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The Ancient Historian and his Sources[1]

In an account of the evidence for the history of Mesopotamian social organization, Joan Oates introduced her second section, from about 3100 BC, with the simple statement, 'Our knowledge of social structure in Mesopotamia increases exponentially with the invention of writing'.[2] To anyone not involved in the study, it then comes as a shock to discover that this exponential increase in information is not unambiguously welcomed, at least not in the last decade or two, following the emergence of the 'new archaeology'. The happy days are gone when historians of antiquity (whether Near Eastern or Graeco-Roman) could relegate archaeology to a minor ancillary activity that produced picturesque information about private life and art with which to dress up the 'real' history derived from written evidence. The ancient historian today has to accept that his armoury includes qualitatively different kinds of evidence which often appear mutually contradictory or at least unrelated.

What is to be done? I believe that the nature and uses of the evidence about antiquity are being debated more widely and determinedly today than at any time since, say, the days of Boeckh and Niebuhr in the early nineteenth century. Partly this is a consequence of the exponential increase in the quantity of available archaeological information and in the quantity of publication generally in ancient studies, and partly it reflects new approaches to the study of history, new interests and the formulation of new questions. The discussion is wholly to be welcomed in principle, though in practice too much of it reads like a trades union demarcation dispute.

I begin with a point so elementary that it borders on the commonplace. In the words of Momigliano writing about literary, but non-documentary, sources: 'The whole modern method

of historical research is founded upon the distinction between original and derivative authorities . . . We praise the original authorities – or sources – for being reliable, but we praise non-contemporary historians – or derivative authorities – for displaying sound judgment in the interpretation and evaluation of the original sources. This distinction . . . became the common patrimony of historical research only in the late seventeenth century.'[3]

The last sentence is critical: not only mediaeval historians and pre-eighteenth-century modern historians paid little attention to the distinction between primary and derivative sources; so did the historians in antiquity. A few, notably Herodotus and Thucydides, distinguished between eye-witnesses who could be carefully cross-questioned and all later testimony that was beyond such personal control,[4] but they failed to develop techniques of source criticism or ways of dealing satisfactorily with derivative authorities. Of course any idiot could have differentiated between a primary and a secondary source, and also between a careful writer and a charlatan; and most historians in antiquity, even the weaker ones, were not idiots. Yet a Livy or a Plutarch cheerfully repeated pages upon pages of earlier accounts over which they neither had nor sought any control. Something other than intelligence was involved, which in the end must come down to a radically different notion from ours of the nature and purpose of the historical exercise. Only Thucydides fully and systematically acknowledged the existence of a dilemma, which he resolved in the unsatisfactory way of refusing to deal with pre-contemporary history at all.[5]

The modern historian of antiquity cannot simply repeat the ancient practice. He cannot write a history of Rome by reworking in a modern language the Latin of Livy as Livy had paraphrased or translated the Greek of Polybius. The 'common patrimony of historical research' that arose at the end of the seventeenth century has made that procedure unacceptable. But that patrimony, it must be added, seems not to interfere seriously with the practice of 'rescuing' Livy and the rest by rewriting their accounts, rather than just repeating or paraphrasing; a rewriting that ends by tacitly accepting the essential veracity of the original.

Unfortunately, the two longest ancient accounts of Roman Republican history, the area in which the problems are currently the most acute and the most widely discussed, the histories of Livy and of Dionysius of Halicarnassus, were composed about 500 years (in very round numbers) later than the traditional date for the founding of the Republic, 200 years from the defeat of Hannibal. Try as we may, we cannot trace any of their written sources back beyond about 300 BC, and mostly not further than to the age of Marius and Sulla. Yet the early centuries of the Republic and the still earlier centuries that preceded it are narrated in detail in Livy and Dionysius of Halicarnassus. Where did they find their information? No matter how many older statements we can either document or posit – irrespective of possible reliability – we eventually reach a void. But ancient writers, like historians ever since, could not tolerate a void, and they filled it in one way or another, ultimately by pure invention.

The ability of the ancients to invent and their capacity to believe are persistently underestimated.[6] How else could they have filled the blatant gaps in their knowledge once erudite antiquarians had observed that centuries had elapsed between the destruction of Troy and the 'foundation' of Rome, other than by inventing an Alban king-list to bridge the gap? Or how could they contest an existing account other than by offering an alternative, for example, to provide ideological support for, or hostility to, a particular ethnic group, such as Etruscans or Sabines, who played a major role in early Roman history?[7] No wonder that, even in the hopelessly fragmentary state of the surviving material on early Rome, there is a bewildering variety of versions, a variety that continued to increase and multiply as late as the early Principate.[8]

Presumably no one today believes the *Alban* king-list to be anything but a fiction, but any suggestion that there is insufficient ground to give credence to the *Roman* king-list is greeted with outraged cries of 'hyper-criticism' and 'shades of Ettore Pais'. Such epithets do not meet the issues. To begin with, a 250-year period occupied continuously by only seven kings is a demographic improbability, perhaps an impossibility: the first seven emperors under the Principate reigned for a total of one

hundred years. Then, to conclude about the second king, Numa Pompilius, that the 'only historical fact' about him is his name and that his biography is 'legendary',[9] is effectively to remove one of the seven from the record. And so on almost *ad infinitum*: it is our incurable weakness that we completely and absolutely lack primary literary sources for Roman history down to about 300 BC and that we have very few available to us for another century. So did Livy and the other later Roman writers (apart from a handful of miscellaneous and often unintelligible documents).

That is unchallengeable as a simple matter of definition. It is then a strange aberration when a reputable Roman historian, writing the volume on the early Romans and Etruscans (down to 390 BC), in a series edited by an equally reputable colleague, prints an appendix headed 'primary sources' which consists of thumbnail sketches in four to ten lines each of a dozen writers, ranging in time from Timaeus, whose long career spanned the end of the fourth century BC and the first half of the third, to Festus, who flourished about AD 150.[10] I cannot imagine that, even as a slip, a Renaissance historian would compile a list of primary sources made up of John Addington Symonds, Burckhardt and Chabod. I suspect that Ogilvie's slip reflects, no doubt unconsciously, the widespread sentiment that anything written in Greek or Latin is somehow privileged, exempt from the normal canons of evaluation.

