Seventy years ago, José Ortega y Gasset began his reflections on the Roman Empire with a very strong claim on behalf of his chosen subject: “The history of Rome, by virtue of its content and of the comprehensiveness of our knowledge of it, may well be called a model history” (Ortega y Gasset 1946: 11; the essay was written in 1940, in exile in Argentina, and reflects a strong inclination to draw parallels between the crisis of the Roman Empire and Europe’s twentieth-century predicament). In fact, the original formulations were even stronger: Ortega declares “es paradigma,” where the English translator suggests a model, and he refers to “madurez” rather than comprehensiveness, thus clearly indicating ripe insight rather than merely extensive knowledge. He was taking a widely held but diffuse view of the Roman record to extreme lengths. It must be added that, as he then saw it, the picture had only just been completed: Rostovtzeff had done for the Principate what Mommsen had done for the Republic, and thus made the period that began with the Augustan settlement and ended with the third-century crisis an integral part of the paradigm. The reassessment of late antiquity was not yet on the agenda. An interpretation in Ortega’s spirit, built on Mommsen and Rostovtzeff, would not necessarily be undermined by this most recent addition to our knowledge of the Roman world: late antiquity could be seen as a phase of resurgence coming after the crisis so memorably portrayed (and, as scholarly consensus now has it, over-interpreted) by Rostovtzeff. But no such approaches are represented in current scholarship; no historical sociologist would subscribe to the claims quoted at the beginning. In fact, the pendulum has swung very far indeed in the opposite direction: there are, as will be shown at greater length below, good grounds to
suggest that the Roman experience is one of the relatively neglected areas of comparative history, and that specific themes to be explored must be cleared of assumptions and connotations that have blocked broader perspectives. From that point of view, the most obvious reason for going back to Ortega would be the contrast that highlights present failure to give Rome its due. There is, however (as I will try to show), more to the abandoned view than that. Although there can be no question of reestablishing Roman history as a “paradigm,” a closer look at the key issues will confirm its quite exceptional significance for comparative studies, and thus in the end allow us to extract a grain of truth from Ortega’s overstatement.

A more focused discussion of Rome’s place in comparative history may begin with three conspicuously relevant categories: states, empires, and civilizations. All are recurrent themes of comparative studies, and intuitively applicable to the Roman case. One of the chapters in this book will discuss the Roman pattern of relations between the three historical frameworks at greater length, and thus approach comparative questions from within the specific configuration that is our main topic; here the aim is, rather, to summarize the evidence and clarify the reasons for inadequate accounts of the Roman record. Each of the three perspectives suggests particular lessons to be noted.

### Seeing Rome as a State: Flaws and Achievements

The scholarly literature on statehood and state formation is very rich, but a few particularly broadly conceived surveys stand out, and a brief glance at their problems with the Romans will be useful. To understand the difficulties, basic ambiguities besetting the very concept of the state should be borne in mind. As Mogens Herman Hansen argues in his discussion of city-state cultures (Hansen 2000: 11–34), multiple and mutually incompatible definitions of statehood dispute the field of comparative political history, and the question of city-states brings the disagreement to a head; in fact, those who prefer a modernistic conception of the state will never settle for a general category of city-state, but try to replace it with emic terms borrowed from each particular tradition. However, Hansen’s attempt to solve the problem by allocating divergent notions of statehood to different disciplines is less convincing. It is not the case that historians and social scientists agree on a broad concept of the state, applying to a long premodern history, while legal scholars and political scientists stick to the modern criteria first theorized by Machiavelli and Hobbes. The opinions of historians, including authorities on Greek and Roman antiquity, are divided; a strong current still supports the view that the state, as an impersonal apparatus of domination, is a late medieval and/or early modern European invention. When this ongoing dispute is confronted with the record of the Roman Republic, the ambiguity of the evidence goes beyond the general problematic of city-states. On the one hand, Roman political thought – and the political imaginary behind it – is commonly credited with taking the notion of *res publica* to a higher level of abstraction than the Greek tradition had done. On
the other hand, the Roman political regime was to a very high degree embedded in social hierarchies of status and power. The key role of aristocratic families and their networks of *clientelae*, formal as well as informal, is the most visible aspect of a general pattern that is often seen as the very opposite of statehood. There are, in other words, obvious reasons for divergent views, and even for changes to conceptual frameworks. Christian Meier’s second thoughts about the crisis of the Republic are a striking example of the latter: in his *Res publica amissa*, he referred to a “Roman unity of state and society” (Meier 1980: 156), but the introduction to a later edition (1997) expresses radical doubt about this modernizing terminology and argues for an explicit shift from state-centered interpretations to a more general understanding of the political.

As has long been seen, the Principate was a compromise. It established a new political center, with correspondingly different relations to its social basis, but in doing so it added new ambiguities. On the one hand, the whole governmental apparatus was adapted to the demands of imperial rule on a vastly expanded scale, and for some historians, this represents progress toward a more advanced level of statehood; Claude Nicolet (1990), for example, has described the Principate as a significant but inconclusive stage on the road to the modern state. On the other hand, several features that mark the new version of the imperial order seem to undermine this claim. The autocratic power center was adapted to the republican institutions in ingenious and effective ways but, by the same token, suffered from a certain under-institutionalization of its more innovative aspects; hence the overpowering emphasis on the person of the ruler and a corresponding weakness of the foundations for continuous and impersonal statehood; hence, too, a permanent temptation to redefine the terms of the compromise and move toward a stronger version of rulership. The emperors who pursued such aims in particularly upsetting ways (Caligula, Nero, Domitian, and perhaps even Commodus) were demonized by historians coming from a senatorial background, but are now portrayed in a more balanced fashion by modern scholarship. None of them achieved lasting results. On the other hand, the strategies and arrangements devised to compensate for shortcomings of the center included the exercise of power through urban communities with local autonomy, but also – another side of the same coin – an empire-wide delegation of power to oligarchies embedded in sociocultural configurations of honor and status. These features of the regime raise further doubts about the application of state-centered models.

In view of this record, it is not surprising that the Roman achievement has, for all its intuitive appeal, been a difficult case for comparative work on state structures and state formation. The most seminal inquiry of this kind, Norbert Elias’s study of the civilizing process (Elias 2000), begins with the fragmented post-Roman condition of early Western Christendom; the Roman trajectory, including the dis-integrative dynamic of its Western finale, is a part of the historical background that is tacitly taken for granted. Neither the persistence of Roman traditions in the Church nor the reactivation of Roman law at a later stage are recognized as factors contributing to state formation. A whole school of thought has taken off from
Elias’s work and expanded the original framework in different directions, but, to the best of my knowledge, there has been no serious attempt to bring Roman themes into this broader picture. Neither comparisons of the long-term Roman and European trajectories, nor genealogical links between them, nor European rediscoveries and reappropriations of the Roman legacy have been given their due in enlarged versions of the Eliasian program. And the most focused attempt to challenge the Eliasian assumptions, Charles Tilly’s work on coercion and capital (Tilly 1990), does not raise questions about Roman connections.

Another interesting example is S. E. Finer’s history of government, undoubtedly the most ambitious work of its kind, and eminently thought-provoking even when it fails to convince. Here the emphasis is on the typology of state forms rather than on the dynamics of state formation, and the approach to Roman political history is determined by that context. A striking aspect of Finer’s account is a sharp distinction between the Republic and the Principate. As Finer saw it, the Republican order was a very strange case of cultural strengths saving an incoherent political regime from itself. The constitution as such was “preposterous” (Finer 1997: I.440), an outcome of a long sequence of improvised maneuvers and compromises invariably designed to preserve the core of oligarchic power while conceding some of the accessories and appearances. If it worked for a long time, it did so “in spite of itself, … because of unwritten conventions that its provisions should, effectively, be side-stepped” (ibid.). And although Finer does not make the point in so many words, it would be in the spirit of his argument to add that when this fundamentally fraudulent order began to fall apart, the same conventions – and the whole complex of cultural traditions behind them – aggravated the crisis: they blocked thinking about an alternative. What they did not do was to confer any kind of dignity on the unfolding political process. During the last century of the Republic, “the practice of politics in Rome was thoroughly degenerate,” marked by “no more sophistication, disinterestedness, or nobility than in a Latin American banana republic” (ibid.). Only the exceptional geopolitical dimensions of the struggle and the scope they gave to a few outstandingly able individuals could to some extent sustain illusions about this rotten core of the regime.

One reason for taking note of Finer’s views is that no modern analyst seems to have come closer to standing Polybius on his head. The superior rationality of the mixed constitution is dismissed as a myth, hiding the reality of incoherent arrangements that depended on extra-political resources for survival; and although the cultural traits of the Republic are acknowledged as military assets, the overall perspective on the republican empire – including the protracted period of crisis that set in soon after the triumphs observed by Polybius – stresses imperial success as a source of illusions about the regime at home, rather than a testimony to its virtues. The Principate is a different story, and it is only because of its achievements that we can credit the Romans with a distinctive input into the history of government. Even if some of the developments in question go back to Republican beginnings, their potential was more fully realized in the Principate. Once again, it is tempting to read the argument as a counterpoint to Polybius: the real achievement of the
Romans was not so much the conquest as the maintenance and perpetuation of empire, and it took the Principate to establish the connection between political regime and imperial reach that Polybius had erroneously attributed to the Republic. The innovations thus coming to fruition were meta-constitutional rather than constitutional.

