

ARCHAEOLOGY AND KNOWLEDGE: THE HISTORY OF THE AFRICAN PROVINCES OF THE ROMAN EMPIRE

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Il était un temps où l'archéologie, comme discipline des monuments muets, des traces inertes, des objets sans contexte et des choses laissés par le passé, tendait à l'histoire et ne prenait sens que par la restitution d'un discours historique; on pourrait dire, en jouant un peu sur les mots, que l'histoire, de nos jours, tend à l'archéologie, -- à la description intrinsèque du monument.¹

I begin with the proposition that archaeology is at best a methodology, and at worst a mere technique, by which we acquire a certain type of knowledge about the past. The practitioners of the technology began to realize, about two decades ago, that there were some disturbing problems afflicting the production of knowledge from mute or inert objects, a realization which rapidly turned to a profound sense of crisis. The *angst* was first and most keenly felt by prehistoric archaeologists in Europe and the Americas, and was centred on the tendency alluded to in the passage from Foucault quoted at the head of this paper: the tendency for archaeology to become, or at least seek legitimacy in, history. But, as David Clarke phrased it in his classic re-evaluation of the discipline, an archaeologist is *not* an historian. Clarke did not consider this negative assertion to be particularly pessimistic, but merely the

first step in clarifying exactly what the purpose of archaeology was. The archaeologist's purpose, he claimed, has been so warped by the influence of Clio that archaeologists feel compelled to write "counterfeit history," rather than to do archaeology itself. His strident proclamation was that, "Archaeology is archaeology, is archaeology Archaeology is a discipline in its own right, concerned with archaeological data which it clusters in archaeological entities displaying certain archaeological aims, concepts and procedures."² A part of this same crisis has also been perceived in the field of Classical archaeology, though only at a more mundane level where some of the limitations of traditional techniques have become glaringly apparent.³ In response, the so-called "new archaeology" sought to define a distinct field of archaeological discourse:

. . . archaeology can be redefined as the discipline concerned with the recovery, systematic description and study of relict artefacts. Archaeology is a discipline in its own right, providing a framework within which the entities and processes of archaeology act upon one another. The entities, processes, aims, procedures and concepts of archaeology have a validity of their own in reference to the archaeological frame and despite their generation by -- and partial correlation with -- former social and historical entities.⁴

The success of this bravely formulated programme depends, above all, on the specification of the peculiar object of archaeological discourse: the particular data it and it alone analyzes in a unique manner. As stated by Clarke above, the peculiar object is the "artefact:" "the most tangible product of hominid behaviour is the material artefact -- *any object modified* by a set of humanly imposed attributes."⁵ Of course, Clarke also admits, although these are not his precise words, that archaeology is a specific type of hermeneutic of artefacts, a peculiar "reading" or interpretation of mute material remains.⁶ The ultimate goal of the new archaeology, then, is to specify the end point of this interpretation, the formulation of a new type of knowledge derived from mute artefacts alone that has an independent existence -- the latter

point being a necessary correlate to the autonomy of an archaeological discipline. How is this to be achieved? By no less than the assertion that the collective evidence (the artefacts) in and of itself forms an enclosed and independent "total system" that can be reconstructed as a separate archaeological reality by the archaeologist. Some, like Binford, would go further and assert that, after the construction of the artefactual system, the archaeologist can advance to the connection between it and the cultural system that produced the artefacts, no matter how tenuous the links between the two might *appear* to be:

It is highly improbable that the multiple variables which determined the form of any item of the distribution of items should be restricted to any one component of the cultural system. This means that the data relevant to most, if not all, the components of the past sociocultural system are preserved in the archaeological records.⁷

Naturally, the critics have noted that this begs the question of whether or not it is even possible for any primary models for interpreting cultural phenomena to emerge from prehistoric data alone, as this model would require. They draw attention to the apparent lack of any success in constituting interpretations from archaeological data alone, and rightly voice their scepticism.⁸

But, whatever the merits of the new approach as a whole, it has correctly emphasized the peculiarity of archaeology as a discipline and has specified a proper object of its discourse. With this I am in complete agreement, although I would pursue the matter further than the analysts mentioned above. Since they are prehistorians they still seem to imagine that there are other objects of archaeology that are not subject to their critique.⁹ In my view this cannot be so; there is no other archaeology (e.g. "historical," "industrial") which is not also "prehistoric," or perhaps better "ahistoric," in the object of its discourse. The hermeneutic involved is the same in all archaeologies, and once any other type of interpretation is involved the resultant methodology is no longer, properly speaking, archaeology. Hence, the intrusion

of a written document of any kind whether it be semiotic, epigraphic, or the written/printed text, necessitates a different type of interpretation which, since it is not archaeological (except in some metaphorical sense not proper to this investigation), is excluded from further consideration in this paper.¹⁰ Once the crucial distinction is made as to the proper sphere of an archaeological discourse and its limitations, we begin to understand why, under the precise historical conditions of its knowledge, archaeology has often contributed less than was expected of it, at least from the historian's vantage point.¹¹

The first fact that must be emphasized with great force is that, along with many other modern "studies," archaeology was a scientific creation of the European Industrial Revolution. That is to say, the discipline that we recognize as archaeology today, and not mere antiquarianism, was born in a specific European historical context that largely determined its actual application as a technology. "Archaeology" was created in the specific circumstances of a will to have a more scientific knowledge about our past, and in the optimism that something like history could be pushed further into the past.¹² The other historical condition that bounded the new science from its inception was its dependence upon and subordination to the interests of other disciplines, mainly art history and political history. In its subservience to dominant academic ideologies archaeology was so suffused with their priorities that it never clearly separated its own identity and interests from those of the fields, principally political history, of which it formed a "colonial study." The confusion between the proper fields of historical and archaeological discourse is so pervasive that it has apparently succeeded in convincing even the new archaeologists that there exists a genuine and separate discipline known as "historical archaeology." The primary interests of history, in the instance which I am to approach Classical history, have tended to dictate the context within which archaeology has been practised, the discipline of ancient history itself being determined even to this day by the dictates of Classical philology. In the specific case of the provinces that made up Rome's empire in Africa west of Egypt, the first historical factor mentioned above was of critical importance. In North Africa archaeology was a new scientific

technology imported in modern times by colonial powers, and its political position only tended to accentuate trends already in force in Europe itself.¹³

The specific connection between archaeology and the history of the Roman provinces must, perforce, revolve about the central problematics determined by history. As a recent *Annales* debate has reminded us, there can be no doubt about *the* central question posed for the African provinces and, for that matter, for all the western provinces of the Empire: the twin problems of culture and conquest, of how North African society changed in the process of becoming part of the Roman imperial system.¹⁴ The problem was recognized long ago, almost intuitively, by the colonial settlers and administrators who came to North Africa for whom the processes of acculturation and political control were of direct consequence. The problem has been posed under a number of different rubrics, although for the longest period it has been studied under the heading of "Romanization," a natural presumption of a one-way evolution viewed from the centre outward. More recently, however, the same question has emerged under the antinomic headings of *résistance* and *sous-développement*.¹⁵ The point is that, whatever the label, the central problem of the history of the ancient Maghrib of the Roman period has always reduced to the same process. What has been the contribution of archaeology?

In one sense, if we were to accept a lax, all-embracing definition of archaeology as merely collecting ancient artefacts, the contribution has been revolutionary. One need only take a "before and after" measurement of North African historiography to appreciate the extent of the contribution. If histories of the "pre-archaeological" era are considered (say those written before the 1850s to 1860s), one has the expected situation where knowledge is based almost entirely on Classical literary texts together with a sampling, usually exotic and inaccurate, of local geography and ethnography. A survey of synoptic histories produced since the 1870s and 1880s reveals a radical shift in our knowledge, almost to the point where modern historical accounts bear little resemblance to those of a century and a half ago.¹⁶ But in so far as one can judge from the histories as written, the immense increase in our knowledge is due to one source alone: epigraphy. The 60-70,000 Libyan, Punic, Greek,

and Latin inscriptions recovered in North Africa have been the crucial factor in the rewriting of the history of the African provinces. Almost every general work on the subject reflects this source, as opposed to archaeological evidence, as the basis of its historical reconstruction. Inasmuch as the epigraphy happens to exist in such abundance, it has been exploited in preference to any other type of evidence. This predilection has led to an imbalance in North African studies that would be hard to match in any other region of the Empire.¹⁷

For example, Broughton's justly famous work entitled *The Romanization of Africa Proconsularis* (1929), a study centred on the cultural and political process referred to above, makes no use of archaeological evidence; in essence it is a literary and epigraphic study.¹⁸ Three decades later, Romanelli's *Storia delle Province Romane dell'Africa* (1959), even though a more broadly encompassing overview of African history, contains only about twenty-five explicit references to archaeological data; all but a few of these are peripheral to the subject under discussion at any point, and most could be discarded without serious impact on the narrative.¹⁹ His account is still overwhelmingly literary and epigraphic. Much the same observation applies to two more recent works that take the theme of resistance as their point of departure: Racht's *Rome et les Berbères* (1970) and Bénabou's *La résistance africaine à la romanisation* (1975).²⁰ These authors have had recourse to discrete pieces of archaeological data about as often as Professor Romanelli, and it is only the strong emphasis in their respective works on military construction on the frontiers that allows them to make more of these references count.

