunes in the Principate (1982: Appendix 7). For a careful warning against trusting many figures reported in ancient sources, see Walter Scheidel, 'Finances, Figures and Fiction', *Classical Quarterly* 46 (1996) 222ff.

[38] The two arguments being advanced here clearly need to be kept separate. The first argument is that average and aggregate aristocratic wealth increased; the second argument is that this increase may have accelerated in the chaotic conditions of the third century, when local aristocrats wrested control over greater resources, probably to the disadvantage of smaller independent land-owners. Eventually, in the western empire, the local power of aristocrats prevented the central government from effectively raising its revenues sufficiently to repel barbarian attacks. Admittedly, this argument faces considerable problems of definition and fact, if only because the boundaries of the fourth-century elite are much wider than the senatorial and equestrian strata of the early empire. Even so, it seems worth bearing in mind the changing economic base of political groupings.

[39] I use political aristocracy as a vague term on purpose. What I have in mind is say the top few thousand families in the Roman empire: six hundred or so senators, a few hundred leading equestrian administrators, and the landed, mostly equestrian families from whom senatorial families had or would come. Romans had a fairly rapidly changing aristocracy of office, with only between one fifth and a third of second-ranking (*suffect*) consuls in the first or second centuries having a consular son, grandson or great-grandson. The Roman political elite was much more volatile than any post-mediaeval European nobility. See Hopkins, 1983: 142.

already cited. If Pliny the Younger, a senator of middling wealth, had an annual income of at least 1.1 million HS, then all 600 senators, plus knights plus high palace officials, must surely have had a collective aggregate income of at least say 600 million HS. This aggregate elite income was significantly more than the Roman state's income, net of army costs.[40] I cite these figures only as very rough orders of magnitude for illustrative purposes.

My overall argument here is twofold. First, taxes and aristocratic rents were rivals for a limited surplus. Secondly, disposable aristocratic incomes, which like taxes were generated mostly from land-ownership, were of the same rough order of magnitude as taxes, especially if we consider taxes as largely precommitted to supporting the standing army, whereas aristocratic incomes were slightly more fluid, more redirectable to alternative ends. *Cognoscenti* will already have thought of an obvious objection: imperial estates. Precisely. Emperors confiscated rich people's estates, because these estates constituted such important elements in graspable national wealth. But by the same token, emperors were caught in the delegatory bind; they could not administer these large and scattered estates themselves; and they had their own favourites to reward and keep loyal. The recipients of imperial largesse or the administrators of imperial estates were in effect replacement aristocrats (irrespective of their formal status, sometimes they were knights) for those who had had their estates confiscated.[41]

SOME IMPLICATIONS OF AND OBJECTIONS TO THE MODEL

In order to analyse the competition between taxes and rents further, I want to return once more to a central proposition of my old article, 'Taxes and Trade'.[42] I return to this proposition, partly because the sharpness of some criticisms has forced me to reconsider, and I hope improve, some of my arguments.

My basic argument then was that taxes or rents raised in place P (provinces) and spent in place R (Rome and the frontier armies) were

[40] Duncan-Jones (1994: 45) reckons second-century non-army government expenditure at 189–279 million HS. I suspect this guess/calculation is rather low, for example in its estimate of the emperors' household costs, and too precise. I would think in terms of 400–500 million HS. This difference illustrates our divergent intellectual styles.

[41] Duncan-Jones 1994: 5–6 and 42–3 gives examples of emperors' confiscations and gifts, and then states (p. 43): 'Clearly there was no real barrier to what the emperor could spend.' Clearly and really?

[42] See note 1 above.

necessarily, in the medium term, balanced by an equal flow of trade, through which tax-payers or rent-payers in P earned money with which to pay their taxes. The process which I had in mind was that tax- or rent-payers, in so far as they were forced to pay their taxes or rents in money, had to earn the money with which to pay their rents or taxes whether by the sale of produce or services, or both. And if the tax-users or rent-receiving landlords were at some distance, for example in the city of Rome, then goods of an equal value (roughly speaking) to the taxes and rents raised had to be exported via merchants in Rome, so that tax-/rent-payers could get their money back, with which to pay taxes and rents in a subsequent year.

Of course, I never had in mind a simple four-axis flow: from a) tax-payer or b) rent-payer on the one hand to c) tax-spenders and d) land-lords on the other hand. Instead, I imagined a whole differentiated network of converters, whose mode of operation and function would differ, depending on whether they were dealing with taxes or rents, in money or in kind. For example, peasants might pay rent in kind to a local landlord or his agent; the landlord then sold the peasants' surplus to wholesale merchants, who in turn either exported it un-converted, or sold it to local artisans, located either in local villages or towns, who ate the peasants' surplus wheat and meanwhile made pots, shoes or other artefacts, whether for local sale or export.[43] But if rents or taxes were exacted in money, the peasant was forced to sell his surplus wheat to a local merchant in order to earn cash, or to work for a local landowner, or even did both. If money taxes or rents were spent at some distance from where they were collected, then a network of trade and manufacture had to be established, so that surplus produce could be sold, perhaps via a variety of towns, and original tax-payers could earn their money back. In this view then, the exaction of rents and taxes, whether in money or kind, was an, and perhaps the most important, element in the complex process of increasing the specialisation of labour (more shoemakers, barbers and lawyers), and the consequent growth of towns throughout the Mediterranean basin. Towns were undoubtedly a political and cultural phenomenon, but they needed economic underpinning, and themselves created economic repercussions.

[43] For taxes in kind, see below. For a fruitful discussion of local networks of trade between peasants and specialist local traders, see now L. De Ligt, *Fairs and Markets in the Roman Empire* (Amsterdam, 1993), especially Chapters 4 and 6. I still cannot help thinking that De Ligt underestimates the aggregate demand of peasants for urban produced goods, while acknowledging with him that they also consumed rural produced goods and that much of the exchange might have taken place at rural markets, outside towns. His discussion is (unlike so much ancient economic history) well informed, theoretically and comparatively.

The impact of rents and taxes probably differed. Landlords were more dispersed than the spenders of taxes. Absentee landlords lived in local and regional capitals as well as in the city of Rome. So rents had much more impact on local economies. Taxes, by contrast, were, I imagine, spent only to a limited extent in the provinces where they were raised. Mostly taxes were spent on the armies stationed in frontier provinces, and in the city of Rome, on the emperor's palace, on his self-indulgences and on administration, as well as on the palace guard and other troops in the city of Rome. Even so, the expenditure of metropolitan elite rents and taxes (net of army costs) together, totalling (say 600 million HS rents + 400–500 million HS taxes, as very rough orders of magnitude) something like 8% of *actual* GDP, concentrated in the city of Rome.[44] In so far as they were raised in money at a distance from the place where they were spent, they inevitably stimulated a network of complementary trade. Tax-payers and rent-payers had to earn back the money transmitted to Rome, as to the frontier armies. The network of exchange created by the reciprocal flows of taxes, rents and trade contributed to the integration of the Roman economy.