For Finer, it is important to distinguish these Roman inventions from a much older set of imperial traditions and techniques with which they were combined. Historians have sometimes failed to grasp this distinction. A polyglot empire ruled in part by bureaucratic methods (however rudimentary by later standards) and in part through semi-autonomous urban communities was nothing new: such formations had a long history in the Near East and the Eastern Mediterranean. The real innovations had more to do with statehood as such than with forms of imperial rule, even if they were implemented on an imperial scale. Finer lists four of them, and they are all worth closer examination. The first is the creation of a “consocial state” (1997: I.601); in fact, this term covers two different trends. On the one hand, it refers to the unification of oligarchy, “a ruling stratum throughout the empire which took no account of race, nation, language, colour, religion or culture, but looked only to wealth and local influence” (ibid.). This is a new variation on the theme most famously developed by Ronald Syme: Rome as a paradigm of oligarchic rule (and since the permanence of oligarchy was, for Syme, the most basic fact of political life, that raises Rome to the level also claimed by Ortega, although not for quite the same reasons). Oligarchy was certainly real and pervasive enough to impress itself on historians with otherwise different thematic foci; Finer’s particular twist has to do with a supposedly unprecedented standardization of oligarchic status across the huge imperial domain. This view is open to various objections. German historians (especially Rilinger 2007) have drawn attention to the coexistence of incongruent stratification patterns within the empire; there was no consistent overarching system with a unified elite at the top. The persistence of key republican institutions under the Principate meant, above all, that the Senate as the apex of the aristocratic order continued to occupy a preeminent place, never submerged in the “empire-network of oligarchs” which Finer sees as the real core of the consocial state. The autocratic ruler – and the power structure that took shape around him – was also an incommensurably superior part of the imperial hierarchy, and with ramifications at more symbolic levels. If it is true that the empire was to a considerable extent integrated through a ranking order of honor (Lendon 1990), it should also be noted that a specific version of this order, not necessarily congruent with all others, crystallized around the emperor.

In short, Finer’s description of a uniform oligarchy seems vastly overstated. The other aspect of his consocial state is the development from a ruling city to a ruling province (Italy), then to “Romania” as an integrated imperial realm, and thus beyond the “narrowly specific meaning of empire” (1997: I.602). Here the argument seems to shift from social structure to collective identity. The most obvious objection is that the ruling city remained central to the imperial imaginary – so much so that when the capital had to be shifted, it could only be envisaged as a new
Rome. If historians talk about the “incomplete identity” of the ruling province (Giardina 1997), one major reason is the enduring and overwhelming presence of Rome as an eternal center. That applies, a fortiori, to “Romania”; moreover, the latter identity could only progress at the expense of a very wide spectrum of older ones, more or less resilient, and had to be superimposed on a Greco-Roman division. The very existence of “Romania,” or rather the degree of reality which it attained, is a matter of debate and ongoing historical research, rather than an established fact to be listed among Roman achievements.

Finer’s second point is the most decisive one, and it touches upon a permanently contested issue. His view is that the Romans were the “first to conceive of a res publica, a nexus of goods, activities and institutions which belonged at large”; he even adds that Roman “constitutional development embodies the idea of political authority as something abstract and not personal” (1997: I.602; this suggests a more benign judgment on the Republic than the one quoted above). It seems impossible to claim that the Greeks had no idea of a public sphere “belonging at large.” It can, as noted above, be argued that the Roman res publica represented a higher level of abstraction; the piecemeal solutions to the conflict between patricians and plebeians pointed in that direction; and the most advanced expressions of political thought during the crisis of the Republic took this trend one step further. But Finer’s interpretation goes beyond these criteria and seems to portray the Roman notion of government as a unique approximation to modern ideas of statehood. Some basic problems with this claim have already been indicated. The res publica was made up of very unequally weighted social components, and that set limits to its structural autonomy. In the political sphere, the “de facto normative, if not strictly legislative authority of the senate” (Beard 1990: 43) was a major counterweight to what Finer calls the abstraction of authority. When the republican order was adapted to an autocratic center, the traditional qualifications of the res publica lost ground, but new ones were added. To cut a long story short, the developments that Finer construes as a breakthrough are better understood as a complex of relative trends, circumscribed and modified by counter-trends. His analysis decontextualizes certain aspects of the Roman experience, and it is linked to another move of the same kind. In his view “the Roman legacy disappeared” (1997: I.604) in the West, and it was only several centuries later that some parts of it were rediscovered. Here the decontextualizing approach leads to an unwarranted separation of tradition from history: the continuity of the former is reestablished after a long historical break. Recent scholarship on the transformation of the Roman world – this notion has to all intents and purposes replaced the traditional paradigm of imperial decline and fall – suggests a different perspective. There was no wholesale disappearance of the Roman legacy; various aspects of it entered into the making of medieval kingdoms from the outset.

Finer’s two last points are best discussed together. They have to do with the “ubiquity of the law in both the public and the private sector,” and with the “nature of this law” which “exists in a purely human dimension” (1997: I.603). To put it another way, the Romans pioneered the rationalization and secularization of law, defining it as a “set of general principles, plus a juridical technique for applying
these to concrete cases in all their singularity”; a logical corollary of this rationalizing process was the establishment of a “juridical world of free-willing and equal individuals” (1997: I.604). There is no doubt that these statements express – in a particularly forceful way – a widely shared understanding of the Roman achievement in one of its most central fields. It is all the more appropriate to add that recent work has brought some correctives to bear on that view, and in so doing suggested a more nuanced comparative approach. A far-reaching secularization of legal norms and legal expertise accompanied the political development of the Roman Republic, and this made both the intrinsic character and the sociocultural status of Roman law very different from the sacred law that prevailed in Jewish, Islamic, or – in yet another way – Hindu traditions. These well-established facts are not being contested, but it can be argued that they constitute only one side of the story. The other side is that a specific “nexus of law and religion” (Ando and Rüpke 2006: 12) remained in force and influenced the later shift to Christian conceptions of legal order. A religious context is, on this view, important to the understanding of Roman religion and its place in comparative history: “Republican law, then, erected boundaries around religion even as it recognized its centrality. Human institutions were recognized for what they were, and limits were established that respected the agency of the gods”; the same authors add that “we come to know Roman law at a time when it had already been laicized, and what we witness in the classical period is the recursive inscription of religion both within the law and as a form of law” (2006: 12–13). This line of argument has far-reaching implications. Here we can only note in passing that it adds force to an older analysis which stresses the common and enduring limits of Greek and Roman ideas about law, and more particularly the dominant assumption that a legislative order concerns the citizens of a given political community. Within this model, there could be specific norms concerning interaction in a more global arena, but they should not be mistaken for notions of a transcending legal order (Wolff 1971).

Between Empire and Civilization

As we have seen, questions about Roman statehood lead to considerations on the empire. But when it comes to a comparative focus on the empire as such, the situation is very different from the one we have just surveyed. On the one hand, it is much easier and more intuitively convincing to think of Rome as an empire than as a state. The Roman example has, on two different levels, become an inbuilt premise of discourses on empire. Rome was the acknowledged ancestor and model of all aspiring Western empires, those with continental European ambitions as well as those that expanded overseas (the rivalry and the succession of imperial projects have to a great extent shaped the course of Western history, even if the failure to achieve lasting imperial unity is also one of its most significant features). The Roman precedent also entered into all attempts to understand and compare empires on a more global scale; cultural memories of Rome affected the perception and
interpretation of analogous phenomena in other parts of the world. But this Roman bias of the historical imagination has never translated into similarly Roman-centered analytical and comparative work. Some of the reasons for this have to do with exceptional characteristics of the Roman Empire. A closer look at them will bring the Romans back in, but in a new capacity: rather than representing a model or paradigm, their historical experience appears as a unique combination of unusual features, and thus as a particularly instructive key to the contrasts and variations that comparative studies of empires have to consider. The Roman case becomes, in other words, a prime reminder of the heterogeneous, changing, and contingent forces involved in empire formation; these general features of the field must be properly grasped before imposing any kind of unifying framework.

A short account of Roman exceptionalism might begin with the relationship between city-state and empire. The point is not simply that a city-state conquered huge territories and converted itself into an empire, far more successfully than any other expanding polity taking off from a similar basis. More significantly, the dual political framework of city-state and empire remained essential from early beginnings to the final restructuring phase; a rebalancing of the two components, rather than a replacement of one by the other, was central to the most traumatic transformation of the Roman order (the rise of the Principate); and the centrality of Rome was a permanent part of the imperial self-image. There seems to be no parallel to this enduring but adaptable intertwining of city-state and empire. Even the partial comparisons that are sometimes suggested must be toned down. The Spartan hegemony that prevailed in a part of the Greek world was too limited to be classified as an imperial regime. The “Athenian empire” is arguably a misnomer (and this seems to be the reasoning behind Ian Morris’s reference to “the Greater Athenian State” [2009: 99–177]). In this case, a city-state developed hubristic ambitions in the wake of an exceptional military achievement and an internal political transformation of a uniquely radical kind, and went beyond traditional models of hegemony; but the attempt did not last long enough to work out a balance between ambitions and environment (see also Raaflaub, this volume).

