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Rome's Mediterranean World System and Its Transformation

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Abstract – The revised version of an analysis of the recent large-scale interpretation of the great transition from the ancient world of the Roman Empire to the worlds of its successor states, economies, and societies offered by Chris Wickham in his 'Framing the Early Middle Ages.'

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ROME'S MEDITERRANEAN WORLD SYSTEM AND ITS TRANSFORMATION

The impressive physical bulk of a new work of history sometimes reflects the enormity of the problem, sometimes the demand for a grand new overview, but often the simple majesty of the narrative. Whatever the cause, the writing of history has of late witnessed a discernable trend back to the big. Among these recent epic endeavours are three monumental overviews of the premodern history of the Mediterranean and circum-Mediterranean lands. The authors of these panoramic histories have focused, above all, on the great forces shaping its history and on the meta-transformations from the ancient to post-ancient worlds of which the middle sea was part. All are by English language scholars working in elite universities. Even so, there is little evidence to show that the writers of these large books directly influenced one other, or that they were aware of each other's megaprojects as they wrote.[1] The convergence of historical interest seems, rather, to be of a more fortuitous and meaningful kind.

It is also manifest that these new interpretations of Mediterranean history have been shaped by the peculiar interests of each set of authors. In consequence, they reflect three different perspectives on a common problem. <u>The Corrupting Sea</u> by Peregrine Horden and Nicholas Purcell, the first of the triad, which appeared on the turn of the millennium, unveiled an innovative historical ecology of the Mediterranean world.[2] Their investigations of the Mediterranean core and its transformations emphasize the creative power of a fragmented human ecology, riven with intensity and difference. In this sea world small developments—but ones widespread in their cumulative effects—led to big changes. Constant movement and adaptation produced, in their own words, a kaleidoscope of small, sometimes microscopic, bottom-up intensifications in production and consumption. Their work was followed in the next year by Michael McCormick's massive <u>Origins of the European Economy</u>, a vast tome covering the whole of the Mediterranean and western Europe in the great transitional age between the fourth and the ninth centuries CE.[3] In contrast, McCormick's story draws attention to major continuities in the emergence of a peculiar northwest European economy—a grand narrative in which the subsistence of big pipelines of exchange of high value commodities, including, critically, the human cargoes of slaves, subsisted as channels of major economic development. If the Roman state-centered system was entering into a marked crisis from the third century onwards, the post-Roman states of the Baghdad Caliphate and Carolingian Francia recentered economic structures at the distal points of a prior Mediterranean system. Emphatic images of viscosity and constant movement, of travel and trade, pervade this history too. The most recent of the triad to appear, and the object of this review, is Chris Wickham's <u>Framing the Early Middle Ages</u>—another work on a meta-historical scale devoted to the same problem and to the same centuries covered by McCormick.[4] The production of three large historical works of nearly three thousand pages in combined length that take on the problem of social, economic, and political change on a globalizing scale, all in the first years of our new millennium, might be a sign of something significant. But of what?

All of these new works stand in the long shadow of Fernand Braudel's great masterwork on the Mediterranean world in the age of Philip II. Braudel's revolutionary perspective on the nature of historical change, originally published in France in the aftermath of the Second World War, began to have a substantial impact on the whole of the English language historical profession following its translation in the mid 1970s—in the formative generation of which these historians were part.[5] Braudel purposefully forefronted the formative power of massive geomorphic and ecological forces—the sea itself, its prevailing winds and sea currents, the mountain highlands and plains surrounding it—as huge and impassive elements that were set in a structure against, famously, the froth and temporality of mere human events. So it is not unfitting that all three investigations emphasize the long-term impact of the geographic stage. Wickham, too, begins his work with an introductory survey of the interrelationship between geography and politics in the circum-Mediterranean lands—although, tellingly, it is the briefest and most perfunctory part of his large book.

The writers of these new histories are reacting not only to Braudel, but also to other fundamental changes in paradigms and models that disturbed the writing of history in the last decades of the twentieth century: the rise of ecological globalism, the search for new paradigms in various postmodernist strands of theorizing, and the demise of classical Marxism, certainly in its more 'vulgar' modes, as a sufficient explanatory model. Horden and Purcell, for their part, abandoned Braudel's grand visionary unity of the Mediterranean in favour of what might be called a postmodernist fragmented understanding of process as defining Mediterranean development. They questioned almost every shibboleth of received historical truth on the subject, from the need to think of set categories like towns and cities to the real existence of a great historical transformation marked by anything that might be usefully or honestly called a 'middle age.' Taking a different tack, Michael McCormick focused on the axial driving forces that fuelled the gradual movement from ancient to early modern European economies. He has argued that highly profitable sectors that were at the leading edges of its dynamic, such as the slave trade remained an important core element of Mediterranean centered exchange systems (Wickham, as we shall see, dissents). The central arguments of both of these works insistently forefront the role of communications—the exchange of goods and materials, the movements of migrant populations, and the fragmented push-on flows of knowledge, innovation and information.

Chris Wickham's masterwork is entirely unlike these other two. Its genealogy, stemming from what now seems to be the nearly-deceased great tradition of Anglo-Marxist history writing, makes understandable its focus on economic and political problems that moved an earlier generation of historians.[6] More than the other two large works—and by far—he concentrates on various kinds of structurally ordered social and economic categories and conflicts which, from Marx to Weber (and beyond) have been placed at the center of the second great transition: modes of economic exploitation, shifts between slavery and serfdom, the emergence of new urban orders, the antagonistic classes of aristocrats and peasants.[7] Recourse to the looser metaphor of 'framing,' however, indicates the less certain vision of this post-Marxist historiography—there is no hint of stages, historical lineages, evolutionary steps, bases and superstructures, or any integrated nexus of well-defined linear sequences. Any teleological interpretations of history, indeed, are specifically abjured, since 'they are always misleading' (p. 831). If he eventually gets around to circulation and exchange, it is in a final chapter that bears hallmarks of a late addition and a shift in thinking. Whatever apprehensions and hesitations might be raised by the radical uncertainties of Wickham's new history, there can be no doubt about the high quality of the scholarship. In his careful and painstaking control of the microscopic and diverse detail that knits together both archaeological and literary evidence, the author is without peer since Rostovtzeff. One of the finest of its analysts concludes: 'all told, Framing the Early Middle Ages will remain a major breakthrough in historiography'—adding, exuberantly, the further judgment that 'the crisis of the Roman empire has never been described with more verve or intricacy since Gibbon.'[8]