That is the theory. How it all worked out in practice may well be a different matter. Models do not mirror reality exactly. And for the moment I want to stress the logical, abstract, model-like nature of the proposition on the complementarity of taxes, rents and trade. It is in my view a logical equivalence, rather like Fisher's famous price equation:

$$P = \frac{M \times V}{Q}.$$

The logical connections cannot be proved or disproved by examples, or by counter-examples. The only proper disproof, I claim, is by counter-argument, i.e. by proving that there is no necessary recipro-cal flow between taxes and rents paid in money on the one hand, and trade on the other hand, or by putting forward a better (more economical, elegant or comprehensive) model.

The model has provoked some discussion, and some criticism, and thanks to that criticism I think I now see more clearly than I did in 1980 some strengths and weakness in my arguments. Howgego in two very clear and scholarly articles has been particularly helpful and

persuasive.[45] Only one of his objections seems weak. He argues that there were exceptions to the complementary flow of taxes and trade: provincials could have [paid] and probably did pay taxes out of stored treasure, they created new money out of mines, they fell into debt, their labourers migrated, and perhaps most important, they transferred their capital assets, principally land, to Roman owner-ship, perhaps to meet their accumulated debts.[46] I agree. These were all temporary expedients designed to soften the impact of the impo-sition of money taxes (mines were not temporary, but localised), and some were important transitional stages in the unification of the Roman economy, and in the transfer of capital and ownership from provincials to Romans. But they are qualifications, rather than refutations. The logical relationship between rents/taxes and trade still stands, if only after all these temporary processes had been exhausted. Methodologically, these processes constitute a marginal case, and do not defeat the basic proposition.

That said, reading Howgego made me rethink a whole series of problems. I shall deal here with three issues, which are best kept separate: first, proof and disproof of general propositions; secondly, methodology and the utility of illustrations; and third, specific points of fact, some critical, some seemingly trivial, some leading to useful revisions or clarifications of the model. Some problems have arisen, I think, because some critics, true to the positivistic traditions of history, have simply taken for granted that showing errors of fact would be sufficient disproof of my propositions.[47] I do not think that disproof is so simple. Let me illustrate and explain.

In my original article, I used four illustrative diagrams. Three have caused trouble. The first was a bar-chart showing the relative frequency of shipwrecks, by two-century periods. Basically, it showed that there were more shipwrecks (and from those I deduced more ship sailings) in the period 200 BC–AD 200 than in any period before or since, up to the early modern period. Subsequent research and refinements have, if anything, reinforced the 1980 picture.[48] Actually,

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[44] By *actual* GDP, I mean here my very rough estimate of actual GDP at *minimum* GDP plus 50%, to allow for non-agricultural and higher value urban production. It is significantly less than Goldsmith's estimate of actual GDP; see note 20 above.

[45] Christopher Howgego, 'The Supply and Use of Money in the Roman World 200 BC to AD 300', *Journal of Roman Studies* 82 (1992) 1–31, and 'Coin Circulation and the Integration of the Roman Economy', *Journal of Roman Archaeology* 7 (1994) 5–21.

[46] Howgego 1994: 18.

[47] One exception is E. Lo Cascio 1991: 351ff (see note 1 above). He saw, correctly as I too now think, that my illustrations were flawed, but that the model with suitable modifications still has merits. I have incorporated most of his modifications with gratitude.

[48] It was based on the work of A. J. Parker. Parker has now refined his data. See now *Ancient Shipwrecks of the Mediterranean and the Roman Provinces* (London, 1992) 10ff. Parker's latest data cover almost 800 dated wrecks from the third century BC through the end of the fourth century AD. They show marked growth and decline. The numbers of datable wrecks

I always thought this chart merely confirmed what many scholars 'knew', namely that there was more trade in the western Mediterranean in this period, partly as a function of the Roman imperial expansion, subsequent peace and increased general prosperity, than in prehistory or after the Vandal invasions. The relative frequency of shipwrecks/ship-sailings also suggested, and that may have been anathema to some, that shipwrecks/ship-sailings diminished progressively in the third and fourth centuries AD. So some of my colleagues objected on the grounds that the evidence was biased, if only because western underwater archaeologists were much more active in southern France than in north Africa or the eastern Mediterranean. This was true and stated in my original article. But to be a powerful objection, the objector has to claim not only that some data are missing (they nearly always are in ancient history), but also that he/she thinks that the overall pattern would be different if the missing data (here shipwrecks from the eastern Mediterranean) were included.

A roughly similar objection has been made, and powerfully expressed, to the second illustration, showing the significant increase in the volume of coinage in the last century of the Republic. Again it is an objection about accurate measurement. Buttrey doubts that money growth in this period can ever be measured accurately.[49] He argues from a position of impeccable agnosticism, that the average number of coins struck per die is and must be unknown, because it varied significantly, and that the attrition rate, i.e. the rate at which coins were lost from circulation, also varied and cannot be known. My defence is simple. But first a word of caution. I did, as Buttrey generously confirms, repeatedly stress in my 1980 article the tentative and hypothetical nature of the estimates made; and not only as a rhetorical device, a *captatio benevolentiae*. That said, there are limits to reasonable caution.

We are dealing with a very large number of dies (hundreds each year) used to produce millions of silver coins in the last century of the Republic. There must be *some* average rate of production per die, and there is little reason to think that this *average* fluctuated madly. Even if Romans got better at coin production, as the volume of their

discovered peaked in the last century BC (N = 183) and first century AD (181), were almost as high in the second century BC (154) and the second century AD (158), diminished in the third century AD (86) to less than half the first-century peak, and diminished again in the fourth century AD (54) to less than one third of the peak; (cf. 64 wrecks in the third century BC). Such data can only be suggestive.

[49] T. V. Buttrey, 'Calculating Ancient Coin Production: Facts and Fantasies', *Numismatic Chronicle* 153 (1993) 335ff, and 'Calculating Ancient Coin Production II: Why It Cannot Be Done', ibid. 154 (1994) 341ff.

production increased, so that more coins on average were minted per die, then my argument, that the volume of coinage increased, holds *a fortiori*. So also for attrition rates; we do not know what the average rate of attrition was, but there must have been some attrition; 2% average loss rate per year is merely a simplifying assumption. The attrition rate could have been 3% per year, or even x% per year. In fact, any reasonable constant loss rate and any reasonable constant average number of coins minted per die make very little difference to the *shape* of the curve. My general point holds, and Buttrey does not dispute it: the volume of coins in circulation in the last century of the Republic rose considerably; I say tenfold, but that is only a rough order of magnitude, perhaps better treated, like so many numbers in ancient sources, more as a metaphor than as a statistic.[50]

Logically, the more powerful objection to both bar-charts would have been that they were only marginally relevant to the basic proposition on the reciprocity of rents/taxes and trade. From a purist point of view, the charts were only rhetorical adornments, side illustrations, which lent my general argument an air of empirical plausibility. But the basic argument itself, as I have already said, cannot be defeated by establishing the correctness or falsity of its illustrations. This point will be even clearer when we turn to the fourth illustration (Hopkins 1980: 113), in which I sought to show that coins in the Roman empire circulated, and that any increase in the volume of coinage in one province in one period was matched by a similar rise of coinage in other provinces. I hypothesised that the complementary flows of taxes and trade were the most likely vectors which brought about this effective distribution of Roman coins throughout the empire, both in provinces where the army was stationed and in other internal and non-army occupied provinces.