Comparisons with earlier developments in the Ancient Near East have tended to focus on Assyria as an apparently clear-cut case of transition from city-state to empire. As Mario Liverani argues in his contribution to this volume, closer examination reveals a more complicated picture. But at this point, one striking contrast between Rome and Assyria is worth noting. “The idea of monarchy was born with the emergence of the Assyrian state and the two grew to maturity together like twins” (Grayson 1991: 199). The Roman trajectory was very different. The consolidation of Rome as a city-state and a regional power took place under a monarchical regime. A crisis that was obviously caused by domestic as well as regional factors brought the monarchy down, and this coincided with a setback to Roman power in Central Italy. The following very long phase of sustained expansion was organized by a regime that was not only based on non-monarchic institutions, but also characterized by an unusually strong anti-monarchic political culture. It was this expansionist republic that made Rome unchallengeable within the Mediterranean world.
Comparative studies on empires have sometimes suggested a structural connection or at least an elective affinity between imperial rule and a strong monarchy tending to autocracy; there is some historical evidence to support this claim, and it can also be argued on more theoretical grounds. Imperial visions of superior and – in the most important cases – world-dominating power are, *ceteris paribus*, eminently compatible with aspirations to autocracy as a higher form of monarchy. The two aspects converge in elevating a power center beyond the constraints and challenges operative at a lower level. The Roman experience suggests that we should qualify and contextualize the connection, rather than reject it outright. The Republic managed to neutralize the autocratic logic of imperial expansion for a remarkably long time, but in the end, it was the main neutralizing mechanism – a highly regulated and ritualized regime of competition within the ruling class – that reopened the door to monarchy in a markedly autocratic form. This paradoxical turn, however, cannot be understood as a case of simple adaptation to imperial logic. The monarchy that took over was a complex historical phenomenon: overdetermined by the encounter with Hellenistic models, circumscribed by republican institutions that had to be preserved in modified forms, and durably affected by contingent features of the Augustan settlement. On the other hand, the foundational phase of the Principate confirmed the link between autocracy and conquest: an unprecedented bout of expansion helped to consolidate the new regime, and more of the same was obviously envisaged. But setbacks then led to an acknowledgment of limits that were not massively transgressed during the subsequent history of the empire (see also Ziolkowski’s chapter in this volume). To round off the picture, it should be noted that expansion was not quite taken off the agenda. New conquests and offensives were still launched by emperors who needed to secure their hold on power or project images of strength and renewal. But initiatives of that kind were limited, the results even more so, and the main post-Augustan additions – Britain and Dacia – were abandoned before the final fragmentation of the empire set in.

In short, the Roman record of monarchy and empire is a complex story, and markedly different from the pattern of mutually reinforcing growth found in many other cases. The rise to uncontested imperial supremacy was achieved without concessions to monarchy; the most massive expansionist push was an integral part of the transition from republic to Principate; but once the autocratic regime was in place, maintenance took priority over conquest. There are other peculiarities to be considered. It is a commonplace that the pre-Christian Roman Empire did not, unlike some others, rely on a universal religion or a comprehensive cultural model to legitimize its rule or unify its domains. But it has also been shown – although perhaps not universally agreed – that notions of religious or cultural tolerance are misleading. The Romans did not – *pace* Finer – simply continue a tradition of tolerant rule over a multilingual and multicultural empire, represented at its best by their Persian predecessors. The uniquely Roman blend of cultural and political power lies somewhere between the two poles, and more precise descriptions of it have proved highly controversial.
Some of the liveliest recent debates on Roman history have centered on the notion of Romanization, and some critics have proposed to discard it altogether; this view seems inspired by postcolonial preconceptions of the more extreme kind (a refusal to credit empires with any kind of cultural productivity), and should be dismissed as an ideological aberration. If it has nevertheless gained a hearing, this is due to difficulties encountered when the character and the limits of Romanization are to be grasped in more specific terms. The Roman practice of ruling through urban communities (and creating them where they did not exist before) entailed the diffusion of corresponding modes of life; it is true that this development involved adaptation to local customs and conditions, and that its social impact was limited, but these considerations are not sufficient reasons for rejecting the idea of Romanization as a process that linked cultural assimilation to political control. On the ideological level, these cultural components of Roman rule were translated into the notion of *humanitas*, its practical impact should not be overestimated (to take a salient counterexample, it was not in the same league as Confucian models of cultivation), but it does belong to the distinctive pattern of relations between empire and culture that sets the Roman case apart from others.

As for the religious dimension, it is still a matter of debate among scholarly authorities whether we can speak of Roman imperial religion or only about religions of the empire – perhaps even cults of the empire, as suggested by those who suspect anachronistic connotations in the very term “religion.” It is, in any case, clear that Romanizing trends in religious culture must be analyzed in the context of a much older and more comprehensive mode of religious life: the rules of mutual translation and identification that had already prevailed between the polytheisms of the Ancient Near Eastern and Mediterranean world. If this tradition, as Jan Assmann argues (Assmann 1996), was a civilizing achievement, the description also implies a capacity to counteract imperial leveling. That said, it can nevertheless be argued that specific Romanizing processes unfolded within the given limits. Roman conceptions of proper ways to communicate with the gods had a certain impact on cultic practices throughout the imperial domain; in exceptional but not irrelevant cases (Druid and Dacian religion), this led to the suppression of pre-Roman traditions. The empire-wide compromise with Judaism reflected a distinctively Roman way of regulating diversity. Last but not least, and whatever particular sense we make of the assimilation of imperial dignity to divine status (not to be mistaken for complete equation), it seems undeniable that “the position of Augustus atop the empire allowed the Mediterranean world to share a deity for the first time” (Ando 2000: 407).

All this changed when the imperial center converted to Christianity. No empire of comparable size and strength ever underwent a religious mutation of such magnitude (the incorporation of Daoism and Buddhism into the religious framework of the restored sixth- and seventh-century Chinese Empire amounted to a major change, but the older tradition of imperial Confucianism was also revived, and in this case the sacral status of the imperial center as such was more important). The change affected both sides. Historians have spoken of the “end of ancient
Christianity” in connection with the end of imperial rule in the West and the consolidation of the papacy as the sole center of Western Christendom; it seems equally justified to describe the Constantinian turn as the end of early Christianity and the beginning of the late antique phase in Church history. Christianity shaped the cultural profile of late antiquity, but it was also reshaped by the overall context of the period. The latter point has certainly not been ignored, but so far it seems to have received less attention than the first one. To reiterate the claim to uniqueness, there is no comparable example of interconnected successive mutations of a world empire and a universal religion.

The growing importance of regional perspectives in comparative history has raised new questions about empires; both their relationship to regional bases and their impact on regional patterns have varied considerably across historical and geographical contexts. In this regard, there is one more unique feature of the Roman Empire to be mentioned. The contrast between contiguous territorial empires (the premodern Eurasian type) and transoceanic ones, first created in the course of European expansion, has been duly noted. But the Roman Empire was the only one to rule over the whole region around an inland sea and make this sea its main axis of integration. The search for analogies to the Mediterranean in other parts of the world has not been very successful. The main contenders are the Baltic-North Sea complex in Northern Europe (Gerner et al. 2002) and the maritime part of Southeast Asia (Lombard 1990) but neither of these two regions gave rise to a unifying empire. The Mediterranean has nevertheless been a particularly rewarding theme for historians interested in regional structures, and the most recent major work on the subject (Horden and Purcell 2000) brings the discussion closer to classical antiquity, but further analysis will be needed to integrate this evolving line of scholarship into debates on the Roman Empire.

Among all unusual characteristics of the Roman Empire, its Greco-Roman cultural identity is perhaps the most striking. No other imperial formation developed a similarly bipolar pattern. Historians of East Asia have become increasingly interested in changing combinations of Chinese and Inner Asian imperial traditions, and moved away from the earlier assumption that conquerors were completely absorbed into Chinese civilization; but the crucial difference is that the empires built by conquering nomads were to a very great extent adapted to Chinese political institutions, whereas the Romans never borrowed political models from the Greeks. From the middle period of Islamic history onward, Turco-Mongol traditions coexisted with Arab or Persian ones within various imperial regimes, but in these cases Islam imposed a broader civilizational unity. The Greco-Roman constellation seems unique. Contributors to this volume will discuss various aspects of it; here we may note a few points emerging from recent scholarship and important for comparative approaches.

