

not to be cited

THE SOCIAL SCIENCES AND ROMAN IMPERIALISM 2000

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Here is a small scholarly field called Ancient History, idiosyncratic, very old and consequently a bit sclerotic, formerly erudite but now getting a bit amnesiac, assumed by many outsiders to be senile, moribund or indeed dead. (It's also rather grandiose: it calls itself Ancient History while confining itself to the Greeks and Romans, two among quite a number of ancient peoples). Among those of us who are fascinated by this field, some want it to enjoy a relatively peaceful old age, pursuing its old hobbies, some perhaps -- such as the author of A World Full of Gods -- want to give it a big shock, presumably in the hope of rejuvenating it; still others desire to give it a radical course of hormone therapy -- and the hormones are sometimes meant to come from the social sciences.

When I began to write this paper, my intention was to point out banally that social-science history is to some extent in decline, at least for the time being. Representations, not Social Science History or Comparative Studies in Society and History, has been the journal of the past decade (I hope not to offend anyone -- I have never published in Representations). No doubt many historians would say that we have largely incorporated from the social-science disciplines what is useful to us. And I intended to argue that the sciences which a Graeco-Roman historian can most fruitfully pay attention to nowadays are not social but natural: psychology, earth sciences, or chemistry, for instance. I enjoyed the prospect of bringing to Greek and Roman historians the cruel news that there were still more fields that they needed to master if they wanted to keep up. I have been studying Graeco-Roman anger for some time now, in the actual or metaphorical company of psychologists and sometimes of psychiatrists (my book is called Restraining Rage and it will come out next year). What I really wish I were capable of studying next is something entirely different again -- ancient metallurgy.

So much for fantasy. But my initial intention was deflected when I started concentrating on what has recently been written about Roman imperialism, for that experience rapidly led to the conclusion that we are far indeed from being able to dispense with the social sciences, or far away in any case from being able to dispense with some of the questions they raise.

There is, as all Graeco-Roman historians know, a long history of involvement between various of the social sciences and Ancient History. Some of the founding fathers of the social sciences were of course expert about classical antiquity -- inevitably, one might say, given the educational system of the European elite in the nineteenth century. It's enough to cite the cases of Weber and Durkheim, the latter a student of Numa Fustel de Coulanges [Momigliano, Sesto Contributo 304]. If the year were 1900 not 2000 the social sciences might be an exciting novelty for us: Momigliano pointed out the importance of the year 1897 in the transformation of ancient history, with the publication in the Handwörterbuch der Staatswissenschaften of Max Weber's Agrarverhältnisse im Altertum and other important essays by leading ancient historians of the day.

Still, for much of the twentieth century, it must be said that even the more adventurous kind of Graeco-Roman historian -- not to mention the majority -- tended to be social-scientific in a somewhat spasmodic fashion, rather like those Christians who go to church at easter and christmas but for the rest of the year get on with making their livings and amusing themselves. One may ask, for example, whether that superb scholar Louis Gernet should be described as an anthropologist, as distinct from a socially-conscious historian of Greek law. Or think of Arnaldo Momigliano himself, who declared at one time that the future of Graeco-Roman history lay with sociology and anthropology, but never actually embraced either one. One might even say that he kept those

disciplines at a rather remarkable distance, given some of the problems he was interested in, such as the social structure of archaic Rome. Both these cases raise the question -- which I cannot pursue to-day -- of national scholarly traditions, and in particular the question what the differences are between the correct definitions of sociology, sociologia, Soziologie and so on and so on (those differences are plainly less now than they were two generations ago, thanks to the Americanization of practically everything). It is possible that Momigliano was sometimes practising sociologia even though he was not practising sociology. Or take a third case: there was once upon a time a laudatory paper by the highly-informed Christian Meier about the contribution which Max Weber made to Graeco-Roman history [in M. Losito & P. Schiera, eds., Max Weber e le scienze sociali del suo tempo, 1988] which I remember reading with mild puzzlement because I could not see very clearly the Weberian element in Meier's own work.