The insufficiency of primary literary sources is a continuing curse. If it looms largest in the study of the archaic, more or less preliterary, periods of Greek and Roman history, that is only because those are the periods for which archaeological evidence is currently dominating the learned discussions. In fact, the lack of primary literary sources bedevils Greek history altogether after the death of Xenophon in the mid-fourth century BC, the whole of the history of the Hellenistic East, important periods of the history of the Roman Republic and of the Principate, including most of the history of the Roman provinces. For example, for the long reign of Augustus the only primary sources, other than documents, are half a book of the naive, superficial history by

Velleius Paterculus, some letters and speeches of Cicero for the early years, Augustus' own account of his stewardship, the *Res gestae*, a model of disingenuousness, and the Augustan poets. The only systematic account that survives is that of Dio Cassius, written near the middle of the third century. Dio used primary sources, to be sure, but our unavoidable reliance on his version is obviously unsatisfactory, as is the dependence on Virgil and Horace for much of the ideology of Augustus and his friends and supporters.

Nor is the situation significantly altered by introducing into the discussion written documents. Numerous as they may seem to be, they constitute a random selection in both time and place, and they often lack a meaningful context. It is hard to exaggerate. I cannot think of an ancient city, region or 'country', or of an institution (with two related exceptions to which I shall return in a moment), of which it is possible to write a systematic history over a substantial period of time. Some individual incidents can be presented historically, perhaps even something of the scale of Caesar's conquest of Gaul, but nothing beyond that. That is the unhappy consequence of our shortage of primary *historical* sources. Unless something is captured in a more or less contemporary historical account, the narrative is lost for all time regardless of how many inscriptions or papyri may be discovered. It is enough to point to the history of Athens and the Athenian empire in the nearly fifty years between the Persian and Peloponnesian wars, a period rich in epigraphical evidence but one for which Thucydides chose not to write a systematic account. We cannot even date some of the battles that Thucydides obviously thought important.

The exceptions are on the one hand in the history of ideas, specifically in the history of philosophy and science, of rhetoric, poetry and historiography, and on the other hand in the history of art and technology. In the former the distinction between literary and documentary sources loses most of its significance; in the latter, the 'documentary sources' are the objects themselves. There are serious gaps, too well known to need enumeration, and there are other difficulties, arising for instance from our uncer-

tainty about the adequacy of the surviving samples; yet reasonable histories have been written of those subjects. Otherwise, the lack of primary sources for long stretches of time and for most regions of the Mediterranean creates a block not only for a narrative but also for the analysis of institutions. There are periods and places about which we have considerable knowledge, not only of the institutions but also of the detailed narrative of political history, of war and diplomacy, of the process of government, and so on – Athens in the latter part of the fifth century and much of the fourth century BC, the last century of the Roman Republic and the first two centuries of the Empire. However, this happy situation should not blind us to the inadequacy, often to the hopelessness, of the available evidence for the rest of Greece outside Athens, for the Roman Republic before the Gracchi, for most of the Roman provinces most of the time. Even for political history in the periods we know most about there are bad gaps, as I was compelled to confess repeatedly in my *Politics in the Ancient World*: it is enough to mention our fundamental ignorance of the way the *comitia tributa* worked, and that was the main legislative organ of the Roman Republic from early in the third century BC and the arena in which the tribunes operated. A complicating factor that reinforces the negative picture I am drawing is the random nature of the documentation that has come down to us, largely disconnected material detached from a larger context, illustrative but neither serial nor synoptic.

It is in the end not very surprising that university students of history, with some knowledge of the sources for, say, Tudor England or Louis XIV's France, find ancient history a 'funny kind of history'. The unavoidable reliance on the poems of Horace for Augustan ideology, or in the same way on the *Eumenides* of Aeschylus for the critical moment in Athenian history when the step was taken towards what we know as Periclean democracy, helps explain the appellative 'funny'. But the oddities are much more far-reaching, extending to the historians themselves in antiquity, in particular to two of their most pervasive characteristics, namely, the extensive direct quotation from speeches and the paucity of reference to (let alone quotation from) actual

documents, public or private. The speeches are to us an extraordinary phenomenon and they produce extraordinary reactions among modern commentators. We have no good reason for taking the speeches to be anything but inventions by the historians, not only in their precise wording but also in their substance. Certainly that is how they were understood in antiquity: witness the discussion in his long essay on Thucydides (ch. 34–48) by Dionysius of Halicarnassus, the most acute and most learned of ancient critics and himself a prolific composer of speeches for his multi-volume *Roman Antiquities*.

Modern writers find themselves in difficulties. Not only does the position of a Dionysius of Halicarnassus seem immoral – it has been said that one would have to regard Thucydides as 'blind or dishonest'[11] – but, worse still, one must consider seriously abandoning some of the most interesting and seductive sections of Herodotus, Thucydides, Polybius, Caesar, Sallust, Livy, Tacitus, Dio Cassius and the rest as primary or secondary sources. There is no choice: if the substance of the speeches or even the wording is not authentic, then one may not legitimately recount that Pericles told the assembled Athenians in 430 BC that their empire 'is like a tyranny, seemingly unjust to have taken but dangerous to let go' (Thucydides 2.63.2). I have no idea what Pericles said on that occasion but neither have the innumerable historians who repeat from a speech what I have just quoted. Except for Thucydides and perhaps Polybius, there is no longer any serious argument, though the reluctance to accept the consequences is evident on all sides, if not always with such extreme gyrations as the 'demonstration' that Thucydides could have obtained precise, authentic information for all his speeches and even for the Melian Dialogue,[12] or the discovery that there are 'two kinds of veracity, the one of circumstance, the other of outlook and attitude'.[13]