The Roman impact on the Greek world was too significant for the notion of a continuous and progressive assimilation to Greek models to be tenable: “the ‘Greek city’ of the Imperial period would be more correctly described as ‘Greco-Roman’: that is, as a fusion or mélange of languages and institutions, types of public
It is, of course, equally true that there were massive limits to this fusion. If there are good reasons to think of Romanization in general as a partial process, this applies even more to the Greek East than to the West (Greek areas in Southern Italy and Sicily were a special case). But as the same author stresses, the limits never translated into a clash of cultures: “no Greek cities, or groups of cities, offered any parallel to the major Jewish revolts of AD 66–74 and 132–5” (1993: 250). What did happen was a sustained reaffirmation of Greek cultural identity, especially in the second century CE, but in ways that did not contest or contradict imperial loyalties (Woolf 1994) and could even be encouraged by the imperial center. This remarkable balance between the two components of the empire was, in the long run and within the surviving eastern part of the imperial domain, followed by an undeniable shift toward the Greek pole, but not to the extent suggested by Millar when he writes that “in the end Graecia capta did indeed imprison her captor” (Millar 1993: 250). On the one hand, the Eastern Empire that became Byzantium preserved too much of the Roman legacy (beginning with what German historians call the Kaiseridee, the very notion of an emperor) for that to be an acceptable description; on the other hand, the Greek culture that achieved the final comeback was already a part of a new formation decisively influenced by non-Greek sources, that is, Christianity.

Both Greek and Roman patterns operated on a macro-cultural scale, superimposed on a vast spectrum of local traditions and societies. They were, in other words, civilizational frameworks. This brings us to a third keyword for comparative studies. Civilizational issues will be considered from various angles in the following chapters. Here a brief comment will suffice. The fact that the Roman experience has not figured prominently in recent work on comparative civilizational themes is obvious, and so is the main reason for it. Far more than any other topic, the question of the Axial Age and its civilizational legacies has dominated the field, and it was difficult to find a place for Rome in this context. Nobody has ever suggested that there was a Roman cultural transformation comparable to those undergone by Ancient Greece, Israel, India, and China around the middle of the last millennium BCE; very few – but not uninteresting – attempts have been made to show how this non-axial record can be reconciled with Rome’s obvious world-historical importance. The shift from “civilizations of the Axial Age” to “axial civilizations,” that is, from a period to a type, and towards a common pattern defined in more abstract terms, has aggravated the problem and made it even harder to find a proper conceptual focus for the Roman experience. A move in the opposite direction, toward a more diversified account of transformations during the Axial Age, might open up new possibilities of comparison, but this has so far been very much a minority option. And to link this problematic more effectively to our present subject, we would need to include the Greco-Roman relationship in its changing forms, and without any reductionist preconceptions, as an integral part of the Roman experience. Greek civilization has always been treated as one of the key axial cases; recent work has underlined its specific features; and if its impact on Roman history is
taken into account, the prospects for comparative analysis will look more promising. This theme will be taken up in some of the chapters in this volume.

This brief survey has noted obstacles and unresolved problems related to the very themes that ought to be most central to comparative inquiry in our field. The picture would be incomplete without mentioning recent attempts to strengthen the Roman connection on a more empirical level, without any systematic reference to the conceptual foci mentioned above, but in ways that can lead to reappraisals of more theoretical issues. Two recent developments of this kind stand out. A French survey of political regimes in world history was meant to include two volumes on empires, one on the European sequence of imperial powers and another on major non-European cases. Within the European context, the obvious importance of the Roman model sets the agenda for the whole project: “There is no empire in the West without reference to the Roman one. There is, in the last instance, only one empire: the Roman one” (Tulard 1997: 14). But this emphatic acknowledgment of Roman primacy is not followed by any systematic clarification of the Roman impact on the formation and the historical dynamics of later Western empires. Medieval and modern cases of more or less stable imperial rule are described in some detail, but there is no comprehensive framework for the analysis of connections and references to the Roman background. Such an approach would, at one end of the spectrum, consider the differences between regions directly and indirectly affected by Roman power, and their respective conditions for empire formation (it is, for example, not irrelevant to the course of medieval history that the center of a supposedly resurrected Roman empire shifted from a former province – the core domain of the Frankish kingdom – to an area on the other side of the former imperial border); at the other end, there are the various uses of imperial symbolism inherited from the Romans. A further point to be noted is that the metamorphoses of the Roman legacy unfolded in different ways within the three successor civilizations. Among other things, the Western Christian centrality of the papacy, capable of innovations paving the way for state formation but not of replacing the empire, set specific limits to imperial ventures. In the Byzantine world, an empire that claimed to continue Roman rule was confronted with alternative empires that could even aspire to conquer the original imperial center (the Bulgarian and, more briefly and controversially, the Serbian one). Finally, the question of enduring Roman influences on the emerging Islamic world must be discussed in relation to two decisive framing factors: the gradual retreat from an initially very strong version of sacral rulership (the Caliphate) and the subsequent appropriation of Persian statecraft. In short, these multiple sequels call for a combination of genealogical and comparative perspectives. More strictly comparative approaches would be appropriate when it comes to a broader focus on non-Western imperial traditions. The unfinished state of the project in question seems to reflect the difficulties encountered on this level: the planned second volume has not been published.

The second, more recent line of comparative inquiry focuses on contrasts and parallels between Rome and China. Two representative examples are the
collections edited by Walter Scheidel (2009) and by Fritz-Heiner Mutschler and Achim Mittag (2008). The former deals mainly with the structural and institutional aspects of imperial power, while the latter analyzes the cultural transformations that accompanied empire-building, especially those that involved new forms of historical consciousness. This is not the place to discuss the two volumes in detail, but they should be noted as landmark contributions to a long-neglected and very promising field of comparative studies. Rome and China are arguably the two most significant imperial formations in world history – because of their sheer magnitude in space and time, their overall impact, and the cultural developments linked to their political trajectories. As it happens, the comparison is made easier by one of the most striking contingent parallels in recorded history: Rome and China emerged as unrivaled regional centers at roughly the same time around 200 BCE. Major contrasts between the internal as well as external contexts of empire formation are evident from the outset. In the Roman world, the most important legacy inherited from a prior phase of state formation was a widespread and adaptable political culture of city-states, whereas the Chinese empire drew on traditions of statecraft developed during a long struggle between larger territorial states. On the external side, there was no East Asian parallel to the enduring antagonism of Rome and Persia; but, on the other hand, the barbaricum with which the Roman empire shared a northern frontier never gave rise to counter-empires of the kind created by China’s Inner Asian neighbors already in the time of the Han dynasty (and there could, a fortiori, be no synthesis of two imperial traditions, such as those emerging from the contest of the Inner and East Asian ones). The crises that in both cases took an explosive turn during the third century CE were due to a changing mix of internal and external factors. In the short run, it was the Roman Empire that responded more effectively and restored unity after a very brief period of fragmentation, whereas its Chinese counterpart entered a much longer period of division (this record has been overshadowed by the later history of persisting imperial unity in China and multistate rivalry in Europe). The Chinese return to a unified imperial order went hand in hand with a civilizational expansion that spread Chinese models into a larger regional arena and created a Sinic rather than simply Chinese world. This happened, once again, at roughly the same time as the civilizational partition of the Roman world. The Chinese pattern of transformation did not involve a religious revolution comparable to the victory of Christianity in the West, and by the same token there were no East Asian analogies to the East–West split within Christianity or the Islamic reform of monotheism. The Chinese phase of fragmentation was marked by religious innovation, drawing on internal sources (Daoism) as well as contacts with another civilizational complex (Buddhism), but these developments were compatible with more civilizational continuity than in the West.

In short, Roman and Chinese experiences seem to offer particularly interesting perspectives for comparative research, and further issues could be added to the above list. But to round off this introductory discussion, one more trend in contemporary scholarship should be mentioned. In this case, theoretical problems are
explicitly posed but without any sustained connection to the framework preferred here. Recent work has brought new views to bear on the comparative economics of empires; Peter Bang’s contribution to this volume is in that vein, and the same author has been involved in other projects with a stronger focus on economic history. Several lines of argument have converged toward more complex and nuanced models of imperial economies. When neo-Marxist historians moved from the “Asiatic” to the “tributary” mode of production, they not only abandoned a restrictive geographical frame of reference. More importantly, the new category stressed the role of extra-economic determinants: forms of tribute presuppose political and military power structures, and empires are by definition based on particularly large-scale mechanisms of tributary extraction. This shift has been reinforced by efforts to move beyond the dichotomy of primitivist and modernist perspectives on traditional economic regimes. The approach that has been gaining ground does not generalize modern patterns of economic development but allows for varying degrees of dynamism linked to institutional contexts; the imperial type of embeddedness has emerged as a particularly important differentiating factor. By the same token, imperial settings seem to have been crucial to premodern episodes of economic growth – “economic efflorescences,” to use the term introduced by Jack Goldstone (2008). The recently published *Cambridge Economic History of the Greco-Roman World* (Scheidel et al. 2007) makes a strong case for treating the Roman imperial economy as a significant case in point. Finally, the debate on premodern patterns of globalization has drawn attention to imperial networks of long-distance trade, not least those related to prestige objects. That aspect has also figured in new accounts of the Roman economy.