Both Howgego and Duncan-Jones have spent some considerable effort in examining these arguments and the evidence on which they could be based. It was Howgego's judicious article which showed me

[50] I would be happy enough to sacrifice my hypothetical total (based on an average of 30,000 coins per obverse die) of Roman silver coins in circulation, at about 400–500 million denarii in the middle of the last century BC (Hopkins 1980: 109). But Ian Carradice, *Coinage and Finances in the Region of Domitian* (Oxford, 1983) 83–6, estimates annual average silver coin production towards the end of the first century AD at 15 million denarii per year, and Duncan-Jones, 1994: 165–8 suggests a similar volume of production of about 17 million denarii per year for the first half of the second century AD (though apparently on the basis of 8000 coins per die on average). At 2% per year loss rate, these figures suggest a total money supply of silver coins of 750–850 million denarii, though Duncan-Jones' estimate is much higher (1994: 168, and see my note 90 below). If you reduce late Republican money supply too much, then you have a huge boom in money supply to account for in the first century AD even allowing for the fact that then Roman coinage covered the whole of the Mediterranean basin (except Egypt) and beyond.

the error of my ways. He examined the evidence of coin circulation with much more expertise than I have ever had, and came to the general conclusion (against Duncan-Jones) that in the longish run coins did circulate between provinces, except that in one particular case, i.e. Spain, just after its conquest by Rome, even though it was taxed, there is no evidence of Roman coinage.[51] In his view, conquest by itself did not induce coin circulation, but silver coins in the first two centuries AD did circulate between provinces, albeit gradually.

In other words, in Howgego's view, Hopkins' proposition may be right in fact, but it is not universally right, and it is not at all clear from this coin evidence that taxes or complementary trade were the main vectors integrating the Roman economy. On reading Howgego, I had a flash of revelation. The whole argument of coin circulation was not vital to my thesis on the complementarity of taxes and trade. Or put another way, I had been looking for a confirmatory illustration in the wrong way.

Egypt is the critical case. Egypt under Roman rule had an enclosed coinage system. Broadly speaking, no Egyptian coins are found outside Egypt, and almost no Roman coins are found in Egypt. In other words, there was no circulation of coinage between Egypt and other regions of the Roman empire. Nevertheless, I would still argue that Egyptians paid taxes in money (as well as in kind), which were largely spent outside Egypt, and that this export of taxes obliged Egyptians to export goods with which to earn money, which then had to be paid in tax in successive years. Here at least the issue of coin circulation is clearly a red herring, and so, in strict logic, coin circulation must also be not critical to the interrelation between other provinces.[52] There must have been some other form of balancing trade-, rent- and tax-flows between provinces, other than by exporting silver coin (see below). In my view, the proposition on the complementarity of rents, taxes and trade is right, because I cannot see or imagine any other way in which the broad mass of tax-payers in the long run could have got money with which to pay their taxes, except via trade. In short, it is not a question here of fact, it is a matter of argument. But more of this in a moment.

Duncan-Jones in several chapters of two books ('Trade, Taxes and Money'; 'Separation and Cohesion in Mediterranean Trade';

'Taxation in Money and Taxation in Kind'; see note 51) has touched on issues which were central to my original article, 'Taxes and Trade'. His work is interesting for me on three counts: perspective, method and conclusion. I shall deal here with four of Duncan-Jones' arguments. As I understand it, he has concluded that taxes were levied to a significant extent in kind, not in money (with the implication that the reciprocity of money taxes and trade was relatively unimportant); secondly, that the circulation of goods was pocketed, i.e. that few goods were interregionally traded; thirdly, that coins also stayed within the region of their first location (with the implication that the economy of the Roman empire was not integrated); and finally, if long-distance trade did increase during the first two centuries AD (which he doubts), then it was unlikely that this was caused by levying taxes in money (Duncan-Jones 1990: 45). Each of these conclusions is important to our general understanding of the Roman economy; but in each case, my views and those of Duncan-Jones are distinctly different, in both substance and method. But once again, the conflict of views has helped clarify and, I think, improve my own arguments.

First then, let us deal with the problem as to whether taxes were levied mostly in money or in kind, beginning with technical details of source interpretation and going on to general argument.[53] Duncan-Jones begins his discussion by citing the second-century land-surveyor Hyginus: 'In some provinces, they pay part of the crop, some (provinces) paying one fifth, others one seventh, while other (provinces instead) pay in money, ... (205L)' (so Duncan-Jones 1990: 187). Duncan-Jones understandably concludes that some provinces paid land-tax in kind (at 14–20%), others in money, and he stresses the importance of the order in Hyginus' list; Hyginus gave 'first place to payment in kind' – this is section 1.1.1 of Duncan-Jones' chapter. But for this text, he cites Lachmann's 1848 edition of Hyginus. Yet the Teubner edition, edited by C. Thulin in 1913, is superior; the text reads: 'in quibusdam [provinciis] fructus partem praestant certam, alii quintas alii septimas, alii pecuniam'.[54] Thulin's *apparatus criticus* shows that a major ancient manuscript, P, has a variant reading: instead of '*other (provinces pay) money*', it reads '*now many (provinces pay) money*' nunc multi pecuniam. This variant reading is clearly either a comment added in antiquity, which indicates that at some stage taxes in money widely replaced taxes in kind, or more

[51] Howgego 1994: particularly 17ff.; cf. Duncan-Jones 1990: Chapters 2, 3 and 12, and 1994: Chapter 12.
[52] Unless one were to argue that the economic links between Egypt and the rest of the empire were quite unlike the links between the other provinces. That is possible, but it seems unlikely that they were so different as to require a different conceptualisation.

[53] Duncan-Jones 1990: Chapter 12, called 'Taxation in Money and Taxation in Kind'.
[54] C. Thulin ed., *Corpus Agrimensorum Romanorum* (Leipzig, 1913) 168. I am grateful to Michael Reeve for advice on this point.

probably this reading was the original version of Hyginus; a scribe
mistakenly added a third *alii* (others) instead of the original 'now
many (pay) money.' So the strongest element in Duncan-Jones' argu-
ment from sources disintegrates.

But now for a more general, and I think more interesting, argu-
ment.[55] Overall, there must have been a serious imbalance between
a) what the government raised as tax in kind and b) its needs. Once
again the case of Egypt is illuminating. The Roman state needed
wheat with which to supply the citizens receiving a wheat dole in
Rome, and basic food for its soldiers, dispersed along its frontiers.
Distributions of free wheat to the citizen population in Rome
accounted for only about 100,000 tonnes per year (250,000 recipi-
ents × 5 modii of 6.6 kg per month). The army of say 300,000
soldiers needed slightly more. There was, as far as we know, no other
regular demand for the government to supply taxes in wheat to state
dependants. But by Duncan-Jones' own calculations, the Roman
government raised 17.5 million artabae = over 500,000 tonnes
annually in tax from Egypt alone (plus money equalling about 40%
of the whole).[56] The amount of wheat raised in tax in Egypt was,
by these calculations, more than twice as much as the government
needed in the whole empire. We have to conclude, therefore, that
taxes which were raised in kind, in Egypt and in other provinces, had
to be transformed into money, in order for the government to be able
to spend the revenue elsewhere.