I do not believe that it is possible to 'save' even Thucydides once it is held that the issue is one of honesty, of morality, in twentieth-century terms. After all, there can be no doubt that on innumerable occasions Thucydides reported as a simple matter of fact that a political figure, a military commander, even a group of

people adopted a particular course of action as the consequence of a particular idea, opinion or judgment when that was at best the historian's own assessment of the reason for the action, an inference back from the act to the thought.[14] One striking form of several that he adopted was this: 'After making this speech Brasidas began to lead his army off, and the barbarians, seeing this, came on, shouting loudly and making a great din, thinking that he was running away and that they would catch him up and destroy him' (4.127.1). Are we to believe that Thucydides was conscious of cheating when he wrote such sentences innumerable times, or when he wrote all his speeches in his own style, when he had speakers reply to other speeches that they could not possibly have known about, when he invented the Melian Dialogue?[15]

It is an endlessly repeated commonplace that the speeches in the ancient historians represent a 'long-established convention' that 'recalls the long association of historiography from its earliest beginnings with epic and drama'.[16] No doubt, but no convention is unalterable, and if this one survived for a thousand years or more, it is untenable that every single practitioner was indifferent to the fact that he was a falsifier, the more blatantly so the more he insisted, with Thucydides and Polybius, that a historian was obligated to tell only the truth. Thucydides must have had something more in mind than just crude deception of his readers when, in the short statement of method in his first book, he wrote the awful part-sentence (1.22.1) that has exercised commentators for perhaps two centuries, with no prospect of a resolution of the difficulties: my method 'has been, while keeping as closely as possible to the general sense of the words that were actually said, to have the speakers say what, in my view, was called for by each situation'. We start from the wrong premise by assuming that Greeks and Romans looked upon the study and writing of history essentially as we do.[17] Collingwood suggested an alternative premise when he wrote in an anguished page on the speeches in Thucydides:

'Custom has dulled our susceptibilities; but let us ask ourselves for a moment: could a just man who had a really historical mind

have permitted himself the use of such a convention? . . . Is it not clear that the style betrays a lack of interest in the question what such and such a man really said on such and such an occasion? . . . The speeches seem to me to be not history but Thucydidean comments upon the acts of the speakers, Thucydidean reconstructions of their motives and intentions . . ., a convention characteristic of an author whose mind cannot be fully concentrated on the events themselves, but is constantly being drawn away from the events to some lesson that lurks behind them, some unchanging and eternal truth of which the events are, Platonically speaking, *paradeigmata* or *mimemata*.'[18]

It was also a not self-evident habit of the ancient historians rarely to paraphrase and even more rarely to quote a document.[19] Thucydides notoriously failed to make any reference to documents in his statement of method (1.22) or to make overt use of them on all but a small number of occasions, although in a context that was essentially irrelevant to his history he demonstrated what could be done to squeeze out evidence by logical inference from two brief inscriptions (6.55). Part of the explanation for the universal indifference of the Greek and Roman historians to documents lies in their paucity and in the rudimentary state of the archives.[20] Modern historians have constantly to remind themselves that the paperasserie with which they are surrounded has not always been a 'natural' product of human behaviour. In the long history of the Graeco-Roman world, massive documentation characterized only the peculiar society of Egypt and to a limited extent the imperial courts of the later Roman Empire.

Records and documents, record-keeping and archives are a function of the society which produces and preserves them, or which largely fails to do one or the other.[21] The psychology and the needs of the Ptolemaic bureaucracy in Egypt had little in common with those of classical Greece or Rome. In the generation after Aristotle had initiated the scholarly collection and publication of various public records, Craterus, a Macedonian disciple, published a corpus of Athenian decrees in perhaps nine books.[22] Book 2 brought his collection past the middle of the fifth

century BC, thus indicating the scarcity of earlier documents he was able to find; and the infrequency of identifiable uses of his collection in later centuries (except for abstruse linguistic or geographical points) suggests the absence of much interest in what we should consider to be an invaluable tool for historical study. As for Rome, hardly more than a hundred publicly displayed laws, *senatus consulta*, imperial 'enactments' and magisterial edicts are today available from the whole of the territory under Roman rule down to Constantine.[23] And it was not until Caesar's first consulate in 59 BC that the *acta* of the Senate were recorded and made public (Suetonius, *Caesar* 20.1). Before that only the bare bones of the decisions, the *senatus consulta*, were committed to writing.

In short, the epoch-making invention of literacy was followed for centuries by the survival of a fundamentally oral non-literate society. Man can function reasonably well in a pre-industrial society with little or no use of the written word. So when men came to write the history of their world, Greek or Roman, they found great voids in the inherited information about the past, or, worse still, quantities of 'data' that included fiction and half-fiction jumbled with fact. That is what modern historians, unwilling for whatever reason to admit defeat, to acknowledge a void, seek to rescue under the positive label, tradition (or oral tradition).[24] Few anthropologists view the invariably oral traditions of the people they study with the faith shown by many ancient historians. The verbal transmittal over many generations of detailed information about past events or institutions that are no longer essential or even meaningful in contemporary life invariably entails considerable and irrecoverable losses of data, or conflation of data, manipulation and invention, sometimes without visible reason, often for reasons that are perfectly intelligible. With the passage of time, it becomes absolutely impossible to control anything that has been transmitted when there is nothing in writing against which to match statements about the past. Again we suspect the presence of the unexpressed view that the traditions of Greeks and Romans are somehow privileged, though no one has yet demonstrated a plausible mechanism for

the oral transmission of accurate information over a period of centuries (e.g. from archaic Greece to Pausanias in the second century AD, or from the Rome of the kings to Livy and Dionysius of Halicarnassus in the late first century BC). After all, it was in an era of literacy that the Roman nobility successfully paraded fraudulent genealogies at the end of the Republic,[25] or that Tacitus, Suetonius, and Dio Cassius, all of whom had access to contemporary writing, confused the account of the great fire in Rome in AD 64 so effectively that no one has been able to unscramble it satisfactorily.[26]

It is true that what we call 'literary tradition is an inadequate label for a very complex conglomeration of data', including linguistic information and data about religious practices, law and political institutions, as well as the narrative of wars, conspiracies and diplomacy.[27] However, the narrative is the queen of the tradition: without it, much of the other data would be unintelligible. How, for example, are we to use for the history of early Rome the fact that there was a close affinity between Latin and several other Italic languages spoken in or near Latium? What does that tell us by itself about Romans and Volsci or Romans and Sabines without the clues supposedly embedded in the literary tradition? There is no guarantee that the tradition has not arisen precisely in order to explain a linguistic, religious or political datum; that, in other words, the tradition is not an etiological invention – the rape of the Sabine women, for example.