The writing is precise and controlled, surgically efficient in its individual parts, carefully assembling mountains of precisely determined facts to argue each particular case. Sometimes it is adventuresome, too. Perhaps no more so than in an Edmund Morris moment when, in the face of no available historical instance, Wickham invents a fictitious post-Roman English village, dubbing it 'Malling' (pp. 428-34)—a village that appears later in the book, rather unnervingly, almost as if it were historical fact (e.g. p. 540, 542, 572; it is also noted as a regular entry in the index). So, too, not insignificant parts of the book are exercises in controlled speculation, especially where the evidence is mutely archaeological, as with Denmark, Ireland, Wales, and Britain in the post-Roman age (much of the sixth chapter, therefore). The mental experiments lead to innovative and productive conclusions as, for example, on the reasons for the strongly divergent post-Roman developments in Britain and northern Gaul (pp.

331-32). The balance between passages of hard description and ones of educated guesswork filled with striking numbers of 'coulds' 'woulds' 'could have beens' and 'might have beens' (e.g. pp. 330-33, 540-41), however, is well struck, and the risks of speculation are more than balanced by the virtues of the sheer scale of the coverage. He offers a truly breathtaking panorama. Complaints have been issued about this or that scholar's favourite region having been slighted or omitted (as frequently, the Balkans). But in his gigantic clockwise overview, Wickham includes detailed analyses of North Africa, Egypt, Syria-Palestine, Greece and Anatolia, Italy, the Gauls and the Rhineland, Iberia, England and Wales, Ireland, and Denmark. Whatever the omissions, this is by far the most comprehensive perspective that has been offered by any recent historian of the period.

Although Wickham questions almost every basic truism that has been accepted as part of the conventional solutions to his problem, he is neither mindlessly iconoclastic, nor does he needlessly stretch to save the appearances. When the facts do not sustain a traditional interpretation of them, no matter how central they are to existing paradigms, they are dismissed, sometimes curtly, but with full justice. He cautions against 'factoids' and dubious assertions, refreshingly denouncing some ideas as 'extremist' and others as 'phony' or just plain 'crazy' (p. 83, 96, 133). Faced with such an expansive and finely detailed revisionist work, the place for the reviewer to begin is at the end. For—we readers are truly grateful—in the concluding pages, Wickham defines both what he means by 'framing' and offers a laudably brief two-page summa of the findings of the preceding eight hundred plus pages of text. For the moment, he announces, he is bailing on any cosmic answers and instead presents his work as just a first step: he only means to essay a complex series of <u>variables</u> that 'frame' the question. Although he repeatedly remarks on the great variability and variety of the processes that he is documenting, he nonetheless insists that there are some overarching themes that define the transitional age between 400-800 CE.

Most of these similarities are linked in some fashion to the simple fact that the political structure of the Roman empire fragmented and then ended—in the west in the fifth century and in the east in the seventh. Almost everywhere fiscal structures became more rudimentary than before (sometimes even disappearing in any meaningful sense) and the power elites—the aristocrats—lost power and wealth. Consequently, it is argued, peasant communities generally became more autonomous. The aristocrats themselves underwent a profound transformation in style and behavior from being the civil elite of a world empire to a militarized class anchoring their power in the various microregions of a now disappeared imperial state. A further consequence is that the bounds of significant action and development became severely regionalized: the microregion, which had always been present, now became, once again, the dominant frame of power and development (pp. 473, 827-830). In all of this, there are cautions: although Wickham accepts the fact of severe population decline in some regions, he emphasizes that in general lands were not abandoned or left uncultivated on any significant scale. There was recession, but no totalizing collapse or catastrophe.

The whole argument is built with a close attention to detail and sequence that is evident on every page. It is constructed carefully, step-by-step. Beginning with a detailed analysis of the state, Wickham moves to the important class of 'aristocrats' and their management of landholdings, and from this basis to peasants and their social organization. He then proceeds to an analysis of the units containing the rural population: villages and other kinds of settlement. The end comes with a duet of chapters that seek to catch these units in movement. The dynamism of the greater system was formed by its interconnections, and in the making and sustaining of these links he insists on the importance of the long distance mass transport of basic commodities. Having made their way though more than seven hundred pages of dense text, on reaching the eleventh and final chapter, on 'systems of exchange,' it might surprise ordinary readers to discover that they have finally reached the 'core of the book' (p. 693). That this chapter was the last in the long gestation of the book is significant, because its language and conclusions seem rather novel compared to what precedes. In important respects, they seem to contradict the emphases and categories with which the book first begins, thereby opening onto a 'subversive history' embedded within a history that already questions the basic categories of historical analysis of this great transitional age.

Consider some of the conclusions to which the author is finally led. He places a driving priority on what he calls 'a primacy of causal factors internal to regions when assessing economic change in our period.' The correlated claim that 'the major underpinning' of the economic system is 'demand, above all for bulk goods,' closely supports the first. By this kind of demand, he means the Mediterranean wide shipment of bulk agricultural commodities such as cereal grains, olive oil, wine, and of mass produced items of mass consumption like common table wares, glass wares, and textiles. Since 'the scale of bulk exchange is the main marker of the complexity of regional economic systems,' it is the critical test of the existence of a larger Mediterranean system of exchange. These two conclusions are linked to two related claims: first, that 'the wealth that underpinned large-scale demand was essentially the wealth of the land-owning aristocracy,' and second, that it was a common imperial fiscal structure that was 'necessary for regional economies to have more than marginal links with each other' (pp. 819-20). The first big problem that the reviewer faces is this: whereas Wickham's analysis demonstrates his first two theses in abundance, his own arguments and evidence seem to indicate that the latter two claims are unfounded—indeed, unnecessary.