These simple calculations transform the problem of taxes in
money and taxes in kind. Let us first think of the problem on a large
scale, interprovincially, and then consider the micro-economic prob-
lems of individual tax-payers. Macro-economically, it is now not so
much a question about the relative scale of taxes in money and taxes
in kind, since, as we have shown, the bulk of taxes in kind must have
been sold in the market in the region where the tax was raised. Most
of the money (i.e. net of local administrative costs) raised from the
sale of tax-wheat was then sent out of the province where it was

[55] This argument is raised very briefly by Duncan-Jones 1990: 193–4, but not pursued.

[56] Duncan-Jones' estimate of average revenue in wheat in Egypt (1994: 53) is far more
precise than it deserves to be. He assumes a stable ratio between private (low tax-paying) and
public (high tax-paying) land, whereas his own figures suggest that over time there was a
significant transfer of land from public to private status, itself a reflection of tax-payers' power
against the state. I cite his estimate of wheat revenue from Egypt, therefore, *exempli causa*,
rather than because I think it correct. On land tenure, now see Jane Rowlandson, *Landowners
and Tenants in Roman Egypt* (Oxford, 1996) especially 63ff; she stresses diversity, underplays
chronological change, but still thinks that there was an increase in private (lower-taxed) land,
which was held disproportionately by richer land-owners.

raised to be spent either on the armies stretched along the frontiers,
or where the emperor was, in Rome. So Duncan-Jones' argument has
had the unexpected result of convincing me that taxes in kind were,
macro-economically speaking, even less important than I assumed in
1980.

Micro-economically, it was quite a different matter, and here
Duncan-Jones' objections allow us now to envisage in a more
nuanced way how the Roman tax-economy worked, especially in
under-monetised districts of the empire. Levying taxes in kind
allowed the central government to get revenues even in under-mone-
tised districts and regions, with relatively little trouble. The primitive
natural economies met the monetised economy only at the point
where the tax in kind was sold on the market, e.g. perhaps at a local
port or district capital. There the wheat was consumed and turned
by the workers it fed into marketable and exportable goods, which
were sold in order to recoup the money taxes, which had been sent
off to Rome or the frontier armies.

Let us now deal with Duncan-Jones' remaining and closely related
arguments, that trade in both goods and money was regionally
pocketed, that the economy of the Roman empire was therefore, rela-
tively speaking, unintegrated, and that if there was any growth in
long-distance trade, it was not stimulated by taxation in money.[57]
Once again, we have here detailed problems of evidence, but above
all we face differences in the forms of argument and in general
perspective. In intellectual style, Duncan-Jones is an inductivist; he
likes piling evidence up, so that his conclusions seem to flow from the
pile. But he also has an informing perspective, which follows from
the grand Cambridge tradition of M. I. Finley and C. R. Whittaker.[58]
They are all what I call static minimalists. They stress the static
nature of the ancient economy over one thousand years, and its cellu-
lar self-sufficiency; they think that the distribution of the surplus was
predominantly local, or where it was not, demand was dictated by
the specific needs of government and aristocracy for food, services
and conspicuous consumption. In their view, the Roman economy
was more a command economy than a market economy.

And so, Duncan-Jones' tactic is first to stress the continuity
between the pre-Roman and Roman empires, in order to diminish
the plausibility of there having been any significant economic growth

[57] Duncan-Jones 1990: Chapters 2 and 3; 1994, Chapter 12.

[58] See M. I. Finley, *The Ancient Economy* (London, 1973), and C. R. Whittaker, *Land, City
and Trade in the Roman Empire* (Aldershot, 1993) especially Chapter 12.

in the first two centuries AD.[59] And secondly, since he thinks that conclusions follow unproblematically from the evidence, his tactic is to show that the evidentiary basis for the Hopkins 1980 model is flawed, or that there is contrary evidence which points in a different direction. Two examples will suffice. He argues from the distribution of signed terracotta lamps that trade was regionalised, i.e. there were regional limits to the pattern of trade in terracotta lamps, and different lampmakers had their lamps traded in different regions.[60] Secondly, he argues that coins of a particular type are found to a different extent in different provinces, from which he concludes that 'circulation was mainly local' and that there is little basis in coin finds for arguing that the 'coin of the Principate circulated about the empire through trade-flows'.[61]

My response could proceed on four separable levels: perspectival (what are the merits and deficiencies of our competing models?), empirical (is he right?), deductive (what are the broad implications either way?), and conciliatory (how can these objections be incorporated to help improve the original model?). But rather than pursue each of these arguments *à outrance* [to the end], it seems sensible to remodel the old model, incorporating the genuine advances which Duncan-Jones' latest research suggests, while noting differences of opinion as we proceed.

[59] Almost at random, as illustrations of Duncan-Jones' static minimalism, I quote, 'the world that he (Strabo) describes had not yet been changed by centuries of Roman rule' (1990: 33). For me, Roman rule had indeed radically changed the economy of Italy, and was in the process of gradually evolving the economy of much of the Mediterranean basin. Or, 'Sea-borne trade was already an active feature of the Mediterranean economy in archaic times, as the growing number of wreck-finds clearly shows' (1990: 35). But again the 'evidence' (Strabo, shipwrecks) is used to buttress an argument which does not quite follow; yes, there was sea-borne trade in archaic times. But in later times, there were much bigger ships, and progressively more wrecks have been found from later periods, at least until AD 100/200 (see note 48). As I see it, the scale of sea-borne trade developed.

[60] This is my short version of Duncan-Jones' distinctively obscure arguments. For his version see 1900: Chapter 3. For a crisply dismissive summary of this chapter, see W. V. Harris, *The Inscribed Economy* (Ann Arbor, 1993) 14, who doubts both the truth and the significance of Duncan-Jones' findings.

[61] Duncan-Jones 1994: 178, cf. 1990: 38ff. I do not agree that the evidence which he cites disproves my point. The fact that you find different proportions of different coin types in different provinces seems unproblematic. For example, few coins struck for the empress Matidia have been found in Syria (1.3% of one hoard), but more of the same type have been found in Britain (3.3% and 4% of two hoards – see 1990: 41). To me, this seems of little consequence. Such variations in small samples are completely expectable. My argument was on a different scale. I suggested that in longish periods in largish regions, the volume of coinage was of roughly the same relative order; i.e. when the volume of coinage grew in one region, it grew in others (1980: 113). I did not suppose that each region got exactly the same types of coins.

I then surmised that the *balancing* between regions may have been promoted by the reciprocal flows of taxes and trade. Now that is difficult, both to prove and to disprove (see below), but I did not mean or imply that the payment of money taxes mixed the total money supply thoroughly, like a cook tossing a salad.

THE MODEL REMODELLED

My broad perspective on the Roman economy is mildly developmental.[62] The prime cause of economic growth was the political integration of the Mediterranean basin under Roman rule. Under the impact of Roman conquest, and because of the prolonged peace which Roman rule eventually brought, the Roman empire could achieve modest, though significant, economic growth.