Moses Finley's scholarship too stands in a rather odd relationship to the social sciences. I take it that we regard him as the social-science historian par excellence among the classicists of his generation. I consider the supposed personal contact with Karl Polanyi to be something of a red herring here, unless new evidence appears. Moses was originally, I think, a product of a three rather different sets of influences, (1) a New York intellectual and social environment teeming with political and sociological awareness (he later recalled "endless conversation with other students"), (2) an apprenticeship in ancient law with A. Arthur Schiller at Columbia, and (3) the teaching of W.L. Westermann in the Columbia History Department (Westermann was an innovative scholar in his day but was by no means a social scientist). That school (though it was hardly anything as grand as a school) produced at about the same time Naphtali Lewis, Meyer Reinhold and John Day. Its main common interest was in the positivistic reconstruction of ancient economic and social life, above all through the study of documents (and its most enduring monument may be the source-book, Lewis and Reinhold, Roman Civilization). (I call it "positivistic" while recognizing that some of Westermann's students were strongly committed politically).

It was really only in England (and I suspect not at the beginning of his residence there) that Moses applied his voracious reading in economics, political theory, and sociology (among other fields) to the history of the Greeks and Romans. One might take issue with the quite excellent introduction which Richard Saller and Brent Shaw wrote for the collection of Finley's papers entitled Economy and Society in Ancient Greece (1981), in which they seem to date this kind of Finley scholarship back to the 1940s, whereas the work in question really began to come out only in the 1960s. I am not criticizing Finley over this matter; rather I am trying to indicate the genuine novelty of his great work of the 1960s and 1970s.

How has the behaviour of ancient historians with respect to the social sciences changed in recent decades? Which social sciences, one should ask, as well as which ancient historians? There has been a certain amount of anthropologically-oriented work, and a certain amount of demography. To go to the opposite extreme, I don't think I have ever seen a paper by an ancient historian de métier which looked or sounded like a paper by a normal economic historian (nothing wrong with that necessarily). But here I will narrow the scope of this discussion from Graeco-Roman history in general to Roman imperialism, understanding this expression to mean the whole vast history and evolution of Roman power from the fourth century BC all the way through to late antiquity.

In the 1970s two great minds attacked the history of the Roman Empire under the banner of sociology. One was Paul Veyne, whose book Le Pain et le cirque (1976) had the sub-title Sociologie historique d'un pluralisme politique. This may have been Veyne's most brilliant period, when he got Roman sexuality broadly right, in my opinion, in his article "La famille et l'amour" (Annales 1978), and got republican imperialism seriously but interestingly wrong in "Y a-t-il eu un impérialisme romain?" (MEFRA 1975). What are the

salient features of Le Pain et le cirque? A quite exceptional knowledge of the textual sources, for one thing (not for Veyne the sophomoric position that since the sources for Ancient History are poor you can ignore them and make up any story you like and propound it as history). As for sociology, the sub-title of Le Pain et le cirque was either confused or ironical, for the Veyne of that time sharply distinguished history from science and in another book selected sociology for a special attack (those of you who know his work will not be surprised to hear that I once heard Paul Veyne explaining how his great ambition as a student in the years after the Second World War was not to become an annaliste). In fact Veyne loves, or at any rate loved, sociology -- conceived as a French scholar might conceive in the 1970s; his book even has an "index sociologique", in which there appear terms such as "authority", "plurifunctionality", "symbolism"; he also attempts a kind of economic analysis. But he proceeds even more by intuition and a desire to contradict. Not for him the explanation of evergetisme as a necessary means of keeping the masses in a docile state (an explanation which the Romans themselves occasionally gave): rather, it was a matter of self-display on the part of the donor and a demonstration of his superiority. But the procedure used for reaching this conclusion necessarily leaves anyone with a social-scientific cast of mind gasping for breath. The last thing the author had in mind was a reasoned testing of alternative models.

The other person who conspicuously marched under the sociological banner in those days was Keith Hopkins, whose Conquerors and Slaves, published in 1978, was really the first intelligent book in English about Roman imperialism; it was sub-titled Sociological Studies in Roman History I. On the surface, at least, the approach could not be more different from Veyne's: "one objective is to experiment with methods borrowed from sociology in order to gain new insights into changes in Roman society.... I try to explore some of the long-term consequences of repeated actions". "Sociological methods" will help us to choose between competing historical explanations: "that is why these two books [that is, this one, together with Death and Renewal, 1983] make use of sociological concepts and arguments, set out explicit hypotheses, and seek to support those arguments with models, figures and coordinates"; but the purpose is perhaps more anthropological than sociological -- "to reveal how Romans thought". What Conquerors and Slaves actually achieves, I think, is less a matter of revealing how Romans thought than of making sense for a modern reader out of some of the most problematical sets of events in the growth of the Roman Empire. The technique is highly Finleyan: with the aid of comparative history, one tries to make sense, above all sociological sense, out of a large set of historical events.