A GROUND-LEVEL READER

Wickham's framing restores the central importance of conventional units, beginning with the state and the upper class—here, 'aristocracies.' A basic typology of the state—tribute-collecting versus land-granting—asserts that the tax-based ones are more intrusive and autonomous; the former strong, the latter weak (pp. 56-58, 144-45, 826). It might be remarked that finding historical examples of non-tribute based states of any significance is a difficult task. Most of the post-Roman states in western Europe, for example, seem to be tax-collecting entities, even if the taxes were expended on a court rather than an army (pp. 96-101, 107). In this system, the critical importance of aristocrats was not so much rulership, or cultural and social leadership, as it was the fact that they held the enormous accumulations of wealth that created the large-scale economic demand that made the Mediterranean system work. As these two pillars of bigness gave way, the argument goes, the Roman imperial state and the whole Mediterranean world associated with it fragmented and dissipated into new and different successor regimes.

Some hard questions must be put to the putative status of these basic units and their imputed functions. Despite the record laid out in vast and minute detail on the role of aristocracies (a mammoth fourth chapter), a closer look at the evidence provokes doubt about the singular role that is ascribed to them. Since the larger and smaller component systems are driven by economic demand, the important role of aristocracies was that they provided the critical powers of acquisition that drove consumption to higher levels. Even given the paeans to aristocratic wealth in the period, and the fact that the truly staggering wealth of some western aristocrats might justify Geoffrey de Ste. Croix's angry denunciation that so much of the wealth of the Mediterranean was somehow 'just draining to the top' in this period, the claim seems dubious.[9] Based on the quantities alone, there are just not enough aristocrats with sufficient expendable wealth in their hands to produce the level of demand that Wickham needs to sustain the immense long-range transport of basic bulk commodities that he sees as the heart of the system. This level of demand must surely have been produced by very large numbers of individual consumers, with not insignificant proportions of them strongly interconnected in towns and cities that concentrated demand. Where he has the best evidence on the ground—from the economy of late Roman Egypt—he shows that it was the mass of little urban and village consumers who 'made up the main buyers for the agricultural surpluses of the rich and poor alike, and stimulated the articulated and controlled agrarian production of big estates like [those of] the Apions' (p. 767). This simple fact suggests, in turn, that the whole subject of the historical demography of the circum-Mediterranean lands cannot be swept entirely off stage and left undiscussed.

The question of how mass demand for basic commodities arose points to a series of drivers that have much to do with forces that can be tamed, directed, and encouraged by various types of human organizations that replicate themselves across space. For these same reasons, I think that perhaps far too great a burden is being placed on the state and its taxation system. First of all, the tributary levels of the Roman empire were low by comparison with the revenues of modern states. Generally, they were on a level of 10 per cent or less of annual production (contra: Wickham, pp. 65-66, 108, whose guesstimate of a 'take' by the state on the order of 25% I cannot accept). State receipts probably represented a truly modest proportion of the annual gross product of the Mediterranean.[10] The Roman state's collation of wealth and spending was an important driver, but it could not, even in the supply of the imperial army, have created the kind of demand envisaged.

The gargantuan eleventh and last chapter of the book similarly embodies another paradox. On the one hand, even a patient reader, having made his or her way though the detailed arguments of the preceding seven-hundred pages of text, is still not prepared for the final onslaught that faces them. It is a grueling march through endless potsherds that will test the endurance of the most dedicated ceramophile. Even those whose libido is moved by transport amphorae and common tablewares, will find themselves challenged to stay the course. On the other hand, and dramatically so, for the first time in the entire book—airs of the most recent addition to the whole?—the author speaks in much grander and sweeping terms of either a Mediterranean World System (henceforth, MWS) or a Roman World System (henceforth, RWS). The two are never really defined or differentiated. Sometimes they seem different from one another, in other cases they seem to overlap or even be the same thing. Whatever their precise relationship, it is manifest that the author is now thinking of change on a truly grand scale.

Wickham has insisted on the importance of tribute and tax-collection as the core to understanding the system since a famous, and still controversial, article he published in the early 1980s.[11] The mantra is repeated throughout the book, which ends with a typically firm declaration (p. 820): 'a fiscal infrastructure was necessary for regional economies to have more than marginal links with each other.' But this is manifestly not so, again on his own evidence. One of the best studies of the means that made Mediterranean bulk exchange possible, the shipping business, reveals a parabolic normal curve with a regular rise to Mediterranean-wide intensity and then an equally regular and Mediterranean-wide fall off.[12] This regularity could be attributed to accidents of discovery, the chance collations of the data, dated shipwrecks, if it were not for the fact that a number of different indices point to the same process: for example, the rise and fall in the whole quantity of coin production, in the dated use of papyrus for writing or, perhaps most striking of all, in the global effects of the air pollution produced by this system that can be now be traced in northern European lakes and even in distant Greenland. They all reveal the same general pattern of rising and falling. And there is one thing that can be said for certain about all of these factors: their rise takes place <u>before</u> the imposition of a Roman imperial system on the Mediterranean, while the systemic decline begins and continues without a break <u>within</u> the high period of the imposition of a Mediterranean-wide tribute system by the Romans. This matches Wickham's own evidence, which demonstrates that in the western parts of the empire, where the trend first becomes apparent, Mediterranean exports to peripheral regions were already in significant decline from the beginning of the fourth century (p. 77-78, 179-80). Although the fourth century might be an age of recovery in the west, the recoupment was at systematically lower levels than those of the high empire. If the state and its tributary networks—the fiscal system—was 'the main motor' for economic development (p. 79), then it signally failed in this function. State enforcement and collection of tribute was more rigorous and at higher levels in the late fourth and fifth centuries than it had been in earlier times, and its expenditures and requisitions greater, all of this in a time when one can track an overall fall in both demand and production.

In short, the evidence demonstrates that the Roman world state was as much a beneficiary as it was a primary cause of the economic system. We privilege the state and its apparatuses because we have been taught, from the nineteenth century European models, to exalt it: to set the state on an intellectual and ideological pedestal to the exclusion of many other human organizations that are analogous in kind. An outside observer might see the state as another kind of intriguing, large-scale human organization that both benefited from and created intensifications of demand and supply. All kinds of them, from great families and houses, to monasteries and religious establishments, which sometimes owned considerable proportions of all arable land, contributed to and competed in the process.[13] The debate over whether private estates did this and public states did that is therefore, in the end, a little arid (e.g. p. 71 on the great estates of sixth-century Egypt). The competition between these units <u>at every level</u> caused both the potential for expanding external domination and the increasing demand for internal refashioning, efficiency, and discipline.[14] But the MWS did not exist because of any one of them. To say that 'each region's economic history did not depend on structural links with its neighbour after the breakdown of the pan-Mediterranean Roman fiscal system' (p. 821) is really only to say what was always true of the system.[15]