This economic growth came about partly through the diffusion of technical innovations (principally along the axis from the more advanced south-east towards the less developed north-west); I am thinking here of factors such as improved agricultural techniques (different crops and improved animal breeding), but also for example of the increase in the size of ships and improvements in their handling.[63] It is also likely that Roman government and prolonged peace cut transaction costs; for example, the suppression of piracy made peaceful transport across the Mediterranean Sea possible, and so reduced the risks and costs of transport, while the enforcement of law secured relatively predictable and peaceful outcomes to contracts.[64] The mere existence of a single unified state must have enhanced the possibilities and reduced the costs of medium- and long-distance trade. Finally, and you would now expect me to say this, the exaction of taxes and rents, and their expenditure at some distance from where they were raised, elongated the lines of trade, and stimulated an increased division of labour (to produce goods of lower volume and higher value for that trade), and urbanisation.[65] Towns served both as economical locations of production, and as entrepôts for medium- and long-distance trade.

The city of Rome was the prime locus of court and elite expenditure.[66] Emperors, palace officials, aristocrats and hangers on, even

[62] I set out some of these views in the Introduction to Peter Garnsey, K. Hopkins and C. R. Whittaker (eds), *Trade in the Ancient Economy* (London, 1983), and 'Models, Ships and Staples' in P. Garnsey and C. R. Whittaker eds, *Trade and Famine in Classical Antiquity* (Cambridge, 1983) 84ff.

[63] For improvements in shipping, see for example J. Rougé, *Recherches sur l'organisaion du commerce maritime sous l'empire romain* (Paris, 1966) 44ff.

[64] On the importance of reducing transaction costs in economic development, in addition to changes in production and consumption, see Douglas C. North, *Structure and Change in Economic History* (New York, 1981). On trade, see, for example, A. Giardina and A. Schiavone (eds), *Merci, mercati e scambi nel Mediterraneo* (Rome, 1981) especially pp. 55ff.

[65] The impact of conquest by Rome was probably different in the eastern and western Mediterranean basins. In the western basin, the imposition of money taxes forced peasants or local communities into the monetary economy, and helped create new towns. In the eastern Mediterranean, Roman conquest elongated the lines of trade, probably promoted further urban growth and probably enlarged the size of the monetary economy.

[66] I stress the importance of the city of Rome in the development of the Roman empire's

the plebs, spent money in Rome. The money which the emperors and elite spent derived principally from taxes and rents; their expenditure then percolated down to the rest of the population. Everyone benefited from the huge expenditure, whether on the construction of public baths, palaces and temples, or even on the simple redistribution of food, by porters, wholesalers, traders, bakers, cooks and garbage collectors, to and from Rome's teeming population. Rome had a population of about one million people; it was as large as London in 1800, when London was the largest city in the world.[67] Food, clothing, housing, heat, entertainment and occasional luxuries for such a large population, plus the elite's demand for goods and services, made the city of Rome into by far the single largest market in the Roman empire.

The city of Rome acted like an accelerator, increasing the speed of the whole Mediterranean economy. Because of the concentration of spending power and people, prices in Rome were much higher than anywhere else in the empire. Wheat, for example, cost roughly speaking four times as much at Rome as it did in Egypt, three times as much as in inland Asia Minor, and twice as much as it did in the Italian countryside.[68] Rome was at the peak of a pyramid of rising prices. Or put another way, goods were sold in Rome, macro-economically to help debtor provinces pay rents and taxes, but micro-economically because individual traders hoped to make huge profits by selling goods in the highest-priced metropolitan market.

When Rome burnt in AD 64, according to the Revelation of St the ship-merchants who grew rich from her, by bringing and luxuries and staples, wept to see their source of profit van smoke – while the moralist crowed at the destruction of the : harlot of Roman imperial power.[69]

The pyramid of prices, reaching their peak at Rome, must encouraged the development of a *network* of 'feeder' towns a: Rome. Because of cheaper sea-transport, relative to land tran this network of towns, linked to Rome, could as easily be in s ern Spain, or north Africa, as in northern Italy.[70] I use the co *network* to underline the complexities of inter-urban relationsh The term 'feeder' towns perhaps overstresses the interrelatio between the towns surrounding Rome and Rome itself. These t presumably served the needs of their own and their surrour populations, and each other, as well as the metropolis. What keen on here is to avoid understanding the high level of urbanis: in the Roman empire, particularly along the Mediterranean litt as an exclusively cultural and political development. Towns ii ancient economy must also have had an economic underpin: Urbanisation, as I see it, may well have begun from the local ex diture of locally collected rents (rentier towns), but in the first centuries AD, towns developed and benefited from their involver in production for, and trade with, Rome.[72]

The exemplary cases here are the cities of Alexandria, Antioch Carthage, each once the capital of a local empire, supported ju: Rome later was by the expenditure of taxes and elite rents. But u Roman rule, tax expenditure in provincial centres was restric And as we have seen, large amounts of provincial land fell into hands of the central Roman elite. Taxes and large amounts of : were no longer spent locally; they were now transmitted to Ro Nonetheless these old and very large cities (with populations in

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economy much more now than I did in 1980. The growth of the population of the city of Rome occurred mostly in the last century BC. But it seems reasonable to suppose that its elite consumption grew (cf. the growth in aristocratic wealth, note 37 above), even if the city's population remained roughly stable in the first two centuries AD. Since Alexandria, Antioch and Carthage maintained or perhaps even grew in prosperity in the first two centuries AD, even though they were no longer capitals of independent kingdoms, it follows probably that total Gross Product was higher too. And the idea that each of these regions, all bordering the Mediterranean, had pocketed, self-sufficient, cellular mini-economies (Duncan-Jones 1990: 44) seems implausible.

[67] For a wonderful study of London in the English economy from 1650 to 1750, see E. A. Wrigley, 'A Simple Model of London's Importance', in P. Abrams and E. A. Wrigley, eds, *Towns in Societies* (Cambridge, 1978) 215ff. His analysis is extremely suggestive for possible economic developments in Rome, Roman Italy and the Mediterranean basin from 200 BC onwards, especially when allied with Jan de Vries' concept of network. See Jan de Vries, *European Urbanization 1500–1800* (Cambridge, Mass., 1984).

[68] The data on wheat prices to support these broad generalisations are lamentably thin (cf. note 13 above). First-century Egyptian wheat prices were probably below 2 HS per modius. And I myself side rather with Jasny (*Wheat Studies* 20 (1944) 160ff), who estimated wheat prices in the city of Rome, from Pliny's flour price of 12 HS per modius (*HN* 18.90), at 8 HS per modius. Duncan-Jones preferred 6 HS per modius, but properly compromised on a notional normal price for wheat of 6–8 HS per modius at Rome, compared with 2.25 HS in Pisidian Antioch and 4 HS in Italy (single and not greatly reliable prices for each). That is practically all we have. The jurist Gaius noted that prices of wheat, wine and oil differed in various cities, without specifying which (Dig. 13.4.3).