But I should move on to recent work. And here I note that most of the questions taken up in the rest of this paper concern what I propose to call Rome's Second Imperialism, that is to say Rome's exercise of power in the period after 9 A.D. when the wars of expansion decelerated. But these questions also apply to republican times as well, and we shall need to range backwards and forwards chronologically. The central questions in much of this recent work, and in the rest of this paper, are about acculturation -- to what extent did Romanness in various forms supplant local cultures? Was the Roman Empire deeply or only superficially Roman? But also, what did Romanness consist of? K. Hopkins, I believe, will answer these questions in the following paper. I undertake the more odious business of criticizing the efforts of recent contributors.

I am going to concern myself with four books, Dialogues in Roman Imperialism, edited by David Mattingly (1997), Frontier and Society in Roman North Africa by David Cherry (1998), Greg Woolf's Becoming Roman; the Origins of Provincial Civilization in Gaul (1998), and finally The Corrupting Sea (2000), the joint production of Peregrine Horden and Nicholas Purcell. These are works of very varying quality, and the last of them is not even about Roman imperialism, but they all have some important bearing on the nature of the Roman Empire. They all form part of a more general trend towards writing thoughtful books about provinces or regions of the Roman Empire, books that

aim not so much at catalogues of information (morasses of facts, in Finley's expression) but at historical understanding; I could list at least half a dozen other examples from the 1990s.

Dialogues of Roman Imperialism I at first greeted with pleasure because it brought a much-needed archaeological perspective to the subject. Nor do I particularly object to reverent invocation of post-colonial theory -- though this procedure can by now produce an extreme déjà-vu effect -- it sometimes sounds as if archaeologists have discovered, seventy or eighty years after the rest of the intellectual world, and twenty-five years after the Vietnam War, that there is a lot to be said against imperialism. The editor harps on an entirely fictitious "consensus model" view of the Roman Empire as a benevolent and beneficent regime (no scholar more recent than F. Haverfield is cited as holding this view; he died in 1919), and sets against it an equally fictitious "new" understanding of Roman cultural arrogance and highly imperfect Romanization. The focus of the book is on Roman Britain, which is no doubt one reason why Roman imperialism emerges from it as unmitigatedly imperialistic -- the imposition of exterior power on a subject population whose reactions can scarcely be reconstructed and are therefore liable to be conjectured on the basis of post-colonial theory. In fact there are some very useful pieces in this collection, and one of the archaeological contributors frames an important problem quite well: it is too simple, this scholar says (R. Hinkley, p.86)

to suppose that the adoption of 'Roman' material culture, whether pottery or a villa, necessarily symbolized a wish on the part of the adoptee [the author means "adopter"] to create some form of abstract Roman social identity.

He is tempted to see in the fact the Romano-Britons eventually gave up building towns with strictly orthogonal street-plans a certain taking of distance from the imperial power. But he has no ideas about how to discover the population's "social identity" (by which I think he means "national identity", in so far as such a thing existed in the ancient world).

What has been learned from the social sciences by most of the authors of this book is a simple but valuable lesson, which is to go easy, when one is writing the history of Roman imperialism, on the contrast between "primitive" and "civilized" (but of course they may just have learned that from the pages of a liberal newspaper). However none of the authors seeks to compare the viability of different models of Romanization (the main subject of the book), which would be an elementary social-science procedure and might have produced some desirable clarity.

David Cherry's book, though it is called Frontier and Society in Roman North Africa, is not about Roman North Africa in general, but about a specific semi-arid, pre-desert frontier zone running east-west across modern Algeria. (There is an unfortunate confusion within the book which sometimes leads the author to make claims about "Roman North Africa" even though he knows in theory that his material comes from a zone which was quite different -- ecologically, economically, culturally and perhaps demographically -- from the coastal zone). Romanization is once again the central topic. Cherry concludes, as Robert Broughton concluded in 1929 [The Romanization of Africa Proconsularis], that in this region there was not very much of it.