The situation is not fundamentally different with respect to early Roman law, despite the survival of substantial chunks of the fifth-century Twelve Tables in easily translatable classical Latin. What, for example, does the following simple sentence from the Twelve Tables mean: 'Patronus si clienti fraudem facerit, sacer esto?' If we had to answer solely from the Twelve Tables, we should have to say that we have no idea. But we have the literary tradition, which refers to clients, and the answer is that we still do not know but that we have several different and incompatible explanations, several centuries later, but they have no claim to credence because they too did not know and they were unable to differentiate the early clientship from the institution of their own

day. Indeed, they saw no need for serious differentiation: the Romans, like the Greeks, were 'relentlessly modernizing' in their attempts to deal with their own archaic institutions and behaviour.[28]

And why not? Since they lacked a 'conceptual framework' for the understanding of such institutions or of long-range social change,[29] their historians could not make proper sense of data, including documents, which their antiquarians discovered; nor could they assess the reliability of the data. Even we, who have no shortage of concepts, and long experience with techniques of evaluation and interpretation, are in grave difficulties over those periods for which the evidence is largely derived from oral tradition, and for which the documents are extremely few and essentially unintelligible.[30] Some of the supposed data are patently fictitious, the political unification of Attica by Theseus or the foundation of Rome by Aeneas, for example, but we quickly run out of such easily identified fictions. For the great bulk of the narrative we are faced with the 'kernel of truth' *possibility*, and I am unaware of any stigmata that automatically distinguish fiction from fact. The narrative as given by Livy and Dionysius (to which we may add Plutarch's lives of Theseus, Lycurgus or Solon) 'has to be ruthlessly scrutinized for signs of anachronism or embellishment', writes Ogilvie.[31] However, in an earlier work Ogilvie had already been gloomily dubious about the prospect when he pointed out that 'the hunt for anachronisms or contemporary references' in Livy's own late-Republican sources for the earliest period 'is peculiarly hazardous and unprofitable since every Caesarian allusion turns out on inspection to be equally Marian or Sullan'.[32] The only hope is a careful and judicious use of explanatory models (to which we shall turn in chapters 4 and 5). Without a theoretically grounded conceptual scheme, the thin and unreliable evidence lends itself to manipulation in all directions, without any controls.[33]

It is now time to return to the subject with which I began, namely, the place of archaeological evidence within the total source-material available to the ancient historian today.[34] Archaeology appears to be in a turmoil. In an understandable

reaction against the 'counterfeit history' produced by earlier generations of anthropologists and archaeologists, a powerful sector among prehistorians has for some two decades been calling for a sustained effort to establish archaeology 'as a discipline in its own right, concerned with archaeological data which it clusters in archaeological entities displaying certain archaeological processes and studied in terms of archaeological aims, concepts and procedures'.[35] The global claims of the 'new archaeology' have been contested both within the discipline itself and outside.[36] Nevertheless, no serious student can or does ignore the work of its disciples in prehistory or in Near Eastern history, but in classical archaeology the heritage of Winckelmann, with its mixture of treasure-hunting and antiquated art history, still retains a considerable following, in some quarters the dominant one, affecting and restricting archaeological work from the initial choice of excavation sites to the final analysis of the finds.[37] In 1973 the then Reader and now Lincoln Professor of Classical Archaeology in the University of Oxford began a review-article with these words: 'A confrontation between the new archaeology and classical Greece has yet to take place, and perhaps never will, since the classical archaeologist is bound to a full and informative literary background and to art history beside which the contributions of new techniques will never be of great moment, and before which the practitioners of the new techniques are usually helpless, except in the detection of forgeries.'[38]

Well, the classical archaeologists concerned with Roman Britain have nothing like a full or informative literary background or great art to bind them, and accordingly they have long concerned themselves with 'settlement archaeology' and similar aspects of the 'new archaeology' of the prehistorians, though in their own terms rather than as disciples of the latter. By the 1950s comparable studies were being pursued by such archaeologists in Italy as Frank Brown of the American Academy in Rome and John Ward-Perkins of the British School, to name only two leading English-speaking figures, while the native school of Bianchi Bandinelli were engaging in a full-scale attack on the position now represented by Boardman. Greek archaeology was admit-

tedly behindhand, but is rapidly catching up.[39] In short, the turmoil and the polemic have reached classical archaeology, too, and the debate touches the study of antiquity at a vital point, the quality and usefulness of the sources (those that are decidedly not forgeries, I need hardly add).

Welcome and indeed essential as the current debate is, it seems to me to be partly misdirected. To begin with, I believe it to be false to speak of the relationship between history and archaeology. At issue are not two qualitatively distinct disciplines but two kinds of evidence about the past, two kinds of historical evidence.[40] There can thus be no question of the priority in general or of the superiority of one type of evidence over the other; it all depends in each case on the evidence available and on the particular questions to be answered.