The story, as Wickham tells it, narrates a normal and somewhat predictable tale. The Roman hegemony over the Mediterranean unravels in almost precisely the opposite direction from which large states had first begun to dominate the sea's ecology and gradually to unify its lands: from northwest to southeast. Beginning with Britain, its weakest link, the system gradually recoils southeastward to its anchor in Egypt, its strongest base, before it finally dissipates. It is tempting to see the modern European Union following the opposite trajectory. In any event, Britain and the central Maghrib were not the first to experience this total breakdown. By the last decades of the third century the Roman state had already abandoned similar outlier regions. In the far northeast it left Dacia, almost all of present-day Romania north of the Danube; in the extreme southeast it abandoned the whole region of Egypt to the south of the first cataract; and in the far southwest, Mauretania Tingitana, present-day northern Morocco, was left to its own devices. Britain, the one outlying region that was not abandoned, was therefore an isolated exception in this process.

Why this manner of unravelling? In highlighting the great importance of 'internal demand to its [i.e. the MSW's] articulation in every case,' Wickham understands how this insight does not just question Pirenne's famous dictum on Mohammed and Charlemagne, but completely reverses it. He also understands that Pirenne's idea still has such great force precisely because 'it fits in with the longstanding metanarrative of medieval economic history which seeks to explain the secular economic triumph of north-west Europe.' (p. 822) The apparent paradox is that it seems that it is into a variant of this same big story that we are parachuted in the process of the framing. That is to say, the new history privileges precisely the same categories of analysis—the state, taxation, estate management and production, the social and economic position of peasants, the role of cities and urban industry, and cultural priorities—that have been at the heart of this model from Karl Bücher and Karl Marx to Marc Bloch and Moses Finley. It also describes with great accuracy how the weakening and, finally, the collapse of the tribute collection system deeply affected the entire system of exchange that was sustained by an imperial Mediterranean state, how almost everywhere the elites became poorer and, in turn, generated much lower levels of economic demand, how regional autonomy eventually became dominant, and how a peculiar response in northwestern Europe emerged and came to characterize a new economic order. This has all the thrill of telling us, after the fact, what we know happened by hindsight. So it seems to confirm, although in a different way and by different means, a version of Pirenne's tale.

AN EXTRATERRESTRIAL PERSPECTIVE

This is one way of understanding this investigation, of reading this text. As with any great work that has complex levels of analytic depth, it can be rotated to other angles of perception, with profit. Another way of looking at Wickham's 'Framing' would be to consider manifest ways in which, taken at face value, it is a subversive work, but perhaps even more subversive than the author himself intended. Pretend for a moment that you are an alien visitor to the planet. This is one of the few big writings that survives. Carry the pretense one step further. Imagine that the whole survives minus its fancy and expensive dustcover, the costly OUP hardcovers, its introduction and conclusion, and the title pages are gone. No 'framing' and no 'middle ages.' What would you make of the story that is told? What would seem to be the principal containers, connectors, modes of production and other important drivers that were involved in the transformations of the ancient Mediterranean world?

First of all, the insistence on the priority of the Roman imperial state and its taxation seems on the internal evidence alone to be overdone. State and tribute are important, but they do not explain either the genesis or the general development of the system. If the Carthage-Rome axis was the 'spine' that anchored the western Mediterranean system (which is generally true), then all one can say is that this 'spine' was in place well before the consolidation of the Roman imperial state. The weighty south-north line formed of networks of exchange and urban development that ran from northern Tunisia, through Sicily, to the southern and central regions of the Italian peninsula, was already apparent by the fifth and fourth centuries BCE. It did not depend on any overriding imperial state to bring it into existence or to sustain it. Quite the reverse. In a sense, it is this same blind commitment to a 'tributary mode' that plays havoc in interpreting the downturn of the system in the west. The causes of its end are laid at the doorstep of war—the Vandal invasion of north Africa in the 430s—and the breaking of the tributary nexus between Africa and Rome (p. 710).

All the evidence elsewhere in the book, however, stands against such an interpretation. Africa, for example, continued to do quite well under Vandal rule (p. 711). And why not? The Vandal rulers were re-enacting the same parameters of the Carthaginian-Punic hegemony in the Mediterranean in the sixth to third centuries BCE. And, far from destroying any western Mediterranean 'world system,' the early western Phoenician and Carthaginian polities were actively engaged in creating it, and with it the axis of economic development that was aligned along the Bay of Tunis-Sicily-Bay of Naples corridor. All the evidence points to the breaking of this system as implicated in a violent transition on the level of war, but it is the Byzantine reconquests of the 530s-560s in Africa and Italy that mark the transition. At this point these lands were no longer part of a general Mediterranean political system, but rather the violent western periphery, the war frontier, of an eastern Mediterranean state. The whole east-west frontier running along the northern frontier of a unitary Mediterranean empire had now swung vertiginously into a north-south vertical line dividing the two Mediterraneans. Instead of being at the center, Italy and north Africa were now on the frontier.

Based on the details that our alien would read, the absolute priority placed on the imperial tribute collection system seems at least to be overly emphasized, if not misplaced. It is repeatedly stated, for example, that the MWS collapses because of a crisis in the fiscal system that supported it (e.g. p. 778-79). But in almost every significant case that Wickham himself documents, the imperial state was losing its tax incomes not because of internal administrative problems, economic downturns within subregions (indeed, sometimes quite the reverse was happening), or difficulties with the collection of tribute, but rather because the central state was systematically losing huge land areas of its tax base through the prior agency of violent force. The whole of his detailed study of the economies of Egypt and Syria-Palestine in the period are as good a demonstration as any of the process. Not only were these regions doing well during the fifth and sixth centuries, they are doing better than ever. Then, suddenly, the end came.