Once again this is a traditional p.c. sort of book which is against colonialism -- as if an American academic writing in the 1990s had any choice in the matter. There was not much Romanization in this region, according to Cherry, but there was not much resistance to Romanization either, because the Roman government and elite were not interested, the author says, in trying to bring about Romanization, at least not here. Most of the evidence adduced in material or epigraphical. The author struggles hard to avoid attributing any significance to the fact that much of the local population adopted a material existence which was Roman. When the members of the local elites built Roman villas, they may well have done so merely because of "ease, comfort, or

expediency", not because they wanted to be Roman -- but, we might reply, if this was their idea of "ease, comfort, [and] expediency", they look suspiciously like Romans, all the more so because there is no evidence that the elite rejected Romanization. The poor, Cherry insists, must have been anti-Roman, even though there is no evidence to this effect ("the poor... really had nothing to gain by giving up their customs", 159). Likewise Cherry strives to deny that the epigraphically-attested adoption of Roman deities amounts to Romanization, and to show that the Roman army, which has of course traditionally been regarded as a major source of Romanization, had much to do with the locals (not much intermarriage, he says). Their liaisons (not official marriages in Roman law) were with women with Latin names -- but surely these women were local and as far as nomenclature was concerned Romanized (and presumably they spoke some Latin, though colonialist soldiers do not necessarily speak to their wives or girl-friends in the imperial language).

Cherry's book built on sand, since it presents no notion of what Romanization might have amounted to (identity, language change, altered moral values, altered religious concepts and practices, new attitudes towards the body, contact with a wider economy, new roles for writing, etc., etc.), let alone alternative models of how it might have worked. I have no doubt that the author is right to suppose that the mass of the population was imperfectly Romanized in upcountry Algeria. But there is no attempt here to exploit the indications that do exist, or the comparative history. The social sciences are absent, but so is the most basic Roman history. We are told, for instance, that the Roman were uninterested in Romanizing Mauretania or Numidia -- but surely we know something in fact about Roman views and ambitions with respect to the provinces.

Which leads me to observe that it's a general weakness of the first three of these four books that they fail to realize fully how Romanization changed. What most stirs my interest is the historical process -- historical causes running in all directions like bits of mercury, and the significant differences of outlook that distinguish one decade (or let's say one century, for this is Roman history after all) from another.

Becoming Roman, Greg Woolf's study of Romanization in Gaul, is an altogether better-executed and more subtle work than the book I have just been talking about. It stands out in fact as one of the most serious and worthwhile books ever written about a Roman province or group of provinces.

Some people seem to think that Woolf rejects in this book the concept "Roman imperialism" (<http://ccat.upenn.edu/bmcr/>), but such is not the case -- he simply and quite rightly wants us to recognize that there have been many different imperialisms in world-history -- not a point that many non-Stalinist scholars would be inclined to deny. For Woolf, Romanization is not a simple or an unchanging process, but rather develops under the emperors into a Roman desire -- that is to say, a desire felt by the ruling Roman elite -- to spread humanitas (very approximately = "civilization") to the indigenous peoples; this was a conception of the Empire utterly different from the triumphalist attitude of Republican times. However, the incorporation of Gaul into the empire altered the social structure of the conquered population, increasing social differentiation as the well-to-do survivors learned to make the most of the prosperity of the wider Roman Empire; the new "Gallo-Roman" aristocracy controlled the lower orders more strictly than the pre-Roman aristocracies had done. They were able to do this because they learned how to make use of Roman culture; but the process of Romanization nonetheless affected the whole population.