There are contexts in which the two types of evidence have to be deployed together so closely that in a sense neither is of much use without the other. As examples I cite the recent work of the Finnish Institute on Roman brick-stamps, and of Garlan on the amphora-stamps of Thasos.[41] In neither instance does the work I have mentioned exemplify the 'archaeological aims, concepts and procedures' proclaimed by the 'new archaeology'. The Roman bricks and the amphoras as objects tell us little in themselves and provide insufficient grounds for choosing among possible historical explanations, while the symbols and abbreviated words inscribed on them are similarly ambiguous or inconclusive without careful quantitative analysis both of the inscribed data and of the site-finds.[42]

Admittedly this kind of symbiotic situation is exceptional. Most commonly the two types of evidence complement each other in one way or another. And sometimes they appear to be mutually inconsistent or in direct conflict. Then one or the other must give way, and that usually (*but not always*) means the literary evidence, provided one can be certain that both the texts and the archaeological objects have been understood correctly, not necessarily an easy or certain conclusion. Once it was discovered, for instance, that many amphoras of the type Baldacci III, related to Dressel 6, originated in Istria, identified by Pliny (*Natural History*

15.8) as a major oil-producing centre, it was assumed that they were containers for oil, especially since in his discussion of wines Pliny had grouped north Italian wines under the rubric *reliqua* (14.67). Yet it is now clear that Pliny was not being dismissive in a quantitative sense, and that the Baldacci III pottery carried wine, not oil.[43] On the other hand, the disappearance by Trajan's time of the Dressel 2–4 type, which had long been the standard Italian wine container, has led to the conclusion that Italian wine export came to a fairly abrupt end at that time. However, the repeated and circumstantial literary evidence, notably of Galen and Fronto, for the continued popularity in the late second century of the famous Falernian wine of Campania cannot be brushed aside; one must therefore conclude that a change in containers occurred in or before Trajan's time although we have not yet been able to identify the replacement for Dressel 2–4.[44] That is the explanation recently offered by André Tchernia, and, though there is so far no confirmation, there is some encouragement in the discovery (mostly still unpublished) for the first time, in the sea off Civitavecchia and off the coast of Tuscany, of large liquid containers, known as *dolia*, up to 1.65m in height and 0.82m in maximum diameter.[45]

For reasons that are rooted in our intellectual history, ancient historians are often seduced into two unexpressed propositions. The first is that statements in the literary or documentary sources are to be accepted unless they can be disproved (to the satisfaction of the individual historian). This proposition derives from the privileged position of Greek and Latin, and it is especially unacceptable for the early periods of both Greek and Roman history, where the archaeological evidence bulks so large (and daily grows proportionally still larger) and where the quantitatively far from inconsiderable literary tradition is particularly suspect.[46] The second proposition is that the most insistent historical question one can put to an archaeological find is, Does it support or falsify the literary tradition? That approach gives automatic priority to literary evidence, and, in the history of early Rome, for example, has led to optimistic claims of archaeological support for the literary tradition, resting on highly selective tests.

One can read everywhere that archaeology now 'confirms in the essential the testimony of the annalistic' tradition.[47] In a sophisticated variant, the argument is that so long as history was only a narrative (*événementiel*), the hypercritical view of the tradition quite properly won the day, but with the switch to more sociological history and new archaeological techniques, renewed study of the topography and building history of the city of Rome confirms the main institutional and chronological lines of the tradition over the longer time-span (*dans la longue durée*), given the notorious conservatism of the Romans.[48]

This leaves me puzzled. The tradition about early Rome is almost entirely a narrative, *histoire événementielle*, and it remains that even though some present-day historians and archaeologists have changed their interest. The confirmation that is now being claimed proves to be in fact extremely restricted. That ancient buildings in the Forum can now be identified on the ground is an important discovery, but it is no confirmation of the traditional attribution of individual buildings to individual kings, an attribution which is too often chronologically impossible. Evidence is now available of the Aeneas myth and even of the Aeneas cult in central Italy as far back as the sixth century BC, but so far none of it from Rome itself.[49] Not surprisingly, therefore, the best modern accounts shift quickly from the question of Roman origins to the no less interesting, but wholly different, area of the intellectual and ideological history of the Roman Republic.[50] As for the relations in the 'regal period' between Rome and its neighbours, there is massive inconsistency between the literary account and what the archaeologists have unearthed.[51] It is sufficient to single out Satricum, a Latin settlement fifty kilometres south of Rome in an area of continuous conflict with the Volsci, mentioned for the first time by Dionysius of Halicarnassus (5.61.3) under date of 496 BC, though clearly by that time it had been one of the main centres in Latium for a century or more, with an important temple of the Italic Mater Matuta.[52]

Two decades of intensive inquiry into early Rome have culminated in the cul-de-sac of Satricum, suggesting that the ancient historian interested in evaluating his sources had better turn

elsewhere. Let us instead consider the most numerous of all archaeological objects, of a kind that has been studied with particular care and sophistication in recent years, both on land and on wrecks in the sea – namely, pottery, the mostly unadorned ceramic ware of daily use, containers, table and kitchen ware, cooking utensils, lamps; not the fine decorated ware that monopolized scholarly attention until recently, giving rise to the illusion that it somehow represented the major part of Graeco-Roman ceramic production. Plain utilitarian pottery can be, and was, made just about everywhere, literally in the millions each year, and fragments (sherds) turn up in vast numbers every year everywhere. Most of it was 'rigorously anonymous', especially the wares with the widest diffusion, from the Corinthian and bucchero pottery of the late archaic era through Attic black-glazed pottery and Campanian A, and on into the Roman imperial age.[53] And the silence of the pots themselves is matched by almost total silence in the literary and epigraphical sources: 'Roman pottery is in a sense prehistoric'.[54] A better test of the mutual deployment and the limits of the two kinds of evidence can hardly be imagined.

The volume of publication in the past three decades on Roman pottery – with which I shall be exclusively concerned – has become almost unmanageable,[55] with particular attention to typology and chronology, the bases for all systematic study, and to the technological and economic aspects. Much has been learned; that the bulk of distribution was local; that shipment over longer distances was predominantly by water (the sea or navigable rivers); that the large containers, amphoras, were shaped for stowing in large ships and were the determining cargo in the selection of routes and ports of call; that other ceramic goods – table ware, cooking vessels, lamps – also shipped in large quantities, were 'parasitic' on the containers and their contents in their occupation of shipping space; and so on. Apart from the evidence provided by pottery marks of the practice of carrying the goods of a number of owners or merchants on a single vessel, most of the rest of what we know comes from the objects themselves, unaided by written texts in any form.