As our external reader would discern, the problem is not so much with what was happening, as with how it was happening. In the emergence and recession of higher and more intense concentrations of population, hierarchical ranking of control, specialized production, the consumption of wider ranges of commodities, the emergence of large scale connections in the Mediterranean, the minutiae of the evidence, so carefully and accurately collated by the author points in one direction, but his conclusions in another. The lands of the Maghrib, for example, suffer all the same down-sizing, diminishing of population, emergence of more peasant autonomy, impoverishment of nobles, and de-urbanization as do the lands of western Europe, and yet only the latter experience the precise kinds of post-Roman developments described. Why? The explanation given is that two of the regions that experienced the most severe downturns after the fragmentation, Britain and the eastern Maghrib, were so isolated and internally committed to a civil version of the Roman cultural system that its failure unduly impacted them. It is as good and convincing an explanation as any currently on the table—better than most. But if true, it seems, on the face of it at least, to point to factors other than tribute collection and the position of aristocracies and peasants as the moving forces of change.

Even where the emperor was by far the single largest landowner (p. 166), would he or his court alone produce sufficient consumptive demand necessary to sustain a substantial part of the entire system? The same question must be put to the aristocrats. In any event, the argument seems to arrive at a negative conclusion on this point: 'the Mediterranean system of exchange… once established, created its own structures of commercial exchange… [which] structures outlasted the failure of the fiscal motor…' (p. 819)—presumably, including demand. A good test case is provided by one of the best-documented regions of all, that of Egypt—the riverine economy of the Nile. Here a strong regional economy was maintained, one that suffered the least demonstrable effects of the collapse of the RWS—<u>despite the weakness of aristocratic demand</u> (p. 766-67, my italics). Egypt's internal economy nicely survived the demise of the MWS in ways that Britain and the central Maghrib demonstrably did not. High population levels and good communications are the proffered explanations. The causes had to be some combination of factors connected with internal markets that were capable of weathering basic shifts in external fiscal systems. The reference to communications requires a series of related factors including, above all, the compact and dense nature of a relatively numerous population where every unit was in ready and less expensive communication with the others.

In this case, too, the impact of the imposition of an entirely new and different culture—in language, in religion, in law, in aesthetics, in social norms—never seems seriously to be confronted. The whole impact of Islam on this transition is just 'not there.' The omission is significant not only for matters of thought, belief, and culture, but also for economic reasons fundamental to the place of the Mediterranean in development.[16] It seems to be assumed that for some reason a huge part of the ancient Mediterranean World System disappears from our purview. If a FMP emerged in these lands (but did it?), it did not have the same valence and impact as that in northwestern Europe. But even for western Europe, the alien reader would be more than a little troubled. Among many other questions, he would surely be concerned with this one: Why is it that of all the possible explanations and models for a collation and intensification of economic, social and political bonds in western Europe, the only one that is presented is one that posits the lordly use of power and force against weak and unwilling peasants who resist such impositions? The question so forcefully presents itself since, despite a prolonged effort to find it (pp. 577-85), what evidence there is gives no indication of either a generalized peasant resistance to the process or of the centrality of force or compulsion as the main cause in the convergence of aristocratic and peasant interests. What happened <u>seems</u> not to be in dispute: that is, the move from a dominant PMP to a dominant FMP. Once again, as the author states, there has hardly been any serious study on <u>how</u> it happened (p. 571).

Pressed in this manner, other ghosts begin to appear in the machine. The words in the narrative repeatedly document highly salient or coordinated violence—most often in the form of war—as a critical cause in shifting the course of economic, social, political and cultural action and communication. War remakes the Iberian peninsula (p. 94), it refashions Africa and breaks the critical 'tributary spine' that is the axis of the western Mediterranean exchange system (pp. 87-88), and it reconfigures the Rhine frontier and hence Francia (pp. 102-03).

It remakes peninsular Italy (pp. 203-04). It is the cause of the break in the 'eastern tax spine' that severs Egypt and Syria-Palestine from the eastern Roman state (pp. 125-26), where the precise consequences of losing and winning wars are presented in graphic detail (pp. 127-28). A special instance is offered by the case of Egypt. Since the removal of the author's favored factors—the Mediterranean state and its tributary system—does not cause an internal disintegration of the system, it compels him to move war to the head of the list of causal factors (p. 769).

It is manifest, indeed, from the host of the assembled evidence that the core of the eastern MWS was flourishing until broken by war: 'the whole network remains in place, hardly touched, when the Persian and Arab invasions begin' (p. 716). In this case, the consequences are severe: this network, so strong for so long, vanishes in just a generation (not three on my calculation and on his evidence): between 614 and 642, Egypt and Syria-Palestine are forever severed from their previous north-south Mediterranean connections (p. 716). Despite all these basic data, two things are remarkably evident. First, the notices about the causal effects of large-scale violence are almost wholly subordinated to the other categories of analysis; they enter the narrative almost as after-the-fact notices. When war is finally noted as one of the major factors involved in the transformation of the MSW (the RWS version) into whatever it was that followed, it is not included in the author's grand conclusions where war is, almost inexplicably, entirely absent. Rather, in the midst of a discussion in one chapter (p. 719), war appears third in priority, after the emphasis given to fiscal/tributary systems and the problems of land ownership. It is interesting to note the grudging language: 'Third, we must recognize the impact of war and generalized destruction… This factor has tended to be referred to here only in passing, but it would be foolish to deny it altogether.' One has the distinct feeling that the author feels compelled to note the factor since it would be imprudent entirely to deny it. War deserves more than this off-hand concession. Much more.

Once again, the problems, I think, go back to that Austro-German model. Little or no theorization of war or violence was built into its crude evolutionary model that highlighted antagonistic classes, economic forces, the natural growth and supersession of stages of human development motored along by production, transformations in property and labour regimes. I admit that the theorization of violence and war is difficult and that almost nothing of merit exists even now for antiquity. But anyone who considers the bare record of what happened to the MWS can scarcely deny a preeminent role of war and violence in the making and unmaking of the system, even in its smallest constituent parts. Belief is another similarly undertheorized strand in this historical theory (and I am well aware of Weber).

As noted above, it is only towards the end of this monumental work that the author begins to speak of a Mediterranean World System (p. 708). Even so, this 'world system' was not at all sufficient in its own right. One difficulty, therefore, is to sort out internal and external pressures, since this system was located, like a series of disjointed tiny island environments at the extreme distal end of a vast Eurasian system of circulation of populations, ideas, and goods. Its containers point to the cosmically larger human systems that are not even hinted at: the huge oceanic resources of the Atlantic to the west (what sort of MOP was this?) and the larger world of pastoral nomadic communities on the vast Eurasian plains to the east. To talk about the impressive wealth of Palestinian regions, for example, in later Antiquity, with no reference to the probable impact of the economies of Sassanid Persia, but almost solely to an inside set of drivers, seems problematic.[17] And how much that was relatively unusual about the MWS was driven by the sheer scale of intensity of communications and populations in insular and peninsular environments, whether those of the Mediterranean or Atlantic-facing Europe? Once asked in this fashion, I think that our attention is deflected from peasants and aristocrats, to another underlying theme that constantly resurfaces throughout the book and which, by its end, is emphasized with a peculiar force: the microregion.