Becoming Roman is an admirable book in a number of ways: in particular, the author has apparently read almost everything, and he is willing to take on large historical issues. I have two main criticisms: one is that Woolf did not in the end recreate the least Romanized elements in the population, a matter to which I shall return shortly. The other is that even he is too synchronic: he seems to think, for example that the passage in Pliny's Natural History

(iii.39) where he talks about Italy's having been chosen by the gods to "unite scattered empires, to make manners gentle, to draw together in converse by community of language the jarring and uncouth tongues of so many nations, to give mankind civilization (humanitatem homini dare), and in a word to become throughout the world the single fatherland of all peoples", that this passage could just as well have been written in Augustan times (Augustus is heavily emphasized). Possibly so -- after all, there is a lot we do not know about the thinking of the Roman elite. But in my view a very important development had taken place between the 70s of the first century BC and the 70s of the first century AD. In 60/59 BC you can already hear Cicero preaching about the desirability of showing humanitas to the provincials, but that is obviously not a missioné ducatrice. Nor is such a thing to be found under Augustus (to my surprise -- excuse the classical digression -- he misquotes and mistranslates the crucial text of Augustan imperial ideology, namely Vergil, Aeneid vi.852, saying that Jupiter instructs the Romans "to impose morality" [on the conquered peoples]; what Vergil actually wrote was "pacique imponere morem", which means "to impose order on peace" or better "on the pacified world", which might be vague enough to include a program of cultural Romanization but does not necessarily do so).

But Woolf's work is very good indeed as far as the "Gallo- Roman aristocrats" are concerned, and his chapter on their consumptions is thoroughly versed in recent debates about the anthropology of consuming (Ch.7: "Consuming Rome"). In fact we can recognize in this work a historical author who really has succeeded in integrating some social sciences into Ancient History.

Horden & Purcell's Corrupting Sea would be difficult to describe briefly and not just because it is 761 pages long (that's what has appeared so far, i.e. volume I). This is not, as I said, a book about the Roman Empire in any direct way; one might describe it for the sake of those who haven't read it as a sort of latter-day attempt to do for the ancient and mediaeval Mediterranean what Braudel did for the Mediterranean in the age of Philip II. But there are profound differences from Braudel -- quite apart from the huge time-span (3000 years or so?), which constantly threatens to make the work disintegrate. Horden & Purcell attack even more big themes than Braudel did, including Mediterranean religion and Mediterranean social anthropology (lots about honour and shame). The authors do not attempt to bring out what the implications of this work might be for the history of Roman imperialism. There seems to be an implication that it did not produce any profound changes, everything in the economic system being determined (more or less) by the Mediterranean ecosystem (I am somewhat travestyng a work which is never content with straightforward formulations).

Now, Horden and Purcell are a brilliant pair, and it may be that the book achieves precisely what they desire. But it could be argued that a more social-sciency approach could have helped them. Prima facie, after all, there was at least one very big economic shift within Graeco-Roman history (leaving aside what happened at the beginning of the Iron Age and what happened in the late Roman Empire) namely a period in which the Greeks and later the Romans created a much more sophisticated trade economy, with gigantically larger capital and a much large slave labour force. The real beginnings of this are to be found in fourth-century Greece, and Rome joins in, I think, from the time of the First Punic War (264- 241 BC) onwards - some people would say from about 200 BC. Whether this change produced sustained economic growth over subsequent centuries, or merely great concentrations of individual wealth, is one of the central questions of Ancient History, which I trust K. Hopkins will answer. My point is that Horden and Purcell should have considered the viability of this model I have begun to outline. They get near to doing so when they criticize (146f.) what Hopkins has baptized the "static minimalist" view of the Graeco-Roman economy, the view (formerly and perhaps even now) very popular in Cambridge, to the effect that the system was static, dominated by subsistence agriculture, and lacking in both the institutional and the psychological tools it would have needed to become anything else. Transport

was expensive, long-distance trade was largely confined to luxuries that were consumed by a minute class of the privileged. Productive technology seldom improved. Horden & Purcell reject this model, but they are inhibited by their belief in continuities from putting forward a historical alternative.

These four books present some sharp contrasts, including contrasting views of the Roman Empire. Can we account for that simply by reference to the different regions which different scholars chose to study? Is it the differences between Roman Britain, Roman Gaul, the African limes and (in the case of Horden & Purcell) the whole Mediterranean that produce these divergent views of what the Roman Empire was like? In part, the answer is obviously yes, and indeed most of the time the scholars I have been discussing keep well in mind that they are writing about particular regions (not always: the Roman Britain experts seem to be most willing to generalize about the Roman Empire on the basis of their single province -- paradoxically, since in a variety of ways Britain was an exceptionally backward province). Woolf is acutely aware of the problem of internal differences in Romanization within the Gallic provinces, but rather suprisingly does not attempt any comparison with what happened in other parts of the Roman Empire.