**Origins, Crises, and Consolidation of Imperial Power**

After this survey of themes and prospects for comparative analysis, a brief discussion of contributions to the present volume is in order. As will be seen, they reflect varying lines of inquiry and interpretation, applied to different aspects and phases of the Roman experience. Kurt A. Raaflaub’s reflections on the origins of Roman imperialism touch upon a wide range of issues concerning the whole trajectory from local beginnings to Mediterranean hegemony. Raaflaub accepts the view that Roman imperialism is too well attested for reasonable doubt to be allowed. But if its enduring reality has been established, it becomes all the more important to explain its origins, and this can only be done with reference to a long-term process that coincides with major changes to Rome’s geopolitical position as well as to its internal regime. The abolition of the monarchy, followed by a long-drawn-out and contested reconstruction of statehood (the “struggle of the orders”), and the fifth-century setback to regional ambitions, slowly overcome through renewed and sustained expansion, were the most salient aspects of the background to Rome’s emergence as an empire of unprecedented staying power. But although some landmarks stand out and suggest lines of interpretation, inadequate sources for the
history of the period as a whole have made it difficult to construct a coherent narrative of imperial origins. Raaflaub argues that comparative approaches can to some extent compensate for this handicap and that the Greek world is the most rewarding context of comparison. Greco-Roman perspectives apply to the initial conditions and early stages of the respective city-state cultures, and they can serve to highlight both contrasts and parallels; they must also allow for connections of a very asymmetric kind at an early date. But the main focus of Raaflaub’s chapter is on similarities and differences between Greek and Roman city-state expansion beyond initial limits. In this regard, developments during the period in question do not reflect any significant Greek influences (the conjecture that markedly non-Greek features of Roman political culture have something to do with Etruscan sources is plausible but difficult to develop beyond guesswork; and there was, in any case, no Etruscan precedent for the imperial trajectory of a city-state).

It should be noted that Raaflaub’s interpretation is somewhat more favorable to the notion of an Athenian empire than the one suggested above. But the relevance of a comparison between Rome and Athens does not depend on both cases being classified as empires: even if we prefer to speak of a “larger Athenian state,” it can be useful to compare the forms and implications of city-state expansion. One notable similarity is that both Rome and Athens conducted and understood their expansionist ventures as “huge and sustained communal efforts,” in contrast to the imperial regimes that identified with strong monarchy. On the other hand, the Roman political community remained under oligarchic rule throughout the decisive phase of expansion, and this made the integration of elites in conquered territories easier than it was for the Athenian democracy whose triumph was closely linked to the expansionist turn. A further contrast worth noting is that the Athenian bid for power over other poleis – hegemonic or imperial, depending on the interpretive model – was initially legitimized by the city’s key role in defending a common cultural world against imperial aggression. There was no comparable background to Rome’s ascendancy.

A few words should be added on Raaflaub’s concluding reflections. He proposes to take the genesis of Roman imperialism, accompanied by the “Struggle of the Orders” and the institutional responses to it, as a starting-point for closer analysis of Rome’s relationship to axial civilizations and their world-historical dynamics. In his view, it is the political history of the early, and the beginning of the middle, Republic – not, as some other interpretations (Moatti 1997) would suggest, the intellectual ferment of the late Republic – that lends itself to comparison with axial transformations in other cultural contexts. The underlying premise of this claim converges with other criticisms of the strong axial model: instead of assuming an invariant dependence of social and cultural dynamics on a cultural distinction between transcendental and mundane orders, we should think of axial constellations as clusters of trends and perspectives, with varying emphasis on different domains of social life, and not uniformly centered on “transcendental breakthroughs.” The innovative thrust may be concentrated in the political dimension. That was, as Raaflaub has argued at length elsewhere, the case in archaic Greece,
and Roman beginnings belong in the same category. As for the period discussed here, the feature to be stressed is a long-drawn-out and inventive adaptation to the twin challenges of internal conflict and imperial expansion. It would seem misguided to dismiss this kind of transformative capacity because of its failure to measure up to unilateral criteria of transcendental grounding. Raaflaub is less inclined to admit any axial component or potential in the later history of the republic. Its response to the problems arising from the conquest of the whole Mediterranean was vastly different from those applied on a smaller scale during the early phase.

The last century of the Republic (from Tiberius Gracchus’s tribunate to the establishment of the Principate), and especially the half-century that began with Sulla’s dictatorship, has long been a particularly fascinating subject for historians’ interpretations. The period ended with a massive political transformation that has often tempted modern authors to speak of a “Roman revolution,” although it has been abundantly clear that some key components of a revolutionary scenario in the modern sense were missing: there was no revolutionary agency, at least not at the level of collective action, no revolutionary project, and least of all a revolutionary ideology. It could hardly be doubted that the republican regime succumbed to a systemic crisis, even if the chronology and the precise character of the crisis remained matters of debate. But the solution that emerged after a series of destructive conflicts and abortive innovations – the Augustan settlement – seemed strangely external and incomplete: it did not grow out of adaptive or reflexive responses to the crisis, and its institutional contours were never defined in a way that would have set it unambiguously apart from the old order. Christian Meier’s model of a “crisis without an alternative” was one of the most seminal attempts to grasp this peculiar historical constellation. Recent scholarship has added further aspects to the picture. If the Roman version of oligarchic rule rested on more ingenious ways of integrating and controlling popular participation than those of other ancient city-states (this seems to be the upshot of discussions sparked by Fergus Millar’s efforts to bring democracy back into the received image of Roman politics), it is by the same token important to clarify what happened to this component of the regime during the period of crisis. Finally, a stronger emphasis is now placed on the pan-Mediterranean dimensions of the transformation, but much more remains to be said on this subject.

Egon Flaig’s analysis of the transition from Republic to Principate relates to these ongoing controversies. Flaig regards Meier’s work on the “crisis without an alternative” as a new opening for interpretive history, not yet fully appreciated by contemporary scholars (and, as the introduction to a later edition shows, at first not adequately articulated by the author); however, some aspects of Meier’s argument call for further elaboration. Two crucial themes of Flaig’s reflections on the road beyond Meier should be noted. On the one hand, the question of the “Roman revolution” is reactivated through a somewhat unlikely detour: Lenin’s definition of a revolutionary situation turns out to be relevant to the Roman case, but not in the sense that it could serve to redefine the revolutionary character of the Augustan regime. Rather, Flaig’s reading of Lenin draws on Meier’s conceptual
scheme while at the same time giving a new twist to its historical content: the upgraded version of the narrative suggests a revolutionary crisis without a revolutionary alternative, and this very constellation had to result in another kind of alternative, more circuitously related to the problems at hand and less clearly opposed to the existing order than it would have had to be to satisfy revolutionary criteria. One aspect of Lenin’s definition, the unwillingness of the lower classes to put up with established ways of class rule, is exemplified by actions and attitudes of the *plebs urbana* during the last decades of the republic. There was no possible revolutionary outcome of this plebeian turn to violence (imperial expansion had taken Roman power beyond the level where urban crowds could have a decisive impact on its structures), but it played a greater role in the decline of the *res publica* than Meier was willing to admit. On the other hand, Flaig links the inability of the ruling class to go on ruling (the other point highlighted in Lenin’s definition) to a longer history of disproportionate claims to wealth, power, and prestige by the most privileged section of the aristocracy, that is, those most directly involved in conquest and predatory warfare. This elite, increasingly uncontrollable by the traditional aristocratic rules and conventions, was so much more powerful than the rest of the upper strata that it does not seem far-fetched to speak of a separate class; the outcome of several rounds of rivalry within this class (which, as Flaig argues, might have led to a fragmentation of the empire into kingdoms of the Hellenistic type) was then imposed as a foundation for an alternative to the republic.

One of the most recurrent and multifaceted themes of debates on the Roman Empire is the question of its place within comparative perspectives on traditional and modern societies. It has proved as impossible to do without this distinction as it is necessary to allow for further nuances on both sides; the specific characteristics and experiences of empires have been stressed by those who want to move away from the invidious dichotomy of tradition and modernity; and the particular achievements of the Roman Empire are bound to attract notice when such issues are debated. As we have seen, economic historians are reassessing the Roman phase of the “ancient economy”; another prominent aspect is the level of urbanization reached under the Principate. Increasingly detailed work on urban history has not only underlined the exceptional dimensions of Rome as the imperial capital, but also the size and importance of a few other urban centers. This very unusual constellation depended on the state that kept the empire intact. Urban development and other trends related to it reflect the capacities of the Roman imperial state, all the more so in view of recent work stressing the poverty and backwardness of local societies (Jongman 2002). If the relative strength of this state remains controversial, it is nevertheless clear that the present round of debate is shifting the terms toward a more careful appraisal of resources, strategies, and results, and of the most distinctively Roman ways to assert state power.