The great importance of the microregion is a point upon which the author insists, and the mountains of evidence assembled by him support his contention. These are the fragmented smaller worlds of regions, sub-regions, and micro-regions that go creatively into the making of this world: they were the ecological units that were the primary units of production and exchange out of which the larger Mediterranean system had to be created. But what are they, really? They keep coming up time and again, as not much more than 'containers'—things somehow just happen at this level, even in response to larger forces. What larger units did was to provoke sufficient demand to encourage the exchange of mass commodities for bulk consumption that linked the smaller units into larger 'world systems.' Others have been exploring these same directions, driven in part by the speculations of Horden and Purcell.[18] Some of these small systems, like Egypt along the Nile, Wickham convincingly sees as analogues of completely internally coherent systems that were already like modern nation states in antiquity: a national unit with its own little national economy (p. 767).

Even so, the really difficult question to answer is: How did the system work? Hints are present, here and there, about how the system changes. At several points, critical ones indeed, the explanation assumes the jargon of the Gladwellian 'Tipping Point,' as the cumulative weight and combination of changes suddenly lead to what the author vividly calls a 'catastrophe-flip' in the whole system: the fate of the cities of seventh century Byzantine Anatolia (p. 633, notably caused by war); the end of African Red Slip ware production (p. 713); northern Italy after the 650s (p. 732-33), among other cases. This points to systemic engines of creation and mutation that escalate and de-escalate in ways perhaps typical of other biological systems. Reversion is another process that is manifest in his evidence: turns that mark the relapsing of larger units onto existing microregions. Replication is another process that seems to be central to its operation. In other words, one sees in the scale and complexity elements that point to neo-Darwinian models as perhaps indicating a way of comprehending the changes. The parabolic curve of development suggests that population growth and recession, and an attendant rise and fall in consumption, were basic causes (as admitted in the well-documented case of Egypt, p. 767). But much else was involved, including peculiar pushers, drivers, and catalysts for which, probably, a new vocabulary will have to be developed by historians.

In the shortest, but perhaps most powerful chapter—the fifth, on land management—the author points away from fixed categories to more labile ones that allow us to escape both his devotion to the bisectorial manorial economy on the one hand and the need to bestow a wholly unique status on slave labour in production on the other. He clearly sees that these are different responses to a similar problem: the need to intensify production. This need fuels the attendant drive to close control and monitoring procedures on the one hand and to specialization in production on the other (pp. 264). Old models, however, just refuse to die: so exhausting and unnecessary reachings are made to demonstrate the earlier existence of this particular mode, the 'demesne type,' here and there in the ancient Mediterranean (e.g. pp. 265-74, all of it grasping at straws) instead of accepting the mode for what all current evidence shows the bipartite agricultural domain to have been: a peculiar response developed in northern Francia (pp. 280-81). (That is to say, <u>in this specific mode</u> since, in general terms, the response of bipartite land management, apart from the privileged use of the term demesne, was surely found at many times and places in global history.) The adoption and extension of this technique along various arcs extending outwards from Francia was perhaps provoked because components in it—like the specialization of market-oriented production—matched the labour regimes and conditions of demand and supply found in that particular historical context. So attention is rightly drawn to vineyards and the production of wine as a leading edge that was already present inside different existing combinations of production and consumption (p. 285). Again, the mechanisms of change and development that Wickham suggests dovetail neatly with his critique and rejection of traditional models of 'the colonate' (p. 521-26). On the other hand, he does not go down the interpretative road on rural labour paved for us by the work of Jairus Banaji. He seems, rather, to accept the interpretation of the laws connected with 'the colonate' as tax-driven micro-regional adjustments and not much more—which they might well have been.[19]

THROUGH THE EYES OF OTHERS

Peter Brown has drawn a comparably grand and cosmic panorama of these same great transitions, insistently hewing along the lines of ideas, beliefs, ritual practices, and sacred institutions. His could easily be argued to be a fourth way of seeing the same big process. Alternative categories are therefore just as possible and successful. Here they track the formation and development of an imperial religion and its manifold transformations and mutations: its growing bigness and its fissioning along microregional lines.[20] Even though following a different set of concerns, this other narrative line points to much the same underlying story, albeit witnessed from a different point-of-view. If one reads and compares the evidence and the closely reasoned arguments of the different perspectives taken by Brown and Wickham, they tell us a number of things. First, whatever the story, it is not susceptible to framing. Despite Wickham's proclamation and the marginal utility of the metaphor, this is surely not what he has done. He frankly admits that his categories of analysis—fiscal structures, aristocratic wealth, estate management, settlement patterns, collective peasant autonomy, urbanism, and material exchange—are ones that he has used because they are the ones that he is best trained to use. Others—belief systems, gender roles, representations, ritual and cultic practices—might be just as good (p. 825). But the units of analysis that dominate the work are not just any collection of ones in which the author somehow happens to have some expertise. They are the conventional core of historical analysis that has been devoted to this problem since the mid-nineteenth century.

Recurrent elements in the story indicate the need to think differently about the nature of the whole process. Here Peter Brown's global perspective on the rise of Christianity points to other necessary parts of this same story. Concepts, mental inventions, and transcendental beliefs, if consistently held by large numbers of people in concert, can compel and drive just as much as war. If the Muslim conquests did anything, they entirely reconfigured the whole of the Roman Mediterranean system, taking out of its primacy a whole circuit of arid lands along its southern periphery from Palestine and Egypt in the east to the Maghrib al-Aqsa in the west. Wickham, too, affirms that this great geopolitical shift took place: these lands were no longer mainly oriented along north-south Mediterranean-centered axes, but rather to an expansive east-west polity that extended eastwards to the Iranian plateau (p. 130-31).[21] The combined force of ideas, identity, and violence were creative and transformative. But these factors point, in turn, to the critical importance of patterns of thinking and belief, language and communication, and other containers of human behavioural patterns, and show them to be just as 'micro-regional' or 'hegemonial' as tributary relations or systems of exchange.