But so far there has been a major failure. In discussing the unparalleled diffusion throughout the Mediterranean basin of North African ceramic ware of every type, beginning about the middle of the second century AD and continuing with increased momentum virtually to the end of antiquity, a phenomenon not even mentioned in the surviving literature, Carandini has recently written: 'But we have not yet succeeded (for the moment) in extracting from these same products the most precious secret – the "social mode" in which they were produced.'[56] Apart from the optimistic 'for the moment', this judgment is shared by all the best students of Roman pottery, whether Italian, Gallic or North African.[57]

To begin with, not many kiln-sites have been identified, even fewer systematically investigated.[58] Secondly, the ownership of potteries and their labour force are unknown except for the relatively small number of cases in which amphora-marks indicate the presence or absence of slaves among the work-force. Our ignorance in this respect includes such central aspects as the relationship of the potters and potteries to the ownership of the land (including the clay-beds), to the men involved in the trade, or to 'branches' in other places. The possibilities are numerous and intricate, more so, I believe, than the speculations in the current literature allow for. That is decisively demonstrated by the publication in 1981 of three mid-third-century AD papyri from Oxyrhynchus.[59] They are contracts for two-year leases of the potteries on large estates (the phrase used in the text) in the district, where the tenants, who identify themselves as 'potters who make wine jars', undertake to produce per annum respectively 15,000, 24,000 and 16,000 jars of a capacity of four choes, for which they are to be paid 32 drachmas per 100 in the first two cases, but 36 per 100 in the third. They are also to manufacture a small number of two- and eight-choes jars, to be paid for in jars of wine or amounts of lentils. The landlords are to provide the potteries, the equipment and the raw materials, the potters only the labour-force (unspecified). And in two of the cases (the unpublished ones) other evidence reinforces the view that the landowners were people of considerable substance.[60]

To the best of my knowledge, this possibility has been ignored in the speculations about the status of potters and potteries. I do not suggest that the Oxyrhynchus leases represent a common method of putting potteries into production in the Roman world (though I see no way to demonstrate that it was uncommon). I merely wish to make the simple point that archaeological evidence or archaeological analysis *by itself* cannot possibly uncover the legal or economic structure revealed by the Oxyrhynchus papyri or the alternative structures in Arezzo, Puteoli, Lezoux or North Africa. The burst of polemical rhetoric with which Carandini closes his survey of the history of North African ware in the final centuries of antiquity[61] serves only to divert attention from the absence of data about the 'social mode of production' of that ware, and, in my view, the impossibility of ever overcoming that gap in our knowledge from archaeological evidence alone. Not for the first time in my life, I quote Stuart Piggott:

'Technology is something that can be illuminated by direct archaeological evidence . . . and from that . . . we can move towards inferring something of subsistence-economics and of man as part of his ecological surroundings. But when we try to infer such things as social structure or in the broadest sense religious practices, such evidence becomes almost wholly ambiguous.'[62]

This 'hard doctrine', as Piggott once referred to it rather tongue-in-cheek,[63] remains unacceptable to those who are prepared to call Troy II 'urban' (or at least 'proto-urban') in the Early Bronze Age, when its area was only about two acres.[64] 'It is simply a question of definition of terms,' writes Renfrew, and, directly inspired by this savagely reductionist view, Guidi has now dated the proto-urban phase of Rome back to the end of the ninth century BC.[65] That such a typology is of much use to an historian is doubtful: the critical definition of terms is not something to be achieved so 'simply'.[66] The study of amphoras is today primarily a study of production and trade, and the evidence which amphoras provide, it has recently been concluded, 'can only be properly assessed against the background of what we know about the system of marketing from literature, epigraphy,

and the still under-utilized legal sources'.[67] I should qualify that: I see no reason for distinguishing the 'evidence' of the amphoras and the 'background of what we know' from other kinds of sources. What ties them all together in the end is the conceptual framework from which the historian works, a framework which, we saw earlier, the historians in antiquity lacked for the inquiry we are now concerned with; and which historians today possess in super-abundance.

Notes

Numbers in brackets after titles refer to chapter and note where full citation may be found.

[2] The Ancient Historian and his Sources

1 – A substantially shorter version of this chapter appeared in *Tria Corda. Scritti in onore di Arnaldo Momigliano*, ed. E. Gabba (Como 1983), pp. 201–14.