[69] *The Revelation of St John* 18. 11ff lists some of the goods which sea-merchants had to Babylon (= Rome) at the time of the great fire of AD 64: 'And the merchants of the weep and mourn over her, for no man buys their merchandise any more; merchandise of g silver, jewels, pearls and fine linen, purple, silk and scarlet, all kinds of scented wood, i\ articles of costly wood, bronze, iron and marble, cinnamon, spice, incense, myrrh and fr. incense, wine, oil, fine flour and wheat, cattle and sheep, horses and chariots, and slaves.'

[70] On relative transport prices, see Hopkins 1983: 102ff (note 62 above). Roughly speak land transport cost 55 times more than sea transport, and ten times as much as river trans Needless to say, variations were very considerable.

[71] On the importance of urban networks, see Jan de Vries 1984: 10ff (see note 67 abo\

[72] On rentier (consumer) cities, see the classic article by M. I. Finley, 'The Ancient C From Fustel de Coulanges to Max Weber and Beyond', reprinted in his *Economy and Soc in Ancient Greece* (London, 1981) 3ff.

range of 150,000–500,000) continued to flourish. As I see it, these large cities must have been significantly dependent for some of their undoubted wealth and prosperity on the goods which they managed to produce or send on to the largest market, i.e. Rome. The expenditure of taxes and elite rents, above all in the city of Rome, helped encourage a network of feeder towns engaged in the supply of goods, which had to be exported and sold in order to earn back the money with which to pay rents and taxes in subsequent years.

Of course, we do not *know* the total value of the metropolitan market in the first century AD, though I have guessed it as accounting for 800–900 million HS per year, roughly 8% of the whole empire's *actual* GDP. Such figures need to be considered sceptically; they are very rough orders of magnitude, more metaphor than fact, but useful enough to give a sense of scale.[73] That said, our evidence from the Roman world is too sparse to allow any confirmation either of the total, or of any shift say from 6% to 8% of *actual* GDP. But there are straws in the wind.

Three illustrations may help. First, from comparative data, I have estimated the capital value of the ships involved in bringing food and supplies to Rome at 100 million HS. Such a huge total investment, and the sizeable value of each large ship laden with wheat and wine (on arrival at Rome, equal in worth to the minimum fortune of a senator), indicate either that some merchants grew very rich from trade, or that the Roman rich were themselves involved, probably through agents, in financing trade with Rome.[74] Second, a single document, recently published, illustrates the huge scale of the trade in luxuries. We know in general terms from Pliny (*HN* 12.84, cf. 6.10) that Roman trade with the Middle and Far East (Arabia, India, China) was worth 100 million HS per year. This document now makes Pliny's valuation seem an underestimate. On one side it records a contract drawn up in southern India; on the other side it notes the value of six items of a cargo liable to customs duty at Alexandria, including nard from the Ganges, used as a medicinal ointment, ivory and wool; the total value of the cargo was just under seven million

[73] The guesstimate that metropolitan trade with Rome may have amounted to about 8% of the empire's *actual* GDP seems consonant with the estimate by D. H. Perkins for fourteenth century China, that 7–8% of agricultural produce was interregionally traded; see his *Agricultural Development in China 1368–1968* (Chicago, 1969) 115ff. The Mediterranean Sea gave the Roman empire a huge geo-political advantage, and enabled a much greater volume of interregional trade. And of course, a great proportion of the total trade was in higher value non-agricultural goods.

[74] For this estimate of the capital value of ships engaged in supplying Rome, see Hopkins 1983: 102 (see note 62 above). For senatorial interest in profits from commerce, see John H. D'Arms, *Commerce and Social Standing in Ancient Rome* (Cambridge, Mass., 1981).

HS, the minimum qualifying fortune of seven senators.[75] The luxury trade clearly required heavy investment combined with high risk. Finally, a single visit to the city of Rome and its museums should be enough even now to convince anyone of the scale and luxury which it formerly achieved. And most of that luxury was bought with money.[76]

If the concentrated purchasing of Rome was the accelerator of the Roman economy, then the city's magnetic attraction to immigrants and their high mortality served as a brake. The presence of foreigners at Rome was a literary commonplace. Rome beckoned travellers, merchants, preachers, poets, artisans and plain workers from all over the Mediterranean world. Seneca, for example, tells us that people of all sorts migrated to Rome, either to work, or for pleasure, or for education, because Rome offered unparalleled opportunities for pleasure and enrichment. We know from the population history of post-mediaeval European towns that mortality in larger cities is greater than in small towns and the countryside.[77] It seems likely that Rome's experience was similar.[78] The historian Herodian confirms this; during an epidemic, he noted, mortality was highest in Rome, 'because of its very large population, and because it took in immigrants from all over' (1.12). Rome was a huge death-trap, which consumed both goods and people.

This urban immigration and differential mortality brought significant economic benefits to different sub-populations scattered throughout the empire. At least that is what may have happened, if we can apply the experience of London and England in the seventeenth century to Rome.[79] A brief survey of a complex process will

[75] P. Vindob. G. 40822 was published by H. Harrauer and P. J. Sijpestein, 'Ein neues Dokument zu Roms Indienhandel', *Anzeiger der österreichischen Akademie der Wissenschaften, phil.-hist. Klasse* 122 (1985) 124ff. The editors take the the valuation on the reverse to be the total due in customs-charges, but that seems improbable, since then the total value of the cargo would be 35 million HS, including customs payment, which seems too large.

[76] The list of estates given to the Christian church at Rome in the fourth century shows that nearly all rents of the estates, which had previously belonged to aristocrats, were paid in money. It seems unlikely that money rents were a fourth-century innovation. See L. Duchesne ed., *Liber Pontificalis* (Paris, 1886) CL and 34; it is now translated by R. Davis, *The Book of the Pontiffs* (Liverpool, 1989).

[77] Wrigley 1978: 220 (see note 67 above).

[78] Seneca, *Consolation to Helvia* 6: 'Look at the crowds for whom there is scarcely sufficient housing in this huge city; most of the masses come from other places. They have flooded in from their towns and colonies; in fact, from all over the world.' In image, Rome was to the author of *Revelation* a scarlet harlot, bedecked with gold and jewels, drinking the golden cup full of abominations, 'Babylon, the mother of harlots' (17.4ff). For a Jewish author, Rome represented a city of wealth and unsurpassed prosperity: 'The great city of Rome has 365 streets, and in each street there are 365 palaces. Each palace has 365 stories, and each storey contains enough food to feed the whole world' (B. Talmud, *Pesahim* 118b).