Insistence on the priority of the state in the specific terms of an imperial empire and its tributary system only makes sense if it did in fact dominate the whole system in the manner claimed. The basic evidence on the kinds of connections with which the author wishes to anchor his MWS tend to lead us in other and different directions. Reconsider that bell curve of development that shows the Roman state to have been as much a result as a cause of the MWS. This is not to assert that, in Northian terms, the enforcement of more consistent norms of legal relations and the availability of more consistent kinds of currency over a much larger geographic area and over large populations, for example, might not assist in hyping the scale, complexity, and duration of production and consumption—effectively doing what states sometimes do well: cutting down systematically on transactional costs. It is just that top-down state driven factors were not essentially causal to the MWS: the system in fact begins its regular takeoff over centuries when there were numerous large states in the Mediterranean basin and a plethora of smaller ones, all marked by their own peculiar cultural norms of communication and exchange: dominant languages, currencies, legal systems, and so on.

The differences and competitive nature of these units in no way impeded the regular advance of the system through the so-called Hellenistic period, that is after the 330s BCE. Indeed, the system, marked by the subsequent emergence of an increasing sameness of language and culture, experienced its main periods of growth and consolidation under conditions in which there was no unitary tributary system governing it. As the author himself notes (pp. 690-92, 790-92), a second phase of the MWS arose in precisely the circumstances where small polities—minor kingdoms, small principalities, city-states large and small, and ecclesiastical corporations, business families, amongst others—as well as fragmented cultural, legal, linguistic, and religious differences—were once again dominant in the lands of the middle sea. This surely points to the great importance of all kinds of organized economic behaviour, many kinds of it on a great scale and performed by so-called illegal or criminal organizations and pursuits (the sex business, the drug trade) that historians, in their apparent need to conduct a moral discipline, studiously ignore. From bare faced criminality to institutionalized protection, all could collaborate to account for the rise of a state like that of a Genoa.[22] It is here that the historian will have to decide on the relative significance of the internal forces generated by a microregion and the external connections, like those of the slave trade, in the rise of a polity like Venice.[23] In any event, competitive small-scale units were well capable of achieving another world system, indeed one which, through its Atlantic connections, was to transform the globe.[24]

So Wickham's solutions sometimes look too peculiar for comfort. While not accepting every criticism that Jack Goody has proffered as bearing equal weight, and being well aware of problems and objections to various precise formulations of his hypotheses, there is enough to be said for his serial attacks on a western historiography that privileges its own categories of development to cause discomfort with another version that looks, despite the denunciation of teleology, to look just like that.[25] There has certainly been more than one capitalist moment in the past. Some of the core elements of this great transformation were anchored in the Mediterranean; others were not because, as Peter Brown has repeatedly emphasized, they never were in the first place. And as Michael McCormick has demonstrated—and his claims have been sustained by other recent studies (with more forthcoming)—the slave trade was one of the core elements in the Black Sea-Mediterranean system that perdured. It is perhaps odd that its importance is so strongly denied by Wickham (p. 696n8, where it is reduced to a footnote). Some of the explanation must lie in his extraordinary devotion to potsherds and dismissal of literary evidence as significant to understanding patterns of large-scale commerce (p. 693-700). But the slave trade was a kind of commerce that did not leave many material traces of the kind that Wickham wishes to privilege. In this sense, it was deadly but silent. Like the other economic indices of a larger Mediterranean system, the slave trade also witnessed recession in response to demand, but then experienced a revival just before it was to feed into a new Atlantic world that its merchants and maritime explorers were creating.

What is perhaps most profound about this new work is that its exacting, precise, and accurate collation and analysis of the known evidence relevant to the problem, even from the purview of its chosen angles of attack, tell of this other hidden history of the Mediterranean. If anyone still believes in a 'middle ages' much less an 'early middle ages,' even as an historiographical convenience, after reading this critical weighing and dissection of the huge mass of relevant evidence, then there is probably very little that the reviewer can add to change their minds. Assembled on this scale and detail, what the data themselves demonstrate is that a dominant nineteenth century Austro-German model of historical evolution is so fundamentally flawed and misleading that it must surely be abandoned. In this manner, a classically-based aesthetic and cultural periodization—classical civilization transits to modernity via a 'middle age'—was somehow transmuted into an historical model armed with the necessary economic stage ('feudalism'). Attempts to save the appearances by endlessly re-tooling the utility of social and economic classes, modes of production, the special status of the western city, and the origins of so-called feudalism will no longer work. As a result of Wickham's monumental investigation, our attention should be more insistently refocused on the problem of what was actually happening, what are the better categories of analysis, and what specific forces were involved in the transmutations of global systems.

Every analysis of the increasing profusion of data is signaling to us, and urgently, the need to abandon the obsolete categories of the great period of nineteenth century myth making about history, whether that of Freud in sequences of mind and spirit, that of Le Play in the history of the family and sentiment, those of Tönnies, Simmel, and Elias in culture, or those of Marx and Weber in historical economics.[26] They are all versions of the same story, calqued on a strangely crude vulgar evolutionary logic intended to explain modernization. First them, now us. Every detail and conclusion reached in this massive volume speaks so much against both the periodization and the aim proclaimed in the title that one must wonder if Chris Wickham is playing with the reader when he says that he is 'framing' the 'early Middle Ages.' Could it be that he is suggesting a usage more normally found among Philip Marlowe and his peers? The dictionary informs us of this other meaning of the verb 'to frame': to set someone up to take a fall, or to conspire at the demise of someone or something. I hope so.

[1] As the last in the series, although not by much, Wickham lists Horden & Purcell in his bibliography, but with indications of haste: one author's name is misspelled and there is little evidence of the actual use of their work (all in the final chapter). All signs are that when the different works were being researched and written, there was very little contact between the three sets of authors.

[2] P. Horden & N. Purcell, The Corrupting Sea: A Study of Mediterranean History, Oxford, Blackwell, 2000.

[3] M. McCormick, Origins of the European Economy: Communications and Commerce, AD 300-900, Cambridge, Cambridge University Press, 2001.