2 – 'Mesopotamian Social Organization: Archaeological and Philological Evidence', in *The Evolution of Social Systems*, ed. J. Friedman and M. J. Rowlands (London and Pittsburgh 1978), pp. 457–85, at p. 473.
3 – A. Momigliano, 'Ancient History and the Antiquarian' (1:10), p. 68.
4 – Herodotus 3.122; Thucydides 1.20–21.
5 – Thucydides' brief survey of Greek developments in the more distant past (1.2–18) is no exception: it is the expression of a general sociological theory about power and progress, not history in any usual sense.
6 – 'We do not find it too hard to recognize the inventive technique in comparatively unimportant details, but it does not come naturally to us to expect the *big* lie as well . . . Nowadays we have learned to pay lip-service, at least, to the dangers of accepting annalistic material as reliable, but the will to believe is still strong': T. P. Wiseman, *Clio's Cosmetics* (Leicester 1979), pp. 52–3.
7 – See D. Musti, *Tendenze nella storiografia romana e greca su Roma arcaica* = *Quaderni urbinati* 10 (Rome 1970).
8 – It is enough to cite T. J. Cornell, 'Aeneas and the Twins: the Development of the Roman Foundation Legend', *Proceedings of the Cambridge Philological Society*, n.s. 21 (1975) 1–32; E. Gabba, 'Storiografia greca e imperialismo romano (III–I sec. a.C.)', *Rivista storica italiana* 74 (1974) 625–42, 'Sulla valorizzazione politica della leggenda delle origini troiana di Roma . . .', in *I canali della propaganda nel mondo antico* (Milan 1976), pp. 84–101; C. B. R. Pelling, 'Plutarch's Adaptation of His Source-Material', *Journal of Hellenic Studies* 100 (1980) 123–40.
9 – R. M. Ogilvie, *A Commentary on Livy, Books 1–5* (Oxford 1965), p. 88.
10 – R. M. Ogilvie, *Early Rome and the Etruscans* (London 1976), pp. 174–6.
11 – F. W. Walbank, *Polybius* (Berkeley and London 1972), p. 44.
12 – F. E. Adcock, *Thucydides and His History* (Cambridge 1963), pp. 27–35. The lengths to which Thucydides continues to drive modern commentators is revealed in *The Speeches of Thucydides*, ed. P. A. Stadter (Chapel Hill 1973).
13 – J. H. Finley, Jr., *Thucydides* (Cambridge, Mass., 1942), p. 102. Or compare, on the speeches in Livy, that 'whilst one may regret that such speeches are unhistorical, in a literal sense, the psychological insight which they reflect' deserves nothing but praise: P. G. Walsh, *Livy* (Cambridge 1961), p. 220.
14 – See C. Schneider, *Information und Absicht bei Thukydides* (Göttingen 1974).
15 – It will be fairly obvious that I myself have no doubt that the speeches even in Thucydides are not authentic in any normal sense of that adjective, but I shall not argue the case further as my analysis would not fall because of an exception or two. I refer to H. Strasburger, 'Thukydides und die politische Selbstdarstellung der Athener', *Hermes* 86 (1958) 492–530, reprinted in his *Studien zur Alten Geschichte* (2 vols., Hildesheim and New York, 1982) II 676–708); Schneider, *Information*, pp. 137–54; briefly and cautiously, A. Andrewes and K. J. Dover, *A Historical Commentary on Thucydides*, Book VIII (Oxford 1981), pp. 393–9. *A fortiori* I have even less doubt about Polybius as the composer of the speeches in his work despite the heroic effort of Walbank to rank his authenticity as a speech reporter even higher than that of Thucydides: *Speeches in Greek Historians* (Third J. L. Myres Memorial Lecture, Oxford [1965]).
16 – Walbank, *Speeches*, pp. 18 and 2, respectively.
17 – It is worth noticing that today we tolerate, and even desire, much more leeway in these matters from biographers.

18 – *The Idea of History* (Oxford 1946), pp. 30–31.
19 – The great exception was ecclesiastical history from its invention by Eusebius early in the fourth century AD; see the subtle analysis by A Momigliano, 'Pagan and Christian Historiography in the Fourth Century AD', in *The Conflict between Paganism and Christianity in the Fourth Century*, ed. Momigliano (Oxford 1963), pp. 79–99, reprinted in his *Terzo Contributo* . . . (Rome 1966) I 87–109.
20 – See G. Klaffenbach, *Bemerkungen zum griechischen Urkundenwesen* (Deutsche Akad. d. Wiss. zu Berlin, Klasse für Sprachen . . ., *Sitzungsberichte*, 1960 no. 6), pt. 1.
21 – See chap. 3 below.
22 – F. Jacoby in *RE* XI 1617–21, reprinted in his *Griechische Historiker* (Stuttgart 1956), cols. 165–7. Despite such predecessors of Aristotle as the Sophist Hippias of Elis, it remains correct to identify the Peripatetics as the founders of Greek archival investigation.
23 – See the collection in *Fontes iuris Romani antejustiniani, Leges*, 2 ed., S. Riccobono (Florence 1941). Epigraphical finds since 1940 have not changed the picture significantly.
24 – For what follows, see Finley, 'Myth, Memory and History', *History and Theory* 4 (1965) 281–302, reprinted in my *Use and Abuse of History* (London and New York 1975), ch. 1.
25 – T. P. Wiseman, 'Legendary Genealogies in Late-Republican Rome', *Greece and Rome*, 2nd ser., 21 (1974) 153–64.
26 – Z. Yavetz, 'Forte an dolo principis (Tac. *Ann.* 15 38)', in *Studies* . . . *C. E. Stevens* (1975) 181–97. The grounds escape me for Yavetz's certainty that 'there must have been some basis for the contention that Nero set fire to the city'.
27 – A. Momigliano, 'The Origins of the Roman Republic', in his *Quinto contributo* . . . (Rome 1975), pp. 293–332, at p. 296.
28 – M. H. Crawford, 'The Early Roman Economy, 753–280 BC', in *Mélanges* . . . *Heurgon* (Paris 1976), pp. 197–207, at p. 198. He is writing specifically about early money and coinage but the characterization applies equally to other institutions.
29 – Wiseman, *Clio's Cosmetics* (2:5), p. 45.
30 – E. G. I^3 1, an Athenian decree regarding Salamis; the so-called *lapis niger* from the Forum: *Remains of Old Latin*, ed. E. H. Warmington, IV (*Loeb Classical Library* 1940), p. 242; the recently discovered inscription from Satricum: *Lapis Satricanus*, ed. C. M. Stibbe (*Arch. Studien van het Nederlands Inst. te Rome, Scripta Minora*, 5, 1980).
31 – Ogilvie, *Early Rome* (2:10), p. 29.
32 – Ogilvie, *Commentary* (2:9), p. 13.
33 – See, as a tentative beginning, comments in the two chapters by C. Ampolo and the subsequent discussion in the two issues of *Dialoghi di archeologia*, n.s. 2 (1980), on 'La formazione della città nel Lazio'.
34 – Inscriptions and papyri are not, properly speaking, archaeological sources; brick- and pottery-stamps are border-line.
35 – D. L. Clarke, *Analytical Archaeology* (London 1968), p. 13.
36 – See e.g. *Research and Theory in Current Archaeology*, ed. C. L. Redman (New York 1973).
37 – See e.g. B. D. Shaw, 'Archaeology and Knowledge: The History of the African Provinces of the Roman Empire', *Florilegium* 2 (1980) 28–60.