Now I turn, penultimately, to three problems which the current literature about the Roman Empire does not discuss in the manner I would wish. First of all, social structure. None of the authors I have been discussing is satisfying on this, though Greg Woolf has things to say about the Gallo-Roman elite. In the standard model of Roman imperialism, as I understand it, Roman power allies itself with available members of local ruling elites, before and during the conquest if possible and in any event when the fighting is over. During the period of conquest, Rome often seizes land as well as other assets; thereafter, however, it is respectful of those who have property rights, just as it is hostile to popular power.

There is a great deal more that can be said. Roman power had the effect of altering local social structures. For one thing, it created economic opportunities (whether it created growth or not). There was access to new markets (for a wide range of commodities), there was new infrastructure (roads, harbours, urban facilities of various kinds). Slave-labour became available on a scale seldom seen previously in any of the conquered territories. So there were new concentrations of wealth. Urbanization too obviously tended to change the social structure. A few regions, Achaea for instance, were already as urbanized when Roman power arrived as they were ever going to be in antiquity. And even in the places where Roman power created most new towns, North Africa for instance, the mass of the population remained rural. But wherever Roman urbanization took place, it had multiple and in some ways profound effects -- among other things, it facilitated both education and luxurious forms of lifestyle, hence raising the barriers between social classes still further, and it encouraged occupational differentiation and hence the emergence of a kind of middle class, there being of course a particular Roman mechanism for doing that, namely the manumission of slaves.

To say how far these changes went, and with what local variations, I take to be one of the main jobs of a Roman historian who studies the provinces or a province. But what are the conceptual tools which we should be using in analysing these social changes? There has been deplorably little discussion of this problem. There is a generalized tendency to speak of "elites" and of people who were "non-elite"; a few quantitatively-disposed scholars, including me, have tried to employ Gini coefficients to measure inequalities in the distribution of property ownership, but the incompleteness of even the best sets of Roman data, combined with the difficulty of finding the right non-Roman comparanda, have so far prevented us from achieving any interesting results. Anyway social class is not mainly about the Gini coefficients of land ownership.

What is it about, in the Roman world? Ancient historians are, I think, still living under the shadow of a single page of Finley's Ancient Economy,

where he attempted to demonstrate the inapplicability (to the ancient world) of the language of class -- not at all a good page, I think. I once wrote a paper on this subject myself, a paper so unsuccessful that not even my own students read it now. Finley's attempt to return us to the language of ordines -- an ideological construct if ever there was one -- was a cop-out. To put the matter very tersely, I advocate that we should return self-consciously to the language of classes, though not classes as re-defined by Marx. This is a matter inadequately dealt with in current accounts of Roman education, housing, marriage patterns -- just to mention three large topics, and it is missing from most accounts of provincial Romanization. Woolf (81) writes a propos of the Latin inscriptions of Gaul that

the inscribing classes included many more than simply the decuriones and seviri Augustales of the cities, but many fewer than the total population, and so indicate how Roman power and Roman culture operated well beyond whatever narrow group we might designate as 'the provincial elite'. To some extent the creation of these intermediate classes was itself a consequence of Roman rule, since there is little evidence for them in the iron age....

But who were "these intermediate classes" -- intermediate between what and what? Later in the same chapter Woolf writes about the local "aristocracies" of the Gallo-Roman cities, also an elusive expression in this context. No need to go on criticizing Woolf, since he is far better than most, but any discussion of Romanization cries out, I think, for a specific analysis along class-lines.

The situation which Woolf uncovers in at least some parts of Gaul is in fact a quite fascinating one in which the some of the wealthy families (only to be called "aristocratic" in quotation marks, I think) were of immigrant Roman descent but others were of local descent, while the smaller farmers could also be of one type or the other. The people who are too inconspicuous in Woolf's account -- because they are inconspicuous in the epigraphical record -- are the free peasants and the landless poor -- between them the bulk of the population. But his towns are also hard to understand, because he is a bit of a "static minimalist", and suggests that artisans were few. How does he think that the town-dwellers of Narbo and Lugdunum made their livings? It is not at all clear to me.