Thus a question of ironic overtone plainly emerges. Why, in spite of more than a century of hard work and production of data, has archaeology contributed so little to a broadly based historical understanding of the North African provinces? In part it must be because historians are not asking the right questions, or because archaeologists are not doing the right things. In this perspective one can point easily to some obvious ironies. It is arguable that the singular importance of Africa to the Empire was its agrarian productivity; even the great number of municipalities and the power of local urban élites, upon which the

central state depended so much, were based essentially on the land. Yet, archaeology in North Africa has concentrated on the superficial aspect of the urban settlement to the virtual exclusion of the countryside.²¹ Why? The colonial context of archaeology which tended to overemphasize current European trends must be seen as part of the explanation. As a monopoly technology wielded by the Europeans, the importation of archaeology was not as innocuous a process in North Africa as in other colonial contexts. The reason for the greater value attached to archaeology stemmed from the fact that North Africa, at least in the view of the *colons*, already had a European past, and the one-sided instrument of archaeology was at hand to recover that lost chapter of European history. The tendency is reflected in the fact that the better part of archaeological field work in North Africa during the first century of colonial domination was carried out not by professional technicians or trained academics, but either by military officers of the occupation forces (men like Lt. Hilaire, Col. Baradez, Commdt. Donau, and Gens. Chanzy and Goetschy), or by members of the colonial administration such as the indefatigable Dr. Louis Carton or the progressive liberal Charles Saumagne.²² This distorted reading of the African past as a variant of present experience, with its emphasis on the instruments of domination and acculturation, led quite naturally to the city. In the patterns of its streets, the plans of its buildings, its iconography, and its official language, the Classical city reflected what the colonialist, given his vicarious identification with the Roman achievement, wished to see in the past. The colonizers were greatly assisted in this predilection by the historical development of urbanism, since, in the period following the Arab invasions, new towns and cities were founded in locations removed from the old urban sites of the Roman period. The ruins of Roman towns were ready to be exploited, as they stood in their hundreds as manifest testimony to the successes of Roman rule.

The devotion of archaeology to the city is ironic on another count: it was precisely the location where the technology was least likely to make a distinctive contribution to knowledge, given the fact that the mass of inscriptions unearthed on urban sites tended to tell the same story, only better. But the degree of this unabated mania for urban

archaeology is perhaps difficult to convey to archaeologists and historians whose normal field of endeavour is elsewhere in the Mediterranean or in northwestern Europe. The magnitude of the imbalance can be assessed in part by comparison with other regions of the Empire in the West. In these areas, admittedly by default, rural archaeology has had an important if not dominant rôle.²³ But there could be no survey for North Africa comparable to Shimon Applebaum's contribution on Roman Britain in Finberg's *Agrarian History of England and Wales*.²⁴ Nor, since the archaeological data are not yet available, could North Africa provide the sort of detailed analysis of agrarian structures produced by Agache and Wightman for the Gauls and Germanies.²⁵ Even in its rural archaeology Africa is in the past: "Once, 'rural settlement' was understood to mean villas, and villas were mostly known for their mosaics and hypocausts," is one assessment of what archaeology used to mean; but, delete "hypocausts" and it is a fair assessment of the historic trend in North Africa.²⁶

The extent of this central problem is also mirrored in general histories that treat the North African rural economy. In his magisterial work on the society and economy of the Roman Empire (1926) Mikhail Rostovtzeff exploited the literary and epigraphic evidence on rural North Africa in some detail but, excepting mosaics, there is not one piece of archaeological evidence in his text. To give Rostovtzeff his rightful due, it is true that archaeological data (again, principally mosaics) do appear in iconographic form in his plates which, along with their accompanying commentary, gave Rostovtzeff's work that distinctive form which has impressed generations of students since its publication.²⁷ But there is a curious disjunction between the pictorial aspect and the text where archaeological data hardly appear. The second edition of this fundamental work, thoroughly revised and supplemented three decades later by an equally assiduous and painstaking scholar, did not add a single piece of archaeological data to Rostovtzeff's account of rural North Africa. The same dependence on literary evidence remains true of subsequent general works such as Haywood's survey of Roman Africa in Tenny Frank's *Economic Survey of Ancient Rome* (1938),²⁸ in Warmington's survey of "The Country" for his book on the African provinces in the later Roman

Empire (1954),²⁹ and in Frend's work entitled *The Donatist Church* (1952, rev. 1971). It was essential to Frend's principal thesis on the origins of the so-called Donatist Church -- namely, that the schism was based in the rural settlements of the High Plains of Numidia -- that he provide a detailed account of the economy and society of this rural region of North Africa. Frend did attempt such an analysis, but his chapters devoted to the subject are literary at core with very unequal, sporadic insertions of archaeological data.³⁰ When, some decades later, Frend issued a second "edition" of his work (1971), he was not able to add any archaeological evidence to his picture; in the preface he laments that the main hindrance to any further advances in understanding the background to African Christian movements is the lack of any rural archaeology, and he notes that "the author's site of Kherbet Khararous northwest of Timgad remains, he understands, as he left it thirty years ago." He then states, "Work on Romano-Berber villages in Algeria, which alone can settle some of the questions raised in *The Donatist Church*, particularly concerning the possible correlation of the rise of a Numidian village economy in the fourth century with the almost unanimous acceptance of Donatist Christianity in Numidia, has to await more settled political conditions."³¹ While agreeing with Professor Frend's observations on the disastrous state of rural archaeology, I am compelled to disagree with him over the root cause; settled political conditions both before 1955 and after 1962 did not radically alter the pre-war trends in North African archaeology. Archaeology, as any survey of the work done in the last two decades will show, remains overwhelmingly oriented towards the urban site.

What then are some of the problems bedevilling a proper archaeological knowledge in the Maghrib? We might begin by turning our attention to the cities which have already been the object of much archaeological digging. In the long-term urban history of North Africa, one of the central questions pertaining to cities and "Romanization" is the advance in municipal status of urban settlements as recognized by central Roman authority. The problem of archaeology and history in this instance is a confusion of the relevance of archaeological and historical methodologies, since archaeology has constantly been pressed into service to

explain municipal development in terms of economic growth, architectural and artistic forms, and urban size. One of the more recent detailed analyses of Roman municipal policy by Gasco (1972) typifies this approach. Gasco lists as the three most important explanatory factors affecting advancement of municipal status 1. "importance économique et richesse de la communauté," 2. "intérêt stratégique," and 3. "degré . . . de romanisation," while greatly subordinating such factors as "protection de patrons puissants" and "faveur exceptionnelle de l'empereur."³² In practice all changes in municipal status explained by Gasco are attributed to the first three factors alone, and of these the first (measure of economic wealth equals extent of ruins) predominates. But surely this is a confusion over what archaeology can and cannot tell us. If, for the sake of argument, one were to ask if the connection between material artefact and cultural result could be struck, the answer would probably be negative. If all the epigraphic data relevant to the question were arbitrarily removed, would the archaeological data alone be able to predict, with any degree of certainty and within a consistent theoretical model, the outlines of a "municipal policy," that is to say, which towns were *civitates*, which *municipia*, and which *coloniae*? Of course not, and Gasco's study and yet another on African municipalities of the later empire by Kotula, though they hold to this explanatory model, contain so many glaring exceptions to the rule that only one certain conclusion can be drawn, namely, that there is no necessary direct causal relationship between factors such as urban size, wealth, geographical siting, and municipal status.³³ Small and economically insignificant places achieve it where prosperous and centrally located towns with a high degree of "Romanization" or artistic and architectural design do not. The students of municipal policy are then compelled to devise a number of *ad hoc* explanations for cases that do not fit.³⁴ It seems preferable to admit the obvious: that the archaeological connection does not exist and that the two sets of factors, the material artefacts and the cultural results, are related by way of a third generative process that actually produces the change in town status. In the only cases where such motives are known, the answer is that the third factor is the connection between local and central political élites and the process