[79] Wrigley 1978: 215ff (see note 67 above).

suffice. Emigration to the bigger cities helped drain off surplus population from rural areas. The rural workers who remained had to feed a larger urban population, so their average productivity grew, and in some areas, particularly close to Rome, agriculture became specialised.[80] Supplying Rome created increased employment in the surrounding satellite/feeder towns, both in the production of goods and in trade. And finally, those who migrated only temporarily to Rome, and [who] survived, learnt and took back home some of the cultured tastes which they had learnt in the metropolis. Metropolitan fashions affected taste well beyond the traditional elite, and some quite modest households consumed manufactured goods. Aggregate demand rose, not hugely but visibly. All sectors of the economy, from large cities like Alexandria, to the smaller satellite towns from Ostia to Cadiz, and even villagers and distant farmers, were marginally affected by the demands and expenditures of the central government and elite in Rome.[81]

None or little of all this can be proved or disproved. It is a reconstructive vision of what may have happened. Of course, it can be supportively illustrated, while sceptics can adduce counter-examples. But basically, to choose between static minimalism and local and economically self-contained pockets on the one hand, and on the other hand mild development plus a thin veneer of economic and monetary integration, as basic characteristics of the Roman economy, is not so much a problem of fact and evidence, but rather a problem of preference, of a general sense of how the Roman world worked, and of the balance of probabilities and plausibilities. I myself think that the political integration of the Roman empire brought in its train a gradual integration of the economy, in the sense that all regions of the empire were forced to pay taxes, and induced to pay rents, to a government and to an elite whose expenditure was relatively concentrated. The Mediterranean Sea made transport of a traded surplus between coastal provinces relatively cheap and easy. In my view, therefore, the balance of probability is that the economy was integrated, in the limited sense that a smallish surplus, say about 10% of the actual Gross Product, was traded via a network of towns, centring on, but not exclusively concerned with, the huge metropolis

of Rome, where prices were highest. The main stimuli to that trade were taxes and rents, which were both complements and rivals.

It is no significant objection to this revised model to show that either goods or silver coins circulated locally. Of course, they did. Or rather some did, and some did not. On the whole it must be true that in Roman conditions, local self-sufficiency predominated. Farmers ate much of their own produce; small towns were fed mostly from their immediate hinterland. Per contra, the distribution of wine and oil amphorae illustrates the wide diffusion of trade. As to coins, Howgego has shown, to my mind convincingly, that silver coins did move between provinces in the first two centuries AD, but only gradually. How then was the balance between creditor and debtor (tax-receiving and tax-paying) regions achieved?[82]

The answer is provided, I think, by conceptualising the Roman economy as operating on five intersecting planes: the natural economy, bronze coinage, silver coinage, gold coinage and credit.

1) The natural economy was large, mostly self-sufficient (in the sense that farmers ate a large proportion of their own produce) or consumed locally (after having been paid as rent in kind, or bartered). This broad generalisation holds true, even when we take account of taxes and rents in kind, delivered without any market transaction, sometimes over considerable distances.[83] This command economy became of increasing importance in the late third and early fourth centuries.

The natural economy intersected with the money economy, for example, when peasants brought food to sell in local markets, or when taxes, raised in kind, were sold by the tax-collectors on the open market, because the government needed money much more than it needed wheat. Upper-class large land-owners also needed money more than they needed themselves to consume the produce of their land; the myth of self-sufficiency ('all we eat here comes from my own farms') was a myth.[84]

2) There was a bronze coinage, which for the most part circulated locally. It intersected with the natural economy below, and with the silver economy above, when peasants sold their surplus in exchange for occasional needs, or when peasants, artisans and small shopkeepers bought goods or exchanged a handful of bronze coins for

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[80] Francesco de Martino, *Storia economica di Roma antica* (Firenze, 1979) 229ff, follows Columella (*Rust.* 1, pr. 20) in regretting Italy's lack of self-sufficiency in wheat, and looks for internal causes of agricultural decline. But Ricardo's theory of rents clearly implies that land near Rome would be much too expensive to use for growing wheat.

[81] Dio Chrysostom, *Oration* 11, fantasises about an ideal and secluded peasant, living a life of simplicity and beauty. But even he has his peace shattered by the intrusions of the city, and he has anyhow escaped to the country in order to dodge persecution.

[82] Howgego 1994: 15 and 20 (see note 45 above).

[83] For an illustrative selection of requisitions by government agents, see for example T. Frank, *Economic Survey of Ancient Rome* (Baltimore, 1936) vol. 2 620ff; for an amusing example of long-distance requisition of transport, see Augustine, *City of God* 18.18.

[84] I do not mean that myths of self-sufficiency are completely untrue, only that they do not tell the whole truth. For an instance of this myth, see Petronius, *Satyricon* 38.

silver, in order to pay their rents or taxes. Local money-changers
or tax-collectors charged dearly for this exchange service (see for
example *OGIS* 484). Duncan-Jones estimates the size of this bronze
economy at 5–10% of the total coin stock by value, with more than
5000 million coins of low denomination in circulation in the mid-
second century.[85]

3) There was a huge silver coinage. Under the Republic, silver
coins had dominated the Roman monetary system. But Duncan-
Jones has recently concluded, I think convincingly, that under the
emperors, gold became the dominant coin, in aggregate value (see
below). Even so, silver coin was still very important. Most taxes were
paid in silver coin; and everyday purchases were made in silver or
bronze. After all, a denarius, the most commonly minted silver coin,
worth 4 HS, was more than enough, even in the city of Rome, to feed
a family for a day.[86]

Duncan-Jones himself estimates the total silver coin stock in the
mid-second century at some 7000 million HS. This is based on an
average of only 8000 coins struck per die, and a low loss-rate of
about 1% per year.[87] It is roughly four times my estimate of the stock
of Roman silver coinage in circulation in the middle of the last
century BC, though then Roman coinage did not cover the whole of
the Mediterranean basin. Even so, we apparently both agree that the
volume of the Roman silver coinage expanded.[88] The surplus had
become more monetised.

4) Gold. Duncan-Jones has argued that under the emperors gold
became the most important component by value in the Roman mone-
tary system. This conclusion is based primarily on the relative value
of gold and silver in coin hoards. In coin hoards in Pompeii and in
the rest of the empire, gold coins account for between 70% and 75%
of total value. To be sure, savings may not exactly mirror the mix of
all coins available; hoarders may have selected gold coins preferen-
tially for hideability and portability. But Duncan-Jones corroborates
his conclusion by arguing from the number of coin dies found (or

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[85] Duncan-Jones 1994: 169–70.

[86] Pliny, *HN* 18.90, gives the price of flour at Rome, and *CIL* 4.5380 from Pompeii gives
the price of a substantial loaf of bread at ½ HS (enough for a slave every two days). Even if we
double the Pompeian bread price, to allow for higher prices at Rome, we can still feed a family
of four for a day on less than a denarius.

[87] Duncan-Jones' actual estimate for the total silver coin stock in the second century AD is
6864 million HS (1994: 170).

[88] This compares with my estimate of 400–500 million denarii in circulation in the middle
of the last century BC (1980: 109). But that was based on an average of 30,000 coins struck
per die and an annual loss rate of 2%.

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estimable) for silver and gold coins.[89] Although I have difficulty with,
and doubts about, his detailed figures, the general drift of Duncan-
Jones' arguments seems convincing.