[4] C. Wickham, Framing the Early Middle Ages: Europe and the Mediterranean, 400-800, Oxford-New York, Oxford University Press, 2005 (a paperback edition was issued in 2007); for American readers at least, this work now bears the somewhat ominous acronym of FEMA.

[5] F. Braudel, La Méditerranée et le monde méditerranéen à l'époque de Philippe II, Paris, Armand Colin, 1949 (second edition: 2vols., 1966); The Mediterranean and the Mediterranean World in the Age of Philip II, 2 vols., translated by Sîan Reynolds, London, Collins, 1972.

[6] Which is why, I suspect, it seems to some to represent a revival, of sorts, of 'social and economic history': M. Whittow, "Beyond the Cultural Turn: Economic History Revived?," Journal of Roman Archaeology 20 (2007), 697-704, at p. 697.

[7] Therefore, much the same categories that moved Perry Anderson's overview produced in the mid-1970s: Passages from Antiquity to Feudalism, London, NLB, 1974; with the follow-on companion volume that, notably, emphasized the connections to the modern state: The Lineages of the Absolutist State, London, NLB, 1974.

[8] J. Banaji, "Chris Wickham and the End of Late Antiquity," appendix 5 [in] Agrarian Change in Late Antiquity: Gold, Labour, and Aristocratic Dominance, revised edition (Oxford, 2007), 257-68, at pp. 268 and 260 respectively.

[9] G. E. M. de Ste. Croix, The Class Struggle in the Ancient Greek World, from the Archaic Age to the Arab Conquests, London, Duckworth, 1981, p. 503 (the phrase was originally Peter Brown's).

[10] See K. Hopkins, "Rome, Taxes, Rent and Trade," Kodai: Journal of Ancient History 6-7 (1995-96), 41-75 = ch. 10 [in] W. Scheidel & S. von Reden eds., The Ancient Economy (Edinburgh, 2002), 190-230, who argues for even lower overall rates of taxation: 5-7% of GDP. Even if Wickham, ad loc., restricts his guess to some sixth-century cases, I am still dubious; the larger point is that they are surely not applicable to the general Roman Mediterranean system.

[11] C. Wickham, "The Other Transition: From the Ancient World to Feudalism," Past & Present 103 (1984), 3-36.

[12] A. J. Parker, Ancient Shipwrecks of the Mediterranean and Roman Provinces, Oxford, BAR International Series, no. 580, 1992; this, with some other relevant evidence, is recapitulated by F. de Callataÿ, "The Graeco-Roman Economy in the Super Long-Run: Lead, Copper, and Shipwrecks," Journal of Roman Archaeology 18 (2005), 361-72.

[13] For example, on the significant role that could be played by networks of families, friends, traders, and tradesmen, see Avner Grief's work, including "Contract Enforcebility and Economic Institutions in Early Trade: The Maghribi Traders' Coaltion," The American Economic Review 83 (1993), 525-48 and A. Greif, P. Milgrom & R. Weingast, "Coordination, Commitment, and Enforcement: The Case of the Merchant Guild," The Journal of Political Economy 102 (1994), 745-76; and A. Grief, Institutions and the Path to the Modern Economy: Lessons from Medieval Trade, Cambridge, Cambridge University Press, 2006, esp. ch. 3, based in part on the documentation of the Cairo Geniza.

[14] V. Tin-bor Hui, War and State Formation in Ancient China and Early Modern Europe, Cambridge-New York, Cambridge University Press, 2005, esp. 79-84 and 178-89, on what she generally calls internal 'self-strengthening' reforms.

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[15] A. Bresson, La cité marchande, Paris, de Boccard, 2000, passim on the existence of these discrete economic zones through the Hellenistic period in the eastern Mediterranean (just by way of example).

[16] J. Banaji, "Islam, the Mediterranean and the Rise of Capitalism," Historical Materialism 15 (2007), pp. 47-74, arguing for its importance in the great shift from a Mediterranean to an Atlantic phase of capital development.

[17] See the review of "Framing" by John Haldon, Historical Materialism 17 (2009), forthcoming, for a critical view of the relevance of the Sassanian evidence to this problem (although the scholarship is arraigned in his review for rather different purposes).

[18] Horden and Purcell, The Corrupting Sea, pp. 79 f., for example; and by the reviewer, applying their concept of the 'virtual' island: B. D. Shaw, "A Peculiar Island: Maghrib and Mediterranean," Mediterranean Historical Review 18 (2003), pp. 93-125; now also by others: David Kennedy, Gerasa and the Decapolis: A 'Virtual Island' in Northwest Jordan, London, Duckworth, 2007.

[19] C. Grey, "Contextualizing the Colonatus: The Origo of the Late Roman Empire," Journal of Roman Studies 97 (2007), pp. 155-75, who offers one of the better surveys of the recent debates.

[20] P. Brown, The Rise of Western Christendom: Triumph and Diversity, A.D. 200-1000, second edition, Oxford, Blackwell, 2003: an good comparative work, since it has much the same western 'tilt' as McCormick and Wickham; it is the revised edition, it must be noted, that is of particular importance to this problem.

[21] In specific cases, like Andalucia, Wickham seems, on the one hand to accept the important of this warfare as a fundamental cause of change (pp. 226-27), only to deny it later (p. 230-31); cf. G. Fowden, Empire to Commonwealth: Consequences of Monotheism in Late Antiquity, Princeton, Princeton University Press, 1993, esp. pp. 138 f., building on the arguments made earlier in his study.

[22] A. Grief, "Building a State: Genoa's Rise and Fall," ch. 8 [in] Institutions in the Path to the Modern Economy (n13 above), pp. 217-68.

[23] That is to say, Wickham, pp. 690-92, as opposed to McCormick (n3 above), pp. 761-77.

[24] Although, again, with heavy peculiarities in development that should not be assumed or ignored: B. D. Shaw, At the Edge of the Corrupting Sea, Oxford, Sir John Myres Memorial Lecture, 2006, esp. pp. 26-27, 30-32.

[25] J. Goody, Capitalism and Modernity: The Great Debate, London, Polity, 2004; The Theft of History, Cambridge, Cambridge University Press, 2006, esp. chaps. 1-3 and 11.

[26] In pre-emptive response to some criticisms already voiced: I am hardly suggesting dispensing with Marx (or Weber, for that matter) tout court; my comments are limited strictly to the adoption of the linear stage-like model of history, and to some of the attendant assumptions.