of patronage. This we know, however, from literary and epigraphic data.³⁵

This example has been cited not in order to question entirely any connection between archaeological facts and historical ones, but only to lead to the restating of two premises. First, that the field of archaeological discourse is effectively limited or bounded by the mere existence of literary records in such a way that the relationship between the methodologies of archaeology and history can be conceived of as sort of "zero-sum game."³⁶ Second, that archaeology is a peculiar discipline with its own sphere of operation, its own objects and type of knowledge. What might the latter be? From the perspective of the ancient historian, an indication has been given in another context by the social historian Keith Hopkins when he discusses the reasons for his emphasis on the "long-term consequences of repeated actions," and his intent to explore "the consequences of these actions independently of the actions of individual actors" because "the actors often did not know the long-term consequences of their actions."³⁷ Hopkins' repeated emphasis on the *long-term* is rather reminiscent of the *histoire de la longue durée* of the *Annales* school.³⁸ It seems to me that this must be one of the peculiar fields where archaeology, by focussing on the long-term history of institutions, social practices, and economic developments, can reveal the trends in events of the long range that are hidden from the specificity of written records compiled by the actors themselves. Here archaeology does much more than attempt to add to or correct specific historical events.³⁹

In so far as the cities of North Africa are concerned, then, one would like to know something from archaeology about their long-term urban history. What is the record of archaeology here? Fairly dismal, it must be admitted. There have not been many important advances since Toutain first treated the subject in a synthetic manner in 1896.⁴⁰ Even as late as the 1960s, as promising as the titles of some approaches to *le développement urbain* might seem they still depend on epigraphic rather than archaeological data.⁴¹ Recently, however, there have been some studies that concentrate on structural aspects of the ancient city and have treated them within an archaeological framework. For example, the problem of water supply is one that is apparently so crucial to urban

development in North Africa that the connection between the construction of elaborate water-supply systems and urban growth seems an obvious and logical one. But that equation or correlation has been questioned and even denied by Leveau and Paillet following on their detailed study of the aqueduct constructed by the city of Caesarea (mod. Sharshall) around the turn of the second century. Although the aqueduct did supply the city with about 40,000 cubic metres of water per day, Leveau and Paillet conclude that it was not necessary for the urban growth of Caesarea. They further contend that aqueducts were not in general responsible for the expansion of any other urban sites in North Africa; the water requirements of the towns were met more adequately by existing wells, cisterns, and other water-storage systems.⁴² Many towns, such as Thuburbo Maius (mod. Hr. al-Qasbat), functioned quite well without any aqueduct; in most cases where aqueducts can be dated, as at Carthage, they follow upon an initial phase of urban growth and appear to be intended for the supply of the "luxury consumption" of water in public edifices such as fountains and baths.⁴³ Hence an apparent contradiction: urban development did not depend on water supply by aqueduct which seems to have been destined for public consumption of water in modes peculiar to the ideological concept of a "Roman city." The construction of massive monumental aqueducts at great expense for "luxury" use by the city is, like the building of roads, "a material expression of urban domination of the countryside." In particular, the building of an aqueduct at Caesarea is now interpreted as a correlate of that city's exploitation of its rural hinterland by a Romano-African bourgeoisie.⁴⁴ So, too, the question of the construction of city walls, another integral part of the façade of the Roman town, has come under closer scrutiny.⁴⁵ Traditionally, in a North African context the city wall has been interpreted functionally as indirect evidence of insecurity in the surrounding countryside. In a detailed study of the town walls of three Roman cities in Mauretania Tingitana (the northwestern part of present-day Morocco), Rebuffat concluded that they were built as much for reasons of civic prestige as for purposes of defence; they were expensive projects indulged in, like aqueducts, at the height of a city's economic expansion. There does not seem to be any direct correlate with military needs, just as

often walls that could have no defensive character are used to attach civilian settlements to military ones (as at Rapidum and Bû-Njem) as a means of defining the extra-camp settlement as part of a true city.⁴⁶

But the long-term history of the city in North Africa will only begin properly once a typology of the different sorts of urban settlement has been made, along with a typology of the internal component parts of the city. The former, so far as I know, has not yet been attempted by any archaeologist or historian; the latter is only beginning, exemplified by studies such as Rebuffat's categorization of African "maisons à péristyle," though many other similar studies of the elements of the urban matrix are needed before we could begin to trace the long-term development of the Roman town in Africa as an archaeological complex.⁴⁷ In the latter case, the problems posed to the development of a typology or spectrum of hermeneutics from archaeology to history by *objets d'art*, such as pictorial relief sculpture and mosaics to name but two obvious examples, are acute. The hermeneutic of mute objects, such as arrowheads and pillars is, obviously, their material function. But art objects of the type just mentioned are involved in an ideological frame where the maker and possessor are attempting to convey a conscious message on a level that the manufacturers of tool artefacts are not. This is at least one serious problem with the Binford-Clarke definition of artefact: it can slip too easily from the specific and restricted definition they give to it (and to which those objects alone belong) to a looser metaphorical usage including objects that have more than just a cultural-functional meaning. If we do not distinguish the two fields of artefact, severe problems arise. Religious ideology provides but one blatant example (to which the relief sculptures and mosaics uncovered by the archaeologist in North Africa belong) of such problems. It is precisely in this sphere that widely divergent historians of critical insight have stressed the impossibility of the reconstruction of beliefs (the cultural-ideological system) from iconography alone and have illustrated the havoc and error that have resulted when the attempt has been made.⁴⁸ Clearly what is needed is a separate hermeneutic of semiology, a way of interpreting conscious signs with which this category of artefact is imbued; but this would place them

beyond the realm of archaeology *sensu stricto*.⁴⁹ But the tendency in North Africa has been to attempt an archaeological interpretation of these finds; in spite of a mass of individual archaeological reports on various aspects of the artistic production of the urban élites, there have been few synthetic studies that clearly separate the evidence into archaeological and other interpretive modes. For example, there can be little doubt that the most brilliant achievement of the North African bourgeoisie was the mosaic; North African city and rural sites have yielded thousands of examples of these visual reflections of the *mode urbaine*. But the tendency has been to study each one in isolation or to produce long lists and catalogues of types. It is a sad testimony to the lack of a conscious framework for the interpretation of this type of artefact that the first comprehensive analysis of this chapter of African artistic history did not appear until 1979.⁵⁰ More and more of the successful interpretations of art pieces, it might be noted, have abandoned the short-term influences of art history in favour of a longer-term approach of a semiotic type.⁵¹

If we turn from the long-term history of urban centres and to the African countryside, what contributions do we see being made by archaeology in this sphere? One outstanding example is, of course, the study of the vast system of centuriation (land survey), the identification of which was made possible by aerial photography. Related to this discovery is the study of patterns of irrigation systems in the southern arid regions of the Maghrib, also made possible by the use of aerial photography. One must emphasize again, however, that these advances, perhaps among the most important pertaining to a new understanding of the ancient agrarian economy of North Africa, resulted from concerns only tangentially related to archaeology. The initial discovery of the systems of centuriation covering north-eastern Tunisia was the result of photographic missions undertaken by the *Institut géographique national* of France in connection with the remapping of the countryside, primarily for reasons of a more efficient bureaucratic administration of land and property in the *Régence*.⁵² Again, the study of the field-patterns and systems of land survey discovered in east-central Algeria and published since 1973 by Jacqueline Soyer of the *Institut d'Afrique Méditerranéenne*

at Aix-en-Provence was made possible, at least in part, as we learn from a chance remark, by photographic missions staged by the French Air Force between 1955 and 1962, probably not for the elevated purposes of science and archaeology.⁵³ Apart from questions of motivation and context, however, there is no doubt that the technology of aerial photography has provided us with a viable source of information on the long-term history of the countryside that cannot be paralleled in scale or in kind by literary accounts of the subject. In particular, it has specified areas where Roman juridical categories were applied to the soil and to property relations, the extent and chronological development of the systems of centuriation, and the types of field patterns typically found in different regional contexts. The content of the juridical categories and the social nature of the property relations, and, to a great extent, the fine dating of the systems, still depend on literary rather than archaeological evidence.⁵⁴