Two general points should be quickly made. First, the priority in
value of gold coin, plus an expansion in the volume of silver coin,
taken together, seem to confirm my mildly developmental perspec-
tive, that under the emperors the Roman economy expanded and
became increasingly monetised.[90] To be sure, the volume of money in
circulation is only one measure of economic activity; ideally, we also
need to know the velocity of circulation. And about velocity, unfor-
tunately we know virtually nothing. But we can assume that gold
coins circulated much more slowly than silver coins. The normal gold
coin of the High Empire, the aureus, was worth 25 denarii (100 HS),
and would have fed a poor family at Rome for a month. Gold was a
prestige currency, used for example for occasional imperial gifts to
soldiers and the plebs at Rome, and reciprocally as a tax to celebrate
a new emperor's accession.

Secondly, of course, the existence of a mass of gold coins in the
currency system helps account for the stolidity in the local circulation
of silver coins. Even if we cautiously downscale Duncan-Jones' extra-
polation from the hoards, from 70–5% for the value of gold in the
total coin stock to say 60% of total value, and if we allocate say 35%
of total value to silver coins, and set bronze at say 5%, then we get

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[89] Duncan-Jones reaches his conclusions by an ingenious series of circular arguments. Coin
hoards show a high ratio of gold (71–4%) by value. There are fewer gold dies known than
silver dies (about 1 or 2:40). Therefore to estimate the total in circulation, while keeping the
ratio of gold to silver, we need to have a higher production of coins per gold die than per silver
die, say 22–43,000 coins per gold die and 8000 per silver reverse die (1994: 164). The ratio
of gold and silver coin production is then corroborated by 1) taking the relative proportion of
coins from each reign a) in hoards and b) in stray finds, 2) correcting the stray find relative
proportions according to the number of years they were in circulation and so available to be
lost (the procedure is worked out in 1994: 113ff). Since these statistical procedures transform
the original data, the consequent global estimates should be treated as only very rough orders
of magnitude. I am convinced by Duncan-Jones' arguments that gold coins in circulation
during the first and second centuries were of considerable value relative to silver. For the
moment, his guesstimate of 70% gold, 30% silver, is the only one available. For the sake of
caution, given the bias towards high value coin in hoards which might not be reflected in all
coins circulating, I think a ratio of 60 gold:40 silver more sensible.

[90] Duncan-Jones 1994: 168–70 boldly calculates the total money stock of the Roman
empire in the mid-second century AD at 7000 million HS in silver coins plus 12,000 million HS
in gold, plus 2000 million HS in bronze (I have rounded his unnecessarily precise figures). So
he values the total money stock at 20,000 million HS. This seems implausibly high, both rela-
tively and absolutely. It is 12 times my estimate of Roman money stock in the middle of the
last century BC (1980: 109), and so incompatible with Duncan-Jones' static minimalism. It is
also absolutely too high. It works out at 330 HS per head of the population – equal to three
times the level of minimum subsistence. Given his view, which I share, that significant sectors
of the rural economy were non-monetised or under-monetised, this staggering total represents
a very high level of liquidity in the monetised sectors of the economy.

a much clearer view of how the Roman monetary system may have worked. Rich Romans, or merchants, or the government, when they wanted to move money from one region to another, as part of the balancing of supply and need, would have found it far more sensible to move small amounts of gold coin than large amounts of silver coin. In the light of Duncan-Jones' radical discovery, the lumpishness or slowness to silver coin circulation really does not matter. For long-distance, high volume/high value trade (such as the trade in luxuries for sale at Rome), gold mattered much more than silver.

5) Finally, credit. When Cicero in the middle of the last century BC wanted to send money to his son at school in Athens, or draw money himself at Ephesus in Asia Minor and at Brindisium in southern Italy, he used the services of his friend and banker Atticus, to arrange for the transfer of credit to a local bank.[91] When minor tax-collectors and even tenant peasants in Roman Egypt had to pay their taxes or rents, they quite often paid their money into a local bank, whether private or public.[92] Finally, Rathbone, in his careful study of a large estate in middle Egypt in the third century, came to the considered conclusion that the rural economy in the Fayum was essentially monetised, but that 'monetisation was not limited by the quantity of coin in circulation, but was extended by the operation of credit arrangements by private banks and the estate itself'.[93] To be sure, we cannot take for granted that Egyptian practices were widespread in the rest of the empire; the economic development of the provinces differed. Nor should we assume that Romans could not do for themselves what Egyptians did regularly. We do not know the scale of credit balancing, but even this skimpy evidence indicates that the volume of coins in circulation was increased by the operation of credit, and that transfers between provinces could be made by balancing credits between bank accounts, as well as by the physical cartage of coins.

SUMMARY

The Roman economy was integrated, as a by-product of the central tax-raising power of the Roman government and the purchasing capacity of the elite, who were concentrated during the first two

[91] Cicero, *Letters to Atticus* 5.13; 12, 24 and 27.
[92] Among numerous examples of payments into local banks for transfer elsewhere: *P. Tebt.* 121 R. 4.12 (AD 42), *P. Tebt.* 235 (AD 41); *P. Oxy.* 309 2 (AD 217).
[93] Dominic Rathbone, *Economic Rationalism in Rural Society in Third Century Egypt* (Cambridge, 1991) 329–30.

centuries AD in the city of Rome. A high proportion of imperial and aristocratic income was spent in Rome. So the city of Rome served as the main motor of economic integration. Looked at another way, conquering Romans turned their political power into economic advantage. Even small peasants in remote villages in the provinces had their lives disrupted by the demands of a distant government for taxes, and by the demands of distant elite landlords for rents. These taxes and rents contributed to the luxury and glitter of the metropolis. The political cost of sharing the profits of empire between government and elite was that the power of the aristocracy to exact rents limited the capacity of the central government to raise taxes.

The very high prices prevailing at Rome enabled merchants to transport and sell there goods which had been produced in the provinces, at lower costs, in and via a network of satellite towns, which themselves in turn prospered, partly because of their involvement in the trade with Rome. These taxes and rents were raised predominantly in money, or if they were raised in kind, they were converted into money by government agents, in the provinces. The reason for the relative unimportance of taxes in kind, or their swift sale in the market, is simple: the government and the elite had limited demand for food; both had a much greater need for money. After all, money provides a generalised capacity to buy unspecified goods at a distance in time and place from where the rents or taxes were originally exacted.

The Roman government in the first two centuries AD produced a much greater volume of coins, by value, than it had produced in the last two centuries BC. This increased coin production was partly in silver, but above all in gold. This increase in the volume of coinage reflected the unification of the coinage system; all coins in use in the Roman world were now minted by the Roman government. But the growth in the volume of coinage was on such a scale, without any corresponding rise in prices, that it seems sensible to think that trade had also grown. The surplus had become more commercialised.

The prime stimulus to this limited economic growth was the exaction of taxes and rents. I can think of no other stimulus with the same force and coverage. The long-term balance between tax-paying and tax-receiving regions was probably achieved not so much by the transfer of silver coins, but rather by the transfer of higher value gold coins and by credit-transfers. All that said, the greatest part of the Roman economy remained local and circumscribed, in that producers, both farmers and artisans, either consumed their own produce or sold the bulk of it locally. The relatively sophisticated, relatively inte-

grated, monetary economy sat on top of this basic natural economy. But it was the relatively sophisticated and integrated sector of the Roman economy which helped the empire maintain its political unity.

PART V

The Nature of the Ancient Economy