The aspects that David Cohen and J. E. Lendon highlight in their chapter have to do with imperial forms of communication and the conclusions to be drawn from a comparison of the Roman record with a state belonging to one of the successor civilizations (the kingdom of Aragon, obviously marked by a high degree of feudal
fragmentation but also, at its most ambitious, a notably active contender for power and clearly not unrepresentative of its age). The comparison must deal with massively unequal source materials: the medieval record is incomparably more copious than the Roman one. As the authors note, this unequal documentation is in itself an interesting subject for research, but certainly not a reason to equate presence in the records with strength on the historical ground. Another preliminary point to be noted is that rulers communicating with their more or less powerful subjects use a whole register of different modes, and each particular one should be understood in the context of the others rather than in isolation. When a Roman emperor explains things to the addressees of his edicts, this is – as the authors argue – better understood as a way of representing authority in a specific guise than in the sense of communicative action. Although that is not an explicit aim of the chapter, it seems legitimate to take the point as an objection to Clifford Ando’s (2000) emphasis on communicative relations between the imperial center and its subjects. That said, the main thrust of Cohen and Lendon’s argument relates to the question of state strength, as reflected in the language of rulers, and the conclusion is unequivocal: the relevant Roman documents suggest a much more entrenched relationship of command and obedience than the Aragonese ones. The Roman emperor issues orders and expects to be obeyed, whereas the Aragonese king cajoles, maneuvers, threatens, and persuades. The image of the Roman Empire as a strong state thus receives further support from a comparative analysis of its discursive modus operandi.

Late Antiquity and Beyond

To situate Adam Ziolkowski’s chapter in the context of current debates, a short comment on the “crisis of the third century” and changing interpretations of it may be useful. Although many aspects of third-century history are still controversial, and likely to remain so, it seems possible to sum up some basic points on which there is now growing if not general agreement. It should now be clear that the notions of crisis and transformation are not mutually exclusive, and that the former need not be used in a purely pejorative sense: a crisis can generate creative efforts and be conducive to transformations. The most conclusive evidence for a third-century crisis is to be found at the level of the most central power structures. Given the extreme instability, weakened legitimacy, and functional inadequacy of the imperial institution between 235 and 284, there are good reasons to speak of a political and military crisis. At its most acute, it led to a temporary fragmentation of the empire, even if the fragments did not develop separatist ambitions. There is also wide agreement on the broader geopolitical context. The early third century saw two basic changes on this level: a strengthening of power centers (whether they should be described as states in formation is a matter of debate) along the northern frontier and the overthrow of Parthian rule by the much more dangerous Sasanians beyond the eastern one. These mutually unrelated developments transformed the
strategic situation of the empire. On the other hand, the idea of an underlying and general socioeconomic crisis, causing the outbreak and determining the course of the political one, has been abandoned. That also applies to Rostovtzeff’s model of a cultural collapse due to a rural revolt against urban elites – one of the most ingenious attempts to turn the Marxian vision of the class struggle against its main political advocates. Andreas Alföldi’s (1967) conception of a “world crisis” was meant to go beyond the socioeconomic frame of reference, but was never developed beyond tentative indications and is no longer present in scholarly debates. Géza Alföldy’s (1989) more hermeneutically grounded construction of a generalized “crisis consciousness” has proved vulnerable to criticism. The question of the third-century crisis has thus been clarified and reframed in significant ways. Within the new perspective, there is nevertheless scope for diverse opinions on a whole range of issues. The impact of the political and military crisis – as well as of imperial responses to it – on various areas of social life differed from one part of the empire to another, and historians differ in their assessment of the overall picture. Two little-known but possibly important factors are epidemics and climate changes.

Ziolkowski’s argument should be seen against this background, and can be summed up in five main points. First, he places a very strong emphasis on the cumulative impact of geopolitical setbacks: this was, in itself, a sufficient reason for the situation of the empire to be perceived as critical and calling for urgent countermeasures. Second, the change for the worse affected the very foundations of the Principate. If the regime was, in Flaig’s terms, based on acceptance rather than full-fledged legitimacy, the emerging geopolitical predicament changed the balance between the forces that had to ensure acceptance. The role of the army had always been decisive; now it became more directly and visibly so, but this also reinforced a fundamental ambiguity inherent in the position of the army: it wavered between choosing emperors from its own ranks and throwing its support behind members of the political elite. At the same time, senatorial reactions to the new prominence of the army aggravated the crisis. All these factors shaped the course of events during the half-century in question. In short, the geopolitical upheaval translated into a legitimation crisis at the very core of the imperial power structure. But Ziolkowski’s third point is that Roman elites and rulers were not simply at the receiving end of a crisis coming from elsewhere. He argues convincingly for the view that Roman perceptions of developments in the barbaricum were adequate enough to translate into preventive policies, and that this was in fact the background to the rise of the first “soldier emperor” in 238. But responses to the new geopolitical prospects were not limited to changing strategies on the ground. As Ziolkowski sees it, measures taken to strengthen traditional religion (the fourth point), and more particularly the religious policies of the emperor Decius, were also inspired by an activist vision of imperial defense and related to the visibly growing external threats. At the same time (this is Ziolkowski’s final point), the very efforts to develop a forward defense aggravated a problem built into the core institution of the empire: the monopoly of conquest and victorious warfare, claimed by the emperor, set strict limits to the initiative of provincial armies and authorities faced with frontier
problems. This factor had already affected earlier strategic decisions. Ziolkowski rejects all versions of the idea that the empire had reached natural borders, and argues that only internal reasons can explain the failure to take advantage of the geographical unity of Central Europe. The third-century crisis gave a new twist to an older antinomy of imperial power: the ambiguous role of the Principate as a “lifebelt” and a “millstone” (Drinkwater 2007).

Discussions of religious life in the Roman Empire have commonly distinguished between religious policies of the imperial center, religious developments triggered by imperial rule and often tolerated rather than initiated by the rulers, and religious responses to new situations brought about by the impact of empire that were not *ipso facto* compatible with the strategies of imperial integration. It is, of course, no less important that the boundaries between the three categories were redrawn in momentous ways: the most autonomous and innovative religious response, suspect from the outset and later subjected to systematic persecution, was in the end co-opted by the center, translated into new and increasingly exclusivist religious policies, and adopted as the framework for a redefined imperial identity. But during the pre-Christian phase, the interaction of imperial power and religious culture resulted in a more ambiguous constellation: while it seems justified to speak of a “religious koine on the territory of the *imperium Romanum*,” this did not amount to a “homogeneous religious space of imperial domination” (Hahn 2004: 7).

The main point that must be added to this description is that the new conditions of religious life proved conducive to major religious transformations. No other empire seems to have provided a comparably fertile ground for that type of innovation. The rise and triumph of Christianity must be understood in this context. On the one hand, the Christian redefinition of monotheism was one of several alternative forces disputing the religious field circumscribed but only in part controlled by the empire, and affinities with the others have often been noted. The rabbinic transformation of Judaism unfolded at the same time as the Christian separation from it, and the relationship between the two processes remains controversial. Historians have extensively studied a broad and multifaceted trend towards monotheism within the pagan religious universe but found it difficult to define a common denominator. The category of “oriental religions” has been questioned, but cults and creeds from the eastern frontier of the empire, as well as an incipient world religion with roots further east (Manichaeism) were in any case involved in the changing composition of the religious koine. On the other hand, distinctive features of Christianity – from its unequaled capacity to build a counter-society to the ideas of incarnation and martyrdom – have been invoked to explain its success.

Guy G. Stroumsa’s analysis of religious mutations in late antiquity links up with these debates. As he argues, it is useful to describe the changes taking place under Roman imperial rule and culminating during its last phase as religious mutations – not just in the sense of a new imperial religion replacing the old one but with reference to a transformation of the very concept of religion. Scholarship in this field is still too one-sidedly preoccupied with the rise and triumph of Christianity; even when qualified by some interest in related developments within other religious
traditions, this perspective is too narrow to do justice to the processes in question. If we want to delimit a common ground, the “end of sacrifice” may seem the most obvious symptom of change; but on closer examination, this turns out to mean a whole spectrum of transformations (including Christian notions of martyrdom), rather than an end to the very idea of sacrifice. The religious mutations unfold on multiple levels; they lead to new conceptions of the role of texts in religious life, as well as to a shift from civic to communitarian frameworks. New types of religious virtuosi – the sage, the Gnostic, and the saint – come to the fore. In all these respects, different religious cultures develop variations on shared themes. To grasp the complexity of the picture, we must take into account the changing relations between Christianity and a mutating Judaism (the Judaic origins of Christianity are only one aspect of this complex constellation), as well as the rival revealed and universal message of Manichaeism and the far-reaching changes affecting the Greco-Roman religious imaginary.

Scholarly interest in and recognition of late antiquity has – especially during the last three decades – brought about major changes to our views of ancient history. As debates on this subject progress, it seems possible to construct a sequence of interpretive approaches. The first step was the reassessment of a historical epoch, now credited with more original characteristics and more substantial achievements than before. The case had to be argued in chronological terms, but it was never easy to agree on conclusive dates. A widely accepted version equated the beginning of late antiquity with the late third-century reconstruction of the empire and the end with seventh-century transformations in east and west, but it was contested by those who wanted to go back to the end of the Antonine era and forward to around 800 (the age of Carolingian and Abbasid ascendancy). A more theoretical stance found expression in the idea of a paradigm change, from the decline and fall of the Roman Empire to the transformation of the Roman world. This approach was less dependent on chronological landmarks. There was no a priori reason to assume that the dynamics of change in different parts of the Roman Empire (and in regions beyond its borders but involved in the overall transformation) would unfold in uniform or synchronized ways, nor that the story would come to an end everywhere at the same time.