Hence, the most profitable avenue to follow seems to be some sort of linkage between historical problems and archaeological method. In this context Christian Courtois' book *Les Vandales et l'Afrique* (1955) is relevant; its excellence does not lie in absolute accuracy of detail but rather in the delineation of a number of arguable theses about the process of "Romanization" within the limits of certain general themes of Maghribi history. The most famous of these was his thesis concerning the opposition between mountain and plain, and the consequence of this dichotomy for Roman rule: "La civilisation romaine s'était répandue à la manière des eaux. Elle avait envahi les plaines sans recouvrir les montagnes . . ."⁵⁵ The thesis is clearly testable by archaeology in some of its long-range consequences. Indeed a solitary scholar named Philippe Leveau, alone and with truly exiguous resources, has performed a series of archaeological investigations in a 700 square kilometre region that once formed the rural hinterland of the city of Caesarea (mod. Sharshall).⁵⁶ While lamenting the general lack of rural archaeology in North Africa, Leveau, following the lead of Agache, studied the archaeological remains in the mountain zone, first by establishing a typology of rural sites and secondly, by attempting to comprehend these as parts of a rural system.⁵⁷ From a surface survey that revealed

hundreds of villas, villages and hamlets, isolated farmsteads, processing and production centres, roads, towers, and other such works, he was able to postulate the existence of a dichotomous rural system. First, there was the system of villas and independent farmsteads tied to the urban centre of Caesarea and, opposed to this system, the villages and hamlets that seem to have formed an isolated and autonomous local society in the mountains. The central point that Leveau has been able to stress is that a "Roman" villa system did effectively penetrate, perhaps even dominate, the mountainous rural hinterland of Caesarea in antiquity, a development perhaps made possible by the lower level of technology of the "colonial" power of that time. The result, based on archaeological evidence alone, is that an historical problematic concerning "Romanization" must be rethought, with considerable repercussions on our understanding of the process and its potentialities.⁵⁸

Fortunately there has recently been an increasing number of rural surveys of this type, among them that by Ponsich in the hinterland of ancient Tangiers,⁵⁹ that by Peyras and Maurin in the north-eastern Bagradas valley in Tunisia,⁶⁰ and that by Acquaro of the Cap Bon Peninsula.⁶¹ All the surveys point to some general conclusions worthy of note. First, they all agree on the density of settlement patterns and the complex interdependence of villas, isolated farmsteads, hamlets, and towns in a rural economic matrix, and they are able to be fairly precise about the actual geographical extension of these systems. Second, regional and temporal variations in these patterns can be specified. Increasingly the villa-based exploitation of the countryside is seen to have a considerable pre-Roman or Hellenistic history.⁶² Moreover, in certain regions, such as the Tangiers hinterland, the villa system seems to have had larger individual units of exploitation and a greater geographical extension in the pre-Roman period; around Tangiers the recession began in the early Empire, apparently due to the rise of olive-producing villa complexes in the lower Guadalquivir valley in southern Spain.⁶³ Trends and developments such as these are archaeological knowledge; it is most unlikely that they would have been revealed by any combination of literary evidence alone.

When the specificity of the literary record can be placed within a

definable long-term context provided by archaeology, the results can be most satisfactory. Daniele Manacorda's work on Tripolitanian ceramics and the exportation of olive oil is a good illustration. It has been observed that amphorae stamps alone are an insufficient guide to economic history, that some typology and quantitative analysis are also required.⁶⁴ At least a typology had been established for Byzacene and Tripolitanian amphorae of the high and late Empire, and it was on this basis that Manacorda was able to build.⁶⁵ He had as his goal the elucidation of the relations between the ruling class of the Empire, both at the centre and in the provinces, and the politico-administrative apparatus of the state. Although both literary and epigraphic evidence was used in approaching the problem of olive-oil exports from Tripolitania, Manacorda correctly regarded the archaeological evidence as a necessary part of "the history of production and consumption." The literary evidence established the connection between the production of olive oil in Tripolitania and the *annona* policy of the state, that is, the free distribution of olive oil on a daily basis to the plebs of Rome (and Italy?) from the time of the Severan emperors onward.⁶⁶ The epigraphic data attest a new imperial procurator *ad olea comparanda per regionem Tripolitanam* of late second or early third-century date, and provide the readings of numerous amphorae stamps.⁶⁷ The archaeological evidence consists of three distinctive amphora types identified by Zévi-Tchernia, of which type three, dominant in the third and fourth centuries, betokens a considerable export of olive oil from Tripolitania. From this combined evidence Manacorda is able to demonstrate an integral connection between the long-term history of growing exports from Tripolitania and the gradual regional shift in important centres of production in the western Mediterranean, and leading members of the ruling élite at Lepcis Magna (Q. Granius Caelestinus, L. Septimius Aper, C. Fulvius Plautianus, L. Silius Plautius Haterianus, P. Cornelius Bassus Severianus), including the Severan emperors themselves.⁶⁸ Thus a set of specific historical "events" are located within the context of a rise in the Tripolitanian contribution to the cycle of production and consumption of olive oil that effectively balanced a slight decline in the previously dominant centres of production along the Guadalquivir in Spain.⁶⁹ That is to

say, archaeology has effectively provided the broader Mediterranean context within which the short-term changes recorded by the actors themselves in their literary texts and records can be understood more effectively.

But a firm grip must be maintained on the spheres and limitations of either type of data. There is as much a tendency amongst historians to do "counterfeit archaeology" as there is amongst archaeologists to write "counterfeit history." My last example centres on the problem of water and land. During the colonial period, committed as the administration was to the view of a more developed Roman past, there was considerable impetus to re-establish the water-control systems constructed by the Romans as a means of reviving the "lost prosperity" of the Maghrib. Colonial administrators, regional government bureaucrats, and army officers were engaged in the purposeful search for Roman waterworks that might be made to function again. In fact, all the comprehensive surveys of Roman hydraulic schemes in North Africa were the result of direct orders issued by colonial governors-general who were motivated by pragmatic interest.⁷⁰ But these surveys, given their official impetus, directives, and assumptions, tended to miss the very object of their inquiry. In their single-minded pursuit of "Roman" hydraulic systems, the officials prepared long catalogues of countless cisterns, wells, storage basins, piping systems, and aqueducts which struck at least one observer even at the time as lacking any methodology or purpose and, as he put it, as arid as any desert in their "desperate monotony."⁷¹ The simple fact is that almost all the urban-based waterworks that were readily identifiable as "Roman" were part of systems of consumption rather than production, a problem that never seems to have been broached by the scientific collectors of archaeological data. Nor was it likely to have been, given their assumptions.

But the problem of the colonial context aside, any attempt to make a specific connection between archaeological context and the specificity of the literary data remains extremely difficult. Take the case of one irrigation system, the details of whose operation are known from a lengthy Latin inscription from the ancient municipality of Lamasba ('Aïn Merwâna) in north-central Algeria. Dated precisely to the reign of the emperor Elagabalus, the inscription informs us of the names of the owners

and gives a precise number of irrigation plots, the size of these holdings, the time and order in which they are to receive water, the amount of water they are to receive, and the water regime according to which they are to receive it.⁷² Although the findspot of the inscription is known and the source of the water is specified on the inscription (the spring called *Agua Claudiana*), numerous attempts to identify the actual archaeological remains of the scheme have ended in failure.⁷³ But the scheme must be one of a limited number of types of irrigation system that have been identified on aerial photographs of the *limes* region. Even if such an identification were to be made, however, the overriding problem would remain: the archaeological data and the literary data basically function at different levels. The literary data of the Lamasba inscription are the best sort of illumination that can be thrown on an individual irrigation system at one point in time; and it tells us directly about the cultural dynamic of the scheme in a way that archaeology could do only with great difficulty: the ostensible degree of "Romanization" of the proprietors through the nomenclature, the social distribution of wealth, the modalities of land ownership (amongst women, heirs, familial groupings, joint owners, etc.), the distribution of water, and the rôle of official arbitrators in settling disputes amongst the farmers. But archaeology clearly can inform us in a much better way about the other aspects of the ancient irrigation economy in North Africa: the full extent of such systems in the arid lands, the relationship of the different types of systems to one another, and their fluctuation over long periods of time. However, it is revealing that the only work I know of that takes such an approach, using aerial photographs, has been done not by a Classical archaeologist or historian but by a geographer, and there is no sign as yet that his work has been recognized or that his lead will be followed by the Classical scholars.⁷⁴