To turn to another problem. Something most strange about much (not all) of the recent literature about the provinces is a certain indifference to local cultures. One encounters some more or less windy talk about the mutual acculturation of Romans and indigenous populations, but there is less discussion than there ought to be of Gauls, "Libyans", even Britons (I know there is a whole industry devoted to studying the pre-Roman peoples of Britain). Even Woolf, who is greatly concerned with Latin epigraphy, has relatively little to say about the few surviving Celtic inscriptions of Gaul (barely more than 200, including graffiti) and nothing at all about the language change. The small number of such inscriptions should not obscure the fact that in the first century AD, a century and more after the Roman conquest, Celtic was still in very widespread use in most parts of Gallia Comata -- unless the comparative evidence from other parts of the Roman Empire is for some reason grossly misleading.

In fact we seem to be slipping back from the position which I thought we had already reached ten or twenty years ago of realizing that in most parts of the Roman Empire local cultures persisted over many centuries. As I think I hinted, I do not share the national-chauvinistic desire of some scholars to glorify the resistance of their supposed ancestors in the face of the imperial awfulness of the Romans. But the evidence for language-survival requires our attention here. Do I have to recite it again? When Paul of Tarsus visited Lystra in Lycaonia, in Asia Minor, he was visiting a place which has left us exclusively Latin and Greek inscriptions, mostly Greek. Not a hint of the local language. But the author of Acts tells us that what people spoke in the city was Lycaonian, and a mountain of parallel evidence about other places

makes it credible that that was a very widespread pattern. Here I have to add that this all becomes much easier to understand if you accept the theses I put forward in my book Ancient Literacy about the (by modern standards) low rates of literacy that prevailed in most parts of the Roman Empire.

We ought to set up a series of possible models for acculturation in the Roman Empire (but it will be no use doing that if we ignore the illiterate masses). They need to be fairly complex because they need to take into account, among other things, the possibility that language change, the easiest aspect of the matter to trace, did not correspond with a sense of national identity. And identity is needless to say a polymorphous concept. How many "national" identity labels could an individual have at the same time in the Roman world?

Yet another important matter which I find somewhat missing in the recent literature about the particular provinces and regions of the Roman Empire is a sense of them as elements in an economic system (this is obviously not a complaint which can be directed against Horden & Purcell). There may be special reason: one is that Roman Britain really was rather marginal to the Roman economy (is any Roman senator known to have owned estates in Britain before the fourth century AD?), another is that there is a diehard breed of British scholar that still believes in the "static minimalist" view of Roman economic history, the canonical text of which is Finley's Ancient Economy. Since such scholars scarcely believe that there was any such thing as the Roman imperial economy, they cannot be blamed for failing to study how particular provinces or regions fitted into it.

But for the rest of this is a vital matter. If you accept anything like what I might call a Pleket model of the Roman economy [H.W. Pleket, "Wirtschaft und Gesellschaft des Imperium Romanum. 2. Wirtschaft", in W. Fischer, J.A. van Houtte, etc. (eds.), Handbuch der europäischen Wirtschafts- und Sozialgeschichte I (Stuttgart, 1990)] -- in other words a relatively sophisticated, dynamic and trade-oriented model, long-distance exchange has to be fitted into some essential sectors, including those that give much of their distinctive character to Roman cities, such as metals, slaves and imported materials for paints, dyes, food-consumption, cosmetics, etc., etc.

How these matters looked at the other end of the Mediterranean I might suggest by quoting Stephen Mitchell (Anatolia I [1992], 241):

The appearance of Anatolia changed fundamentally between the second century BC and the second century AD. Cities took the place of fortified refuges; settled village populations superseded transhumant groups; cereal agriculture spread to areas previously devoted to pasture and stock-raising [he goes on to talk of demographic expansion, among other matters].... Cities were organized on a Graeco-Roman model and thus produced a new form of society, which was readily adopted by the indigenous population, and which enabled them to join a cultural and political system which now covered the whole Mediterranean basin.

I understand the desire to work on acculturation and the nature of the Roman Empire from the vantage-point of a benighted outpost on Hadrian's Wall or the fringes of the Sahara, but we may be getting a bit too centrifugal. A proper account of such matters needs to take in the prosperous freedmen of a hundred Roman ports (and not only ports), men who began in slavery and as a result of fortunate dealings in the complex economy of the Roman world emerged with money and a sort of honour. Roman history-writing requires us to balance the two perspectives.