38 – John Boardman, in *Encounter* (April 1973) 67.
39 – See e.g. A. M. Snodgrass, 'La prospection archéologique en Grèce et dans le monde méditerranéen', *Annales, E.S.C.* 37 (1982) 800–12.
40 – 'In fact, it is or ought to be obvious that history is a single science, to which the different disciplines must contribute in their different ways and which in the final instance provides the ultimate criteria of assessment': F. Coarelli, 'Public Building in Rome between the Second Punic War and Sulla', *Papers of the British School at Rome* 45 (1977) 1–23, at pp. 1–2.
41 – T. Helen, *Organization of Roman Brick Production in the First and Second Centuries* AD, and P. Setälä, *Private Domini in Roman Brick Stamps of the Empire* (*Acta Instituti Romani Finlandiani* IX 1 and 2, 1975–77), with review by J. Andreau in *Annales, E.S.C.* 37 (1982) 920–5; Y. Garlan, 'Koukos', in *Thasiaca* (*Bulletin de correspondance hellénique*, Supp. 5, 1979), pp. 213–68.
42 – On the traps awaiting the historian, see J. Y. Empereur, 'Les anses des amphores timbrées et les amphores: aspects quantitatifs', *Bulletin de correspondance hellénique* 106 (1982) 219–33.
43 – See briefly J. Paterson, '"Salvation from the Sea": Amphoras and Trade in the Roman West', *Journal of Roman Studies* 72 (1982) 146–57, at p. 153, with bibliography.
44 – A. Tchernia, 'Quelques remarques sur le commerce du vin et les amphores', in *The Seaborne Commerce of Ancient Rome*, ed. J. H. D'Arms and E. C. Kopff (*Memoirs of the American Academy in Rome* 36, 1980), pp. 305–12; cf. A. Hesnard and C. Lemoine, Les amphores de Cécube et de Falerne, *Mélanges de l'école française de Rome. Antiquité* 93 (1981) 243–95, at pp. 262–4.
45 – P. A. Gianfrotta has made reference to such finds in *Bolletino d'Arte* 56 (1981) no. 10, pp. 80–81, and he proposes to publish a proper account shortly. These *dolia* have been found together with numerous Dressel 2–4 amphoras and are dated early in the reign of Augustus.
46 – See the short but important review by A. Drummond in *Journal of Roman Studies* 72 (1982) 177–9.
47 – J. C. Richard, *Les origines de la plèbe romaine* (*Bibliothèque des écoles françaises . . .* 232, 1973), p. xii.
48 – F. Coarelli, 'Topographie antique et idéologie moderne: le forum romain revisité', *Annales, E.S.C.* 37 (1982) 724–40, at p. 728.
49 – Cornell, 'Aeneas' (2:8), pp. 11–16.
50 – See above all A. Momigliano, 'How to Reconcile Greeks and Trojans', *Mededelingen* of the R. Dutch Acad. of Sci., n.s. 45 no. 9 (1982); cf. Cornell, 'Aeneas' (2:8); E. Gabba, 'Studi su Dionigio de Alicarnasso, I. La constituzione di Romolo', *Athenaeum*, n.s. 28 (1960) 175–225.
51 – See briefly T. J. Cornell in a discussion, *Dialoghi di archeologia*, n.s. 2 (1980) 206–7.
52 – See the historical chapters by H. S. Versnel in *Lapis Satricanus*, esp. pp. 102–7. Note should be taken of the suggestion by Stibbe (*106n10*) that the original name may not have been Satricum but e.g. Pometia, which has not been located.
53 – J. P. Morel, 'La céramique comme indice du commerce antique', in *Trade and Famine in Classical Antiquity*, ed. P. Garnsey and C. R. Whittaker (Cambridge Philological Society, Supp. vol. 8, 1983), sect. 3.1.1. This article (pp. 66–74) is an important statement of the methodological problems and principles.

54 – D. P. S. Peacock, *Pottery in the Roman World* (London 1982), p. 2.
55 – A good introduction is provided by Peacock, *ibid.*
56 – 'African Pottery', in *Trade in the Ancient Economy*, ed. P. Garnsey et al. (London and Berkeley 1983), pp. 145–62, at p. 155.
57 – See e.g. Morel, 'Céramique' (2:53); Morel, 'La produzione della ceramica campana: aspetti economici e sociali', and G. Pucci, 'La ceramica italica (terra sigillata)', in *Società romana e produzione schiavistica*, ed. A. Giardina and A. Schiavone (3 vols., Bari 1981) II 81–121.
58 – D. P. S. Peacock, 'Recent Discoveries of Roman Amphora Kilns in Italy', *Antiquaries Journal* 57 (1977) 262–9, with bibliography; cf. Hesnard and Lemaine in *Mélanges* (2:44).
59 – H. Cockle, 'Pottery Manufacture in Roman Egypt: a New Papyrus', *Journal of Roman Studies* 71 (1981) 87–95. Strictly speaking, only one of the papyri has been published; the others are mentioned in the article only in the case of variants. The two not published are for lease of one third and one fourth of a pottery, respectively, and one must assume, with the editor, that the remainders were leased in documents we no longer have. I have adjusted the figures accordingly.
60 – I have omitted some details and possible further complications. It is relevant for what follows immediately in the text that the practice has been known to historians for a long time, though not in such detail, from several other Egyptian pottery leases. Two, from the sixth century AD, are leases for a fraction of a pottery, one fourteenth in one case, one third in the other, for ten years and for the lessee's lifetime, respectively: *P. Lond.* III 994 (p. 259) and *P. Cairo Masp.* I 67110.
61 – Carandini, 'African Pottery' (2:56), pp. 156–8.
62 – In *Man, Settlement and Urbanism*, ed. P. J. Ucko et al. (London 1972), p. 950.
63 – Cited in my *Use and Abuse* (2:24), p. 88.
64 – C. Renfrew, *Problems in European Prehistory* (Cambridge 1979), p. 35.
65 – A. Guidi, 'Sulla prime fasi dell' urbanizzazione nel Lazio protostorico', *Opus*, 1 (1982) 279–85.
66 – See the reply to Guidi by C. Ampolo, and the ensuing discussion, in *Opus* 2 (1983) 425–48. That the definition of a city is far less simple than Guidi and his mentors believe is shown, e.g., by P. Wheatley, 'The Concept of Urbanism', in Ucko, *Man, Settlement* (2:62), pp. 601–37.
67 – Paterson, '"Salvation from the Sea"' (2:43), p. 157.