In view of the above survey, it perhaps comes as no surprise to the reader that the author became progressively more pessimistic about the track-record of the contribution of archaeology to our knowledge of Roman Africa. In order that the analysis not be reduced to a simple litany of complaint, however, some specific conclusions should be drawn. The failure, it seems, is due principally to a lack of understanding of

the field of archaeology itself. Archaeology, like history, remains inescapably based in the present; we can safely say that no model of behaviour, cultural change, or processual dynamic can emerge from the past artefactual record alone. These must be provided by the historian, the anthropologist, and the sociologist.⁷⁵ The basic division remains: I do not expect the type of evidence provided by archaeology at its best, as armed with a whole battery of new technologies, to change very much, even if its quantity is vastly increased. All that the "new archaeology" seems to have offered, apart from a correct definition of the field, is the mistaken view that more and more of the same data and increased precision of the technology will bring about a qualitative revolution.⁷⁶

In my view, archaeological knowledge has all the relative merits and restrictions of long-range social history. It can tell us much about the structure of the material world, as is indeed reflected in most archaeological jargon: "types, spectra, distributions, clusters, settlement patterns," and so on. This path leads from the artefact to a sort of "sociology," and hence Rodinson's (perhaps excessively) glowing appraisal of Tchalenko's work in Syria.⁷⁷ But the specific short-term causes and events that are an integral part of history, and the ideological and material interactions within human communities that produced commensurable artefacts remain beyond the reach of archaeology. On a more tactical level, my optimism is born of the fact that isolated individuals, working with very limited resources, such as the indefatigable Philippe Leveau, seem to have sensed the perimeters of the technology and its potentialities, and have probably achieved more in respect of any contribution to our understanding of the history of North Africa than have the efforts of numerous countries, their *équipes archéologiques*, UNESCO, and the financial resources poured out in the last decade at Carthage.⁷⁸

The problem is knowing what archaeology can do best. I can only paraphrase Mr. Eastwood: "A good methodology has got to know its limitations."

NOTES

Sections of this paper were delivered to a seminar entitled "The Archaeology of Knowledge" at the University of Birmingham, June 1977, and to the session "The Archaeology of the Provinces of the Roman Empire," at the annual meeting of the American Philological Association, Boston, December 1979. I would like to thank Dr. C. Wickham and Ms. S. Robinson for their comments (Birmingham), and, respectively for their criticism and encouragement, Dr. C.M. Wells and Dr. E.M. Wightman (Boston). Naturally they are exonerated from any support of the views expressed, which are those of the author alone.

¹ M. Foucault, *L'archéologie du savoir* (Paris 1969) 15 = *The Archaeology of Knowledge*, trans. A.M. Sheridan Smith (London 1972) 7.

² D.L. Clarke, *Analytical Archaeology* (London 1968) 12-13 (I note his apologies to Gertrude Stein). He notes the sense of crisis and the "new consciousness" since the 1950s (see p. xiii). In much the same vein, L.R. Binford, "Archeology as Anthropology," *American Antiquity* 28 (1962) 217-25 = *An Archeological Perspective* (New York and London 1972) 20-32, at p. 20: "It is argued that archeology has made few contributions to the general field of anthropology with regard to explaining cultural similarities and differences"

³ A.M. Snodgrass, *Archaeology and the Rise of the Greek State* (Inaugural Lecture, Cambridge 1977) 4-7; much earlier, and in another field, see J.A. Wilson, "Archaeology as a Tool in Humanistic Studies," *JNES* 1 (1941) 3-9; V.G. Childe, "The Sociological Interpretation of Archaeological Data," ch. 5 in *Social Evolution* (London 1951) 57-73. Strong on enumerating specific problem areas, though weak on general analysis, is S.C. Humphreys, "Archaeology and the Social and Economic History of Classical Greece," *PP* 116 (1967) 374-400 = ch. 4 in *Anthropology of the Greeks* (London 1978) 109-29.

⁴ Clarke (at n. 2) 13, in a programme linked to the perceived "crisis;" so on p. 20: "The data studied are so inherently unlike those of other disciplines that archaeology must erect its own systematic approach or perish as a separate study."

⁵ Clarke, *ibid.* 134 and 186.

⁶ I mean hermeneutic in the sense of a reading or interpretation of a text, not in the psychologistic or existential sense championed by Dilthey and Heidegger; see H.-G. Gadamer, *Truth and Method* (New York 1975) = *Wahrheit und Methode: Grundzüge einer philosophischen Hermeneutik* (Tübingen 1960) 261 f., 361 ff. Cf. his "Vom Zirkel des Verstehens," in *Festschrift Martin Heidegger zum siebzigsten Geburtstag* (Pfullingen 1959) 24-34.

⁷ L.R. Binford, "Archeological Perspectives," ch. 1 in S.R. and L.R. Binford, eds., *New Perspectives in Archeology* (Chicago 1968) 5-32 at 21 = *An Archeological Perspective* (1972) 95, where it is suggested that "material culture" can and does represent the structure of the "total cultural system;" Clarke (at n. 2) 21, using the "observer-fact" dichotomy; see the critique of Leone (1975) 134 f. (cited at n. 8).

⁸ See especially M.P. Leone, "Archeology as the Science of Technology . . . ," ch. 9 in C.L. Redman, ed., *Research and Theory in Current Archeology* (New York 1973) 125-50, at 126-27, where he demonstrates with devastating effectiveness the proposition that successes in interpreting archaeological data, such as those of Murra and McC. Adams, have depended not on models emanating from the data themselves, but on the wholesale importation of models from the social sciences that have been able to study living systems.

⁹ Clarke (at n. 2) 12; cf. Redman, (at n. 8) 5, who likewise excludes from the critique archaeology as practised by scholars in "such disciplines as Classics, Art History or Near Eastern Languages;" and see too the attempted rejoinder by C. Renfrew, "Space, Time and Polity," ch. I.7 in J. Friedman and M.J. Rowlands, eds., *The Evolution of Social Systems* (London 1978) 89-122.

¹⁰ E.g. Clarke (loc. cit.), as Binford, constantly directs his analysis to prehistorians and allows (e.g. p. 12 f.) for "other archaeologies;" see M.I. Finley, "Archaeology and History," *Daedalus* 100 (1971) 168-86 = *Historical Studies Today*, ed. F. Gilbert and S.R. Graubard (New York 1972) 281-99 = ch. 5 in *The Use and Abuse of History* (London 1975) 87-101, at p. 87, for the exclusion of all types of written documents from the definition of "archaeological evidence."

¹¹ For a general critique of the place of ideology in relation to

human interests, see J. Habermans, *Knowledge and Human Interests*, trans. J.J. Shapiro (London 1977), and, more specifically, his "Technology and Science as 'Ideology'," ch. 6 in *Toward a Rational Society* (London 1971) 81-122.

¹² See, for example, the history of the discipline in G. Daniel, *The Origins and Growth of Archaeology* (New York 1967); cf. Leone (1973) 129.

¹³ A. Laroui, *A History of the Maghrib: An Interpretative Essay*, trans. R. Manheim (Princeton 1977) ch.1 = *L'histoire du Maghreb: un essai de synthèse* (Paris 1976); on the decolonization of archaeology in Europe itself see Wightman (at n. 25 below) 585, n. 2.

¹⁴ M. Bénabou, "Les romains ont-ils conquis l'Afrique?" *Annales (E.S.C.)* 33 (1978) 83-8; P. Leveau, "La situation coloniale de l'Afrique romaine," *ibid.* 89-92; and Y. Thébert, "Romanisation et déromanisation en Afrique: histoire décolonisée ou histoire inversée?" *ibid.* 64-82.

¹⁵ E.g. M. Bénabou, "Résistance et romanisation en Afrique du Nord sous le Haut-Empire," in M. Pippidi, ed., *Assimilation et résistance à la culture gréco-romaine dans le monde ancien, Travaux du VI^e Congrès international d'Etudes Classiques* (Paris 1976) 367-75; *La résistance africaine à la romanisation* (Paris 1976); A. Deman, "Matériaux et réflexions pour servir à une étude de développement et sous-développement dans les provinces de l'Empire romain," *ANRW* 2.3 (1975) 3-97; T. Kotula, "Les Africains et la domination de Rome," *DdHA* 2 (1976) 337-58; and, on a more specific scale, Y. Thébert, "La romanisation d'une cité indigène d'Afrique: Bulla Regia," *MEFRA* 85.1 (1973) 247-310.