A third step was taken with the attempt to impute a general historical logic to the trajectory of late antiquity. The road from empire to commonwealth, analyzed by Garth Fowden in his book on the political consequences of monotheism (Fowden 1993), can be seen as a model of that kind; the explicit aim of the book was to identify a late antique precursor to the more familiar Byzantine commonwealth, but the argument can also be taken to suggest a pattern that was developed in different ways by the three post-Roman civilizations. The commonwealth represents a grouping of political formations, sometimes but not necessarily structured around an imperial center, and always embodying a civilizational affinity beyond imperial control. Fowden’s contribution to this volume shifts the focus to another level; it represents a fourth approach to late antiquity, but in this case the historical boundaries of the period are called into question, and the first millennium CE is proposed as a more adequate frame of reference. The emphasis is now on the development
of monotheistic religions. As Fowden sees it, the three main monotheistic traditions – Judaism, Christianity, and Islam – all went through their most formative phases in the first millennium. In comparative terms, Islam seems most important to Fowden’s argument, both because of its success as the world religion (and civilization) par excellence around the end of the first millennium, and in view of its aspirations to represent a purified form of monotheism. Fowden’s model of religious development – unmistakably Weberian in spirit, but never formulated in these terms by Weber or his followers – distinguishes between a prophetic, a scriptural, and a patristic stage. The last term is well known from the history of Christianity, but has so far not been applied to Judaism or Islam. If it be objected that the first millennium includes only the patristic stage of Judaism, Fowden’s response would obviously be that in this case the product of that stage – rabinic Judaism – was so different from preceding versions of the reformed tradition that the dividing line can be justified.

Successors, Adversaries, and Others

The dynamics of transmission and transformation in the three post-Roman civilizations differed from case to case. Some fundamental similarities between the two Christian ones set them apart from the Islamic world, but they also differed from each other in significant ways. In the western parts of the empire (the regions that became the historical domain of Western Christendom), the Christianization of barbarian kingdoms, in most cases complicated by temporary conversion to the “Arian” heresy, succeeded the incomplete Christianization of the empire. A recent major work on the beginnings of Western Europe (Dumézil 2005) argues that this aspect of the transition from the ancient to the medieval world has been neglected. On the other hand, the kingdoms appropriated cultural, institutional, and organizational aspects of the imperial legacy, and the outcome was in each case determined by varying circumstances: ethnic composition, geopolitical fortunes, dynastic vicissitudes, and church-state relations, to mention only the most conspicuous factors at work. Among the post-Roman regna and gentes (Goetz 2003), the trajectory of the Franks and their kingdom is – for a whole range of reasons – a particularly instructive and decisive example. It constitutes a very long story, from early beginnings on the contested imperial frontier to uncontested primacy among the claimants to the imperial heritage. It unfolded without interruption by imperial attempts at reconquest or by stronger rivals among the barbarian kingdoms. An early entry into the Roman Church, without the Arian detour taken in comparable cases, led in due course to a particularly close relationship with the papacy, and this factor helped to engineer a change of dynasty that also strengthened the monarchic center. Last but not least, it was the kingdom of the Franks that took the imitatio imperii – attempted or imagined, to a varying degree, by some of the other kingdoms – to its ultimate conclusion and created an imperial state that became central to the civilizational formation of Western Christendom.
Matthias Becher’s chapter traces the progress of the Franks as Rome’s heirs in the west. Much of the story revolves around the ups and downs of the Merovingian dynasty. Paradoxically, the crowning success of the Franks – the rise of the Carolingian dynasty and the restoration of empire under Charlemagne – entailed a reinterpretation of their historical record and a downgrading of their pioneering state-builders. The Merovingians were of much greater historical importance than the version long accepted by later generations would have it. At its strongest, the Merovingian kingdom ruled over a very large part of the future Carolingian empire. The monarchic order constructed during this period was the basis for Carolingian aspirations to more power and higher status, even if dynastic change also meant some upgrading of the monarchic institution. Becher is skeptical about Germanic roots of kingship among the Franks and argues that the sixth- and seventh-century monarchy, as we know it from historical records, was a post-Roman creation, drawing on Roman sources but adapting them to new circumstances. This view implies a strong emphasis on developments under Merovingian rule. It is true that the center was weakened by enduring structural problems and conflicts. Merovingian rulers had to cope with a lasting (albeit not definitive) division of the realm and a devolution of power in favor of dominant aristocratic families. The former problem was, as Becher argues, due to changing power balances rather than to any inbuilt weaknesses of Merovingian kingship; the latter was solved by dynastic change and reconsolidation. Finally, both the crises and the successes of the Frankish kingdom should be seen in the broader context of an emerging Western Christian civilization. For the self-strengthening strategies of the monarchy, especially during its Carolingian period, the combination of close relationships with the papacy and with the Anglo-Saxon Church was of major importance. The Anglo-Saxon connection is particularly interesting because it involved a branch of Christianity that had taken shape long after the breakdown of the local Roman order, developed some specific characteristics (not least through contact with the originally extra-Roman Irish version of Christianity) but subsequently aligned itself with the papal order.

Among the successor civilizations, the Byzantine is by far the most directly linked to the Roman background – so much so that it poses particularly difficult questions about periodization. The relocation of the imperial center to Constantinople is sometimes taken to mark the beginning of a Byzantine empire that lasted until 1453; this view seems incompatible with the new understanding of late antiquity as a distinctive period prior to the division into three civilizational domains. More complex issues arise in connection with the “age of Justinian,” as it is commonly labeled by historians. There is no doubt that Justinian’s strategy of reconquest and all-round reaffirmation of empire was grounded in a late antique context but, at the same time, its failure and the unintended consequences foreshadowed the end of an era and the beginning of a new one. Moreover, recent work on the sixth-century plague and its impact on history (Meier 2003) has raised new questions about traditional criteria of periodization. That said, there is still a strong case to be made for a seventh-century transition that separates Byzantine civilization from late antiquity. The idea of an early seventh-century refoundation of the empire,
most memorably defended in Ostrogorsky’s influential history of Byzantium (1968) has been abandoned. But later historians, notably John Haldon (1990), propose a more gradual and multi-faceted seventh-century transformation, affecting economic structures, forms of urban life and – less conspicuously – the relationship between religious and political frameworks of imperial power. Some analyses suggest that the eighth century, with its double-edged record of successful defense against threats to the empire and abortive religious reform, may be seen as a second phase of the transformation.

Be that as it may, the fact that the Byzantine empire continued to identify with the Roman makes it more difficult to speak of an “end of Rome” than in the eastern and western neighboring regions. Haldon’s contribution to this volume, building on his earlier work, deals with a whole spectrum of major changes that seem to add up to a historical watershed, even if the empire was neither dismembered nor conquered. The transformation that marks the beginning of Byzantine history in the narrower sense was brought about by a complex interplay of geopolitical and social factors. An exceptionally traumatic sequence of imperial offensives and defeats – from Justinian to Heraclius – led to massive readjustments, and their ramifications in turn affected the whole social fabric. Haldon stresses two conspicuous and dominant trends. On the one hand, a recomposition of the elite – at the expense of the senatorial aristocracy – strengthened the emperor and enhanced the central role of the state in economic and social life. This state was neither all-powerful nor conflict-free (Haldon describes it as “a collective, many-headed and internally inconsistent set of relationships and structures”), but its massive presence was for many centuries a defining characteristic of Byzantine history. On the other hand, the seventh century saw a “marked retrenchment of urban life” (not to be mistaken for a wholesale collapse); regional variations were certainly not insignificant, and many aspects of the process are disputed, but the overall trend seems clear, and it contributed to the strengthening of a more exclusive imperial center. Beyond these structural changes, there was an ideological side to the Byzantine transformation: a reconfiguration of the relationships between imperial, spiritual, and divine authority had to be undertaken, not least because of the simultaneous geopolitical disasters and internal consolidation of the empire. But Haldon’s conclusions on this point are more cautious, and he is less willing than some other historians to see the iconoclastic controversy of the eighth century as a key episode.

It has taken some rethinking of the historical record to vindicate the view of Islam as a product of late antiquity and a successor civilization related to the Roman Empire. A basic precondition for that shift of focus was a broad perspective on the transformation of the Roman world; in this context, the peripheral region where the “venture of Islam” (Hodgson 1974) began can be seen as a part of the changing larger world within and beyond the imperial borders. Another connection has been established through the interpretation of Islam as a response to the problems and schisms of Christian monotheism inside and outside the empire. Last but not least, a clearer distinction between the emergence of Islam as an expansive and politically dominant religion, on the one hand, and the much
more gradual formation of an Islamic civilization on the other, has highlighted the importance of multiple Greco-Roman legacies for the latter process. From legal and urban institutions to scientific knowledge and philosophical reflection, aspects of the classical world inherited by the new civilization have been analyzed in detail, and some surprising lines of transmission emerge from that work – including, for example, Peter Brown’s reconstruction of links between the classical notion of paideia and the Islamic idea of adab (Brown 1984). On the other hand, the whole problematic of an Islamic connection – or affiliation – to Greco-Roman antiquity has to be considered in relation to another factor that distinguishes this part of the post-Roman world from the others: Islamic civilization incorporated the conquered territories and the cultural resources of Rome’s Persian antagonist, and the importance of this other background to the emerging synthesis is also a major theme in recent scholarship.