¹⁶ See, e.g., Dureau de la Malle, *L'Algérie. Histoire des guerres des Romains, des Byzantines et des Vandales* (Paris 1852); *Résumé de l'histoire ancienne de l'Algérie et de la Régence et de Maroc avant la conquête musselmanne*, 2 vols. (Paris 1864); and H. Feuilleter, *Les Romains en Afrique: Episodes de l'histoire de l'Algérie* (Limoges et Lille 1868).

¹⁷ Compare A.J. Toynbee, *A Study of History*, I (Oxford 1934, 1962) 5-7, on the respective cases of the Seleucid Empire and Ptolemaic Egypt, and his remarks on the "Industrial Revolution mentality" that causes scholars to mine the more abundant material evidence simply because it exists, irrespective of its relative historical value.

¹⁸ T.R.S. Broughton, *The Romanization of Africa Proconsularis* (Baltimore 1929; repr. New York 1968); I could find no use of archaeological data other than the archaeological atlases (Gsell, AAA, and Merlin, AAT) for the purposes of site identification, in which case the evidence of inscriptions and the itineraries is still determinant.

¹⁹ P. Romanelli, *Storia delle province romane dell'Africa* (Rome 1959).

²⁰ Bénabou, *La résistance africaine* (at n. 15), and M. Rachet, *Rome et les Berbères: une problème militaire d'Auguste à Diocletien* (Bruxelles 1970).

²¹ All the surveys of archaeological work printed in the annual issues of *BAA*, *BAM*, and *RAF* clearly justify this conclusion; the number of rural sites excavated in any one year usually is less than five as opposed to the many dozens of urban sites reported in detail. So, too, a glance at the contents of P. Romanelli, *Topografia e archeologia dell'Africa romana* (Torino 1970): a maximum of eleven to eighteen pages on the countryside in a large survey volume of over seven hundred pages; A. di Vita, "Leggende 'Topografia e archeologia dell'Africa romana' di Pietro Romanelli: considerazioni, note, segnalazioni," *QAL* 7 (1975) 165-87 does not alter this imbalance.

²² As a perusal, for example, of the indices of *BCTH* for the years concerned will readily show; on Saumagne see J. Berque, *French North Africa between Two World Wars*, trans. J. Stewart (London 1967) 202, 267, 287; on Carton see E. Thepenier, *RSAC* 55 (1923-1924) 351-52.

²³ Wightman (at n. 25 below) 585 n. 4: "If British archaeologists may fairly claim to have taken the lead in the study of rural settlement patterns, it is partly because the gap between prehistorian and student of Roman Britain has never been so great in that country, so that techniques have been readily adopted and borrowed." That is to say, there were no unnecessary diversions from proper archaeological work; according to one (oral) source not even one intact standing stone column of the Roman period has been found in Britain; the contrast with North Africa is obvious.

²⁴ S. Applebaum, "Roman Britain," in H.P.R. Finberg, ed., *The Agrarian History of England and Wales, I, 2, A.D. 43-1042* (Cambridge 1972)

1-277.

²⁵ R. Agache, "La campagne à l'époque romaine dans les grandes plaines du Nord de la France d'après les photographies aériennes," *ANRW* 2.4 (1975) 658-713, based on his three major works: *Vues aériennes de la Somme et recherche du passé: Bulletin spécial de la Société de Pré-histoire du Nord*, 5 (Amiens 1962); *Archéologie aérienne de la Somme*, *ibid.* 6 (Amiens 1964); and *Détection aérienne de vestiges préhistoriques, gallo-romaines et médiévaux dans le bassin de la Somme et ses abords*, *ibid.* 7 (Amiens 1970). E.M. Wightman, "The Pattern of Rural Settlement in Roman Gaul," *ANRW* 2.4 (1975) 584-657. Contrast the evidence used in J. Percival, *The Roman Villa: An Historical Introduction* (London 1976) 61-6 on North Africa (some Tripolitanian fortified farms and sketchy material from Ponsich on the Tangiers region) with the fulsome treatment of the Gauls, Germanies and Britain.

²⁶ Wightman (at n. 25) 584.

²⁷ M.I. Rostovtzeff, *Social and Economic History of the Roman Empire*, 2 vols. (Oxford 1926; rev. ed., P.M. Fraser, Oxford 1957) 687, n. 96 (the "Seigneur Iulius" mosaic); 688, n. 99 (the arch of Septimius Severus at Lepcis and some Tripolitanian relief sculpture). See A.D. Momigliano, "M.I. Rostovtzeff," *The Cambridge Journal* 7 (1954) 334-46 = ch. 5 in *Studies in Historiography* (London 1966) 91-104, at 91. Cf. G.W. Bowersock, "The *Social and Economic History of the Roman Empire* by Michael Ivanovitch Rostovtzeff," *Daedalus* 103 (1974) 15-23, at 19-20; Finley (at n. 10) 87-8.

²⁸ R.M. Haywood, "Roman Africa," in T. Frank, ed., *An Economic Survey of Ancient Rome*, IV (Baltimore 1938) 1-119.

²⁹ B.H. Warmington, *The North African Provinces from Diocletian to the Vandal Conquest* (Cambridge 1954), ch. 5, "The Country," 55-68: two mosaics: 64, n. 2, and 65, n. 1.

³⁰ W.H.C. Frend, *The Donatist Church: A Movement of Protest in Roman North Africa* (Oxford 1952; 2nd ed. 1971), esp. ch. 3, "Town and Country in Roman North Africa," 32-47; other than archaeological material relating to the frontiers and mosaics, there is one survey (by Gsell and Graillot, 39, n. 2), one villa site (Alquier, baths at Oued-Athmenia), the survey by M. Martin and F. Logeart, *Les vestiges du Christianisme*

antique dans la Numidie centrale (Alger 1942), and olive presses around Sufetula (46, n. 5): a sampling of archaeological data, but not an account where it is integral.

³¹ Frend, *ibid.* vi.

³² J. Gascoü, *La politique municipale de l'Empire romain en Afrique proconsulaire de Trajan à Septime-Sévère* (Rome 1972) 12 f. In attempting to count the cases of municipal advancement explained by Gascoü, I arrived at the following approximate distribution (multiple explanations and some haziness on Gascoü's part explain some of the overlap): strategic-military factors: 8; agricultural wealth: 20; "degree of Romanization:" ca. 6. P. Romanelli, "La politica municipale romana nell'Africa proconsulare," *Athenaeum* 53 (1975) 144-71, at 154 f., rightly contests Gascoü's rejection of imperial *beneficia* as the leading causal explanation (cf. p. 170), although he lodges this objection only for some of the more obvious exceptions to Gascoü's analysis.

³³ Gascoü, *ibid.*, e.g., 34 (Bulla Regia), 73-74 (Hadrumetum), 79 (Lepcis), 125 (Thizika), 125 (Avitta Bibba), 133 (Zama), 134 (Althiburos), 136 (Thaenae), 137 (Ulusippira), 167 (Pupput). See T. Kotula, "Snobisme municipale ou prospérité relative? Recherches sur le statut des villes nord-africaines sous le Bas-Empire," *Ant.Afr.* 8 (1974) 111-31, at 116.

³⁴ For example, it was realized long ago that there were many wealthy, "Romanized," large towns with Roman-style artistic and architectural façades that did not achieve advancement until relatively late (e.g. Dougga, Roman Thugga). This was explained by H.G. Pflaum, "La romanisation de l'ancien territoire de la Carthage punique à la lumière des découvertes épigraphiques récentes," *Ant.Afr.* 4 (1970) 75-117, as a correlate of the domination held by the colony of Carthage over its *pertica* or *territorium* -- it artificially retarded these cases of advancement. Gascoü follows Pflaum in this explanation (e.g. 145, Vina; 146, Segermes; and 189, Municipium Septimium). Given his materialistic explanatory framework, Gascoü is at a loss to explain adequately the elevation of truly insignificant backwater communities such as Turris Tamalleni (135: but surely there were dozens of other such "strategic points" on the *Limes Tripolitanus*; why advance this one alone?), or Gens Septimiana (191), when other wealthier and larger centres do not advance at all.

Given his positive approach to the subject, Gascou only studies cases of known advancement and does not consider the large number of negative cases that would greatly affect his argument. Rejecting this argument, I would also deny the applicability of other "archaeological" methods, such as central-place theory, for predicting status ranking of communities; see, e.g., I. Hodder and M. Hassall, "The Non-random Spacing of Romano-British Walled Towns," *Man* N.S. 6 (1971) 391-407.

³⁵ Especially since the only specified known causes of municipal status advancement in Africa are patronal: Gigthis (*CIL* viii, 22707, 22737 and *ILTun* 41), Utica (*Aul.Gell. NA* 16.13), and Volubilis (*ILMar* 116). Gascou often seeks to deny this explicit testimony in favour of an economic explanation (see, e.g., 119 f., 122 f., and 142 f.). Much better, see F. Millar, *The Emperor in the Roman World* (31 B.C.-A.D. 337) (London 1977) "Provincial Communities: the Acquisition of City Statutes," 493-509.

³⁶ The fundamental argument of Finley (at n. 10) 99-101.

³⁷ K. Hopkins, *Conquerors and Slaves: Sociological Studies in Roman History*, I (Cambridge 1978) x.

³⁸ The approach delineated by F. Braudel, "Histoire et sciences sociales: la longue durée," *Annales (E.S.C.)* 13 (1958) 725-53. See, however, the criticism of W.W. Rostow, "Histoire et sciences sociales: la longue durée," *ibid.* 14 (1959) 710-18; W. Kula, "Histoire et économie: la longue durée," *ibid.* 15 (1960) 294-313. See further, T. Stoianovich, *French Historical Method: the Annales Paradigm* (Cornell 1976), and "Social History: Perspective of the Annales Paradigm," *Review* 1.3-4 (1978) 19-48, and E. Le Roy Ladurie, "Événement et longue durée dans l'histoire sociale . . .," in *Le territoire de l'historien* (Paris 1973) 169-86, trans. G. and S. Reynolds, *The Territory of the Historian* (London 1979).

³⁹ Some examples of simple disproof of literary theses by archaeology from North Africa would be the rejection of the literal meaning of epigraphic phrases such as *vetustate delapsa* (see, e.g. M. Euzennat, *Africa* 5-6 [1978] 139), or the rejection of an hypothetical Diocletianic withdrawal from the *limites* in western Caesariensis (see, e.g. P. Salama, "Occupation de la Maurétanie Césarienne occidentale sous le Bas-Empire,"

in *Mélanges . . . Piganiol* (Paris 1966) 1291-311 -- though even here his proofs are, at base (coins and milestones), still literary and epigraphic; cf. R.I. Lawless, "The Lost Berber Villages of Eastern Morocco and Western Algeria," *Man* N.S. 7 (1972) 114-21.

⁴⁰ J. Toutain, *Les cités romaines de la Tunisie: essai sur l'histoire de la colonisation romaine dans l'Afrique du Nord* (Paris 1896), and "Le progrès de la vie urbaine," *Mélanges Cagnat* (Paris 1912) 319-47.

⁴¹ See, e.g., J. Lassus, "Adaptation à l'Afrique de l'urbanisme romain," *VIIIe Congrès international d'archéologie classique* (Paris 1963) 245-59; and two important studies by P.A. Février, "Notes sur le développement urbain en Afrique du Nord. Les exemples comparés de Djémila et de Sétif," *CArch* 14 (1964) 1-47, and "Aux origines de l'occupation romaine dans les Hautes Plaines de Sétif," *CT* 15 (1967) 57-64 -- other than the excavation at Sétif, most of the other material is epigraphic, mainly from the *castella* in the Sétif plain.

⁴² P. Leveau et J.L. Paillet, *L'Alimentation en eau de Caesarea de Maurétanie et l'aqueduc de Cherchell* (Paris 1976) (see the reviews by C.R. Whittaker, *JRS* 68 [1978] 220-1, and J. Andreau, *Annales (E.S.C.)* 33.2 [1978] 357-60). They outline the problem in general for North Africa, with references to other Mediterranean regions, in ch. 1, 21-41.

⁴³ Leveau et Paillet, *ibid.* 19-20; see F. Rakob, "Das Quellenheiligtum in Zaghouan und die römische Wasserleitung nach Karthago," *MDAI(R)* 81 (1974) 41-89.

⁴⁴ Leveau et Paillet, *ibid.* 166-7.

⁴⁵ R. Rebuffat, "Enceintes urbaines et insécurité en Maurétanie Tingitane," *MEFRA* 86 (1974) 501-22; see especially the secondary studies by Février, Harmand, Roblin, and Seston cited by him. Cf. R. Evans, "The Rights of Retreat and the Rights of Exclusion: Notes Towards the Definition of the Wall," *Architectural Design* 41 (1971) 335-9.

⁴⁶ Rebuffat (1974) 513 f. for his conclusions (cf. 518 f. for one refutation of a "military" functional interpretation of the walls of Tipasa).

⁴⁷ R. Rebuffat, "Maisons à péristyle d'Afrique du Nord, I et II," *MEFR* 81 (1969) 659-724, and 86 (1974) 445-99.

⁴⁸ See, e.g., A.L. Oppenheim, *Ancient Mesopotamia* (2nd ed., Chicago

1977) 172 f. on temples, and his more general remarks at 171-83 on "Why a 'Mesopotamian Religion' should not be written," which did not prevent the attempt in 1979. See also R.L. Gordon, "Franz Cumont and the Doctrines of Mithraism," ch. 14 in J.R. Hinnells, ed., *Mithraic Studies* (Manchester 1975) 215-48, at 246.

⁴⁹ E.g. U. Eco, *A Theory of Semiotics* (Bloomington and London 1976), a revised version of his *La struttura assente* (Milan 1968); R. Barthes, *Système de la Mode* (Paris 1976); and T.A. Sebeok, ed., *The Tell-Tale Sign: A Survey of Semiotics* (Lesse 1975).

⁵⁰ Catalogues such as P. Gauckler, *Inventaire des mosaïques de la Gaule et de l'Afrique*, I (Paris 1910); L. Foucher, *Inventaire des mosaïques . . . (Sousse)* (Tunis 1960); and M. Alexander et al., *Corpus des mosaïques de Tunisie*, I, 1-3 (Tunis 1973-1976). Individual studies include those of T. Précheur-Canonge, *La vie rurale en Afrique d'après les mosaïques* (Paris 1965); L. Foucher, *Navires et barques figurés sur des mosaïques découvertes à Sousse et aux environs* (Tunis 1952). But only K.M.D. Dunbabin, *The Mosaics of North Africa: Studies in Iconography and Patronage* (Oxford 1979) could reasonably be considered to be in the class of a synthetic archaeological analysis of the subject.

⁵¹ See, e.g., P.A. Février, "Colonisation romaine et forme artistique dans les provinces de la Méditerranée occidentale," in *Actes du 2e Congrès international d'études de cultures de la Méditerranée occidentale* (Alger 1976) 49-102, and "L'art funéraire et les images des chefs indigènes dans la Kabylie antique," in *Actes du 1^{er} Congrès d'études des cultures méditerranéennes d'influence arabo-berbère* (Alger 1973) 152-69.

⁵² C. Saumagne, "La photographie aérienne au service de l'archéologie en Tunisie," *CRAI* (1952) 287-301, at 287-8; R. Chevallier, "Essai de chronologie des centuriations romaines en Tunisie," *MEFR* 70 (1958) 61-128, at 61; and A. Caillemer et R. Chevallier, "Les centuriations de l'«Africa vetus,» sous l'oeil d'Asmodée . . .," *Annales (E.S.C.)* 9 (1954) 433-60, at 433-4 (photographs taken by *IGN* between 1947-1951).

⁵³ J. Soyer, "Les cadastres anciens de la région de Saint-Donat (Algérie)," *Ant.Afr.* 7 (1973) 275-96, and "Les centuriations romaines en Algérie orientale," *ibid.* 10 (1976) 107-80.

⁵⁴ A. Caillemer et R. Chevallier (pref. A. Piganiol), *Atlas des*

centuriations romaines de Tunisie (3e éd., Paris 1959-1960); cf. R. Chevallier, "La centuriation et les problèmes de la colonisation romaine," *Etudes rurales* 3 (1961) 54-80.

⁵⁵ C. Courtois, *Les Vandales et l'Afrique* (Paris 1955) 121.

⁵⁶ P. Leveau, "Prospections archéologiques autour de Cherchel: méthodes et premier bilan," *RHCM* 8 (1970) 7-21; "Paysanneries antiques du pays Beni-Menacer, à propos des 'ruines romaines' de la région de Cherchel (Algérie)," *BCTH N.S.* 8b (1972) 3-26; "Paysans maures et villes romaines en Maurétanie Césarienne centrale (la résistance des populations indigènes à la romanisation dans l'arrière-pays de Caesarea de Maurétanie)," *MEFRA* 87.2 (1975) 857-71, and his forthcoming contribution to the *ANRW* volume on North Africa.

⁵⁷ Leveau 1972 (at n. 56) 4. The malady is not new; as he himself notes, Gsell lamented the same situation around the turn of the century *MAA* 2 [1901] 28 f.), and Courtois (at n. 55) 12, n. 5, did the same in the mid 1950s.

⁵⁸ Leveau 1972 (at n. 56) 19-22 and 25, and his "L'opposition de la montagne et de la plaine dans l'historiographie de l'Afrique du Nord antique," *Annales de géographie* 86 (1977) 201-5.

⁵⁹ M. Ponsich, *Recherches archéologiques à Tanger et dans sa région* (Paris 1970), and his "Contribution à l'Atlas archéologique du Maroc: région de Lixus," *BAM* 6 (1966) 377-423; cf. the other parts of the same survey: A. Luquet, "Contribution à l'Atlas archéologique du Maroc: région de Volubilis," *ibid.* 5 (1964) 291-300; "Contribution à l'Atlas archéologique du Maroc: région du Rharb," *ibid.* 6 (1966) 365-75; and M. Tarradell, "Contribution à l'Atlas archéologique du Maroc: région de Tétouan," *ibid.* 6 (1966) 425-43.

⁶⁰ L. Maurin et J. Peyras, *Ureu, Municipium Uruensium. Recherches archéologiques et épigraphiques dans une cité romaine inédite d'Afrique proconsulaire* (Paris 1974) = *CT* 19 (1971) 11-103; cf. J. Peyras, "Le *Fundus Aufidianus*: étude d'une grande domaine romaine de la région de Mateur (Tunisie du Nord)," *Ant.Afr.* 9 (1975) 181-222.

⁶¹ E. Acquaro, P. Bartolini, E. Ciasca, and M. Fantar, *Prospezione archeologica al Capo Bon* (Rome 1973).

⁶² Acquaro et al., *ibid.*; Ponsich (at n. 59) 202-4. 215-21; cf.

P. Cadenat, "Un établissement pré-romain dans la région de Tiaret (Oranie)," *Ant.Afr.* 6 (1972) 29-58, and "La villa berbero-romaine d'Aïn-Sarb (Département de Tiaret, Algérie)," *ibid.* 8 (1974) 73-88.

⁶³ Ponsich (at n. 59) 204, 215, 218, 273-79, and 282; the trend was already apparent, of course, from the archaeology of Tripolitania -- see D. Oates, "The Tripolitanian Gebel," *PBSR* 21 (1953) 81-117, and R.G. Goodchild, "Roman Sites on the Tarhuna Plateau of Tripolitania," *ibid.* 19 (1951) 43-77 = J. Reynolds, ed., *Libyan Studies* (London 1976) 72-106; cf. A. di Vita, "Il limes romano di Tripolitania nella sua concretezza archeologica e nella sua realtà storica," *Lib.Ant.* 1 (1964) 65-98.

⁶⁴ A. Tchernia, "Les amphores romaines et l'histoire économique," *JS* (1967) 216-34, esp. 219. Cf. F. Zévi, "Appunti sulle anfore romane," *ArchCl* 18 (1966) 208-47.

⁶⁵ F. Zévi et A. Tchernia, "Amphores de Byzacène au Bas-Empire," *Ant.Afr.* 3 (1969) 173-214, esp. 192-5 for the Tripolitanian types; see A. Carandini, "Produzione agricola e produzione ceramica nell'Africa di età imperiale: appunti sull'economia della Zeugitana e della Byzacena," *Studi Miscellanei* 15 (1969-1970) 95-119. For the results, see D. Manacorda, "Testimonianze sulla produzione e il consumo dell'olio tripolitano nell'III secolo," *DdArch* 9-10 (1976-1977) 542-601, at 563-6.

⁶⁶ Manacorda *ibid.* 555-62; cf. HA, *Sept.Sev.* 18.3; 23.2; *Cl.Alb.* 12.7; *Alex.Sev.* 22.2; *Aur.* 48.1; *Chron. ann.* 359, p. 158 (ed. Mommsen); *Aur.Vict. Caes.* 41.19.

⁶⁷ Manacorda *ibid.* 543-55; cf. *AE* 1973, 76, and J. Reynolds, *JRS* 66 (1976) 189.

⁶⁸ Manacorda *ibid.* 566-79; cf. M. Torelli, "Per una storia della classe dirigente di Leptis Magna," *RIL* (ser. 8) 28 (1973) 377-409, for an identification of these individuals within the ruling élite.

⁶⁹ Manacorda *ibid.* 582-92; cf. *DdArch* 7 (1973) 415 f.: just as Ponsich suspects, an actual decline in olive production in the Tangiers region under the influence of the development of the Guadalquivir valley in the period of the early Empire.

⁷⁰ P. Gauckler, *Enquête sur les installations hydrauliques romaines en Tunisie* (Tunis 1897-1901), containing numerous reports by colonial officials and military officers, was the result of an inquiry ordered by

the *Resident Général*, René Millet. S. Gsell, *Enquête administrative sur les travaux hydrauliques anciens en Algérie* (Paris 1902) = *NAMS* 12 (1902), was ordered by the *Gouverneur-Général* of Algeria, M. Jonnart. M. du Coudray la Blanchère compiled his survey "L'aménagement de l'eau et l'installation rurale dans l'Afrique ancienne," *NAMS* 7 (1897) 1-108 (Paris 1895), covering the Enfidaville region of north-eastern Tunisia between 1885-1891, at the behest of the *Resident Général*, J. Massicault, whose direct concern it was to restore the past agricultural prosperity of the Roman period. The trend persisted right up to the year of Algerian independence: J. Birebent, *Aquae Romanae. Recherches d'hydraulique romaine dans l'est Algérien* (Alger 1964) was, once again, a project intended to uncover "Roman" waterwork systems that could be restored to their former function. Birebent was suitably optimistic, both concerning the identification of Roman remains and the viability of their restoration.

⁷¹ L. Carton, "Hydraulique dans l'antiquité en Barbarie," *RT* 19.93 (1912) 221-30, at 222-3.

⁷² *CIL* viii.18.587 (=4440) = *ILS* 5793 (Lamasba, mod. 'Aïn Merwâna); cf. F.G. de Pachtère, "Le règlement d'irrigation de Lamasba," *MEFR* 28 (1908) 373-400.

⁷³ De Pachtère, *ibid.* 388 f., cf. Birebent (at n. 70) 390 f.

⁷⁴ H. Achenbach, "Römische und gegenwärtige Formen der Wassernutzung im Sahara-Vorland des Aures (Algerien)," *Die Erde* 104 (1973) 157-75.

⁷⁵ See B.G. Trigger, "The Future of Archaeology is the Past," ch. 7 in C.L. Redman, ed., (at n. 63) 95-111, at 108-9.

⁷⁶ See A.C. Spaulding, "Archeology in the Active Voice: the New Anthropology," ch. 23 in C.L. Redman, ed., *ibid.* 337-54, at 349, a glaring self-revelation of the poverty of this aspect of the new methodology in an otherwise aggressively assertive article about the benefits alone; see the excellent critique by Sir E. Leach of this functionalist-behaviourist trend in the new archaeology in his concluding remarks to C. Renfrew, ed., *The Explanation of Cultural Change* (London 1973).

⁷⁷ M. Rodinson, "De l'archéologie à la sociologie historique: notes méthodologiques sur le dernier ouvrage de G. Tchalenko," *Syria* 38 (1961) 170-200; an appraisal which, however, is generally valid: compare,

for example, the archaeological approach of G. Barker, "The Conditions of Cultural and Economic Growth in the Bronze Age of Central Italy," *PPS* 38 (1972) 170-208, and "Prehistoric Territories and Economies in Central Italy," ch. 5 in E.S. Higgs, ed., *Palaeoeconomy* (Cambridge 1975) 111-75, with the literary-historical approach of E.T. Salmon, *Samnium and the Samnites* (Cambridge 1967) ch. 2-4 (a-b) to the same regional problems.

⁷⁸ A bibliography on all that has been done at Carthage would be too long to cite fully here; the reader might consult the annual reports, J.H. Humphrey ed., *Excavations at Carthage by the University of Michigan*, I, 1975 (Tunis 1976); II, 1975 (Ann Arbor 1978); III, 1976 (Ann Arbor 1977); IV, 1976 (Ann Arbor 1978), and his report, "North African News Letter 1," *AJA* 82 (1978) 511-20, which gives most of the relevant works published to that date.