Chase Robinson’s chapter discusses the early Islamic record of conquest, state formation, empire-building, and civilizational synthesis; connections and main lines of comparison with the Roman predecessor are also noted. It would be hard to deny that the Islamic empire, ruled first by two Umayyad dynasties and then by the Abbasids, was Rome’s principal successor. Although it failed to encompass the Mediterranean, an imperial domain extending from Spain to Central Asia was an unprecedented achievement; the heartland of this realm also became the core of trading networks extending much farther east. And although the in-depth Islamization of the conquered territories took place after the imperial center had lost its grip, the religious unification initiated by conquest was in the long run more effective in imposing a civilizational framework than the Christianization of the Roman Empire had been. Key preconditions for this remarkable success were created during the early phase of expansion; this period is, as critics of the traditional account have shown, very difficult to reconstruct in detail, but some basic points seem well established. The first onslaught on the imperial powers in place would not have been possible without a very high degree of mobilization, fueled by religious fervor; but it is less clear whether the conquering community led by the prophet Muhammad can already be called a state. State formation began in earnest after the first conquests and was from the outset closely linked to empire-building. The new rulers built their imperial state with the aid of elites and apparatuses that survived the fall of Byzantine and Sasanian power; this strategy, accompanied by selective borrowing of cultural resources, seems to have varied in specific respects from one conquered province to another. But what made the long-term impact of Islamic expansion unique and incomparable with other imperial ventures from the periphery was the ability of the Arabs to impose both their language and their religion. This was decisive for the civilizational outcome of the process that began with conquest in the name of revelation.

If the Greek world is the most obvious source of comparative insights for historians dealing with the Roman ascent from city-state to empire, interpretations of the imperial phase – the late Republic, the Principate and the late antique mutation – call for broader perspectives. Among other cases, the much older imperial traditions
of the ancient Near East, whose domains were in part inherited by the Romans, must be taken into account. Comparisons in this vein have tended to focus on Assyria, commonly seen as resembling Rome in that a city-state became an imperial power, and also as having taken a decisive step beyond the more limited imperial projects of earlier phases, toward the vision of universal empire subsequently pursued on a larger scale by the Persians. Even before entering into details, some qualifications to this claim may be suggested. The rise of Assyria as an imperial power began after several cycles of imperial expansion and decline in the Mesopotamian heartland. Scholars disagree on the chronology of this tradition (some would argue that imperial ambitions, to some extent successful, were characteristic of the first Mesopotamian states), but even if we prefer a shorter version of the story (such as the traditional view of Akkad as the first empire), it is clear that the Mesopotamian city-state culture went through successive phases of imperial growth followed by decay. By contrast, early Roman expansion took place in a territorial setting where no imperial ancestors had been active (it is one of the salient facts about Etruscan history that none of the most powerful city-states ever achieved a hegemony comparable to Greek examples, let alone an imperial status, and hegemonic ambitions among the Western Greeks did not get beyond control of Sicily by some Syracusan rulers). The Romans invented their mode of imperial rule in relative isolation, and when their geopolitical entanglement with the Eastern Greek world began, they already represented a superior power. Another difference to be noted is that the Assyrian empire itself had its phases of successes and setbacks: historians distinguish between late second-millennium, ninth-century, and eighth-to seventh-century periods of Assyrian ascendancy. There is no comparable pattern in Roman history. Finally, the last and extreme phase of Assyrian expansion seems to have been an exemplary case of imperial power undermining itself by its destructive methods and making itself vulnerable to sudden assaults from the periphery. Assyria would thus, as Mario Liverani (2008) has argued, come close to the model of self-inflicted decline and fall, applied to Rome by earlier historians but now abandoned. On this view, the ancient image of Assyria as the inaugural world empire was linked to a vision of collapse that drew on historical experience but was projected on to different trajectories of later date.

Liverani’s contribution to this volume takes a cautious line on comparing Rome with Assyria, and stresses the need for clear definitions of the formations to be compared. Recent interest in comparative perspectives on city-states and empires seems to have been accompanied by a certain tendency to redefine both categories in less precise terms. Liverani accepts that a return to more narrowly circumscribed criteria still leaves us with prima facie parallels between Roman and Assyrian trajectories; but when it comes to details, he is – on balance – more inclined to stress the contrasts. One interesting and very un-Roman feature of the Assyrian record, obviously related to others, is the changing role of the urban center: successful expansion repeatedly led to the foundation of new capitals (some more durable than others), while Assur remained an uncontested ceremonial and religious center. No such shifts accompanied the rise of the Roman Empire, and it can be assumed that
this tendency to separate political and military power from its sacral background had something to do with the equally striking fact that Assyrian rulers were – in stark contrast to Roman emperors – never deified.

We noted earlier the particular importance of comparisons between Rome and China and the growing interest in that line of analysis. The specific issues most relevant to the themes of this volume have to do with the dynamics of state formation and empire-building. Recent work on these two outstanding cases of imperial integration tends to look for parallels between the processes that led to comparable results (Gizewski 1994; Scheidel 2009). A more balanced approach might stress different backgrounds and configurations. City-states existed in ancient China, but no developmental phase was dominated by a broader city-state culture. Conversely, there was no Mediterranean counterpart to the Zhou empire that dominated the Chinese world in the early centuries of the last millennium BCE, nor to the eminent role which cultural traditions ascribed to it came to play in Chinese history. This was clearly not an empire of the same kind or strength as the one later created by the Qin and Han dynasties, but it did impose a hegemonic order whose crisis then gave rise to the “warring states” of a later period. It is one of the most basic facts about early Chinese history – noted by Scheidel (2009: 15), but worth emphasizing even more than he does – that the rival centers “created stronger state structures than anywhere in Europe prior to the modern period.” By contrast, rivalry and endemic warfare between the Hellenistic kingdoms did not (with the partial exception of Egypt under the Ptolemies) result in any sustained upgrading of state structures. It seems likely that this latter exception to an otherwise widespread pattern had something to do with the very different origins of the two state systems. The “warring states” grew out of a long-drawn-out fragmentation of the Zhou regime, accompanied by local changes to the structures of social power; the Hellenistic monarchies were much more contingent outcomes of a failed imperial takeover. Qin, the Chinese state that eventually triumphed over all the others, had its home base in a western frontier region and has in that regard been compared to Rome; but it was obviously more integrated into the Chinese cultural world than Rome on its road to empire was into the Hellenistic one. Finally, the Chinese empire was taken over by the Han dynasty after the very short rule of its Qin predecessor, and although – as Michael Loewe shows in his chapter – traditional notions of an instant shift to a new order are misguided, the Han succession did mark the beginning of a long adaptive phase which in the end gave rise to a different kind of imperial order. There is no parallel to the Qin–Han sequence in Roman history.

In view of these divergent approaches, it seems advisable to base further discussion on a careful overview of Chinese imperial methods of control and regulation. This is the task undertaken in Loewe’s chapter; the core structures of the Chinese pattern are described, with particular stress on the forms of administration and the state-centered social order, as well as on penal law. References to Rome indicate specific points for comparison, but can be expanded beyond Loewe’s explicit formulations. A basic difference, more or less directly related to many others, is that the Chinese Empire did not rule through a decentralized regime of urban
communities with limited autonomy (or residual city-states, as they might also be called). Its mechanisms of central control were correspondingly more developed than the Roman ones; despite some corrections to the received image of Han rule, it seems beyond doubt that China represented a much more bureaucratized type of empire than Rome. A higher degree of homogeneity also seems to have prevailed on the level of social structures. And a final point to be mentioned is that in the Chinese Empire there was nothing comparable to the Greco-Roman division of the Roman one, with its multiple structural and historical consequences.

Among the adversaries of the Roman Empire, Sasanian Persia claims a unique place. Far more than its much less stable Parthian predecessor, it represented an imperial other, capable of sustained and intermittently successful challenges. It was, in short, the late antique antagonist par excellence. The Sasanians ruled the eastern part of an erstwhile imperial domain that had been unified under the Achaemenids, more briefly under Alexander and the Seleucids, but could never be brought under Roman control; although Roman accounts probably exaggerated Sasanian claims to the Achaemenid heritage, the enduring division of the once unified Near East was enough to pose a particular problem for an empire aspiring to universal primacy. The geopolitical importance of the relationship between Rome and Persia calls for a closer look at contrasts and parallels between the two imperial regimes. In the context of late antiquity, this includes changing constellations of political and religious power. Some earlier interpretations saw the Sasanian alliance with the Zoroastrian religious establishment as the first example of state-enforced orthodoxy, followed by the fourth-century Christianization of the Roman Empire. Recent scholarship tends to prefer a more nuanced picture of Sasanian religious policies, so much so that a comparison with the brief phase of relative tolerance after Constantine’s conversion might be suggested. Zoroastrianism was a favored and dominant religion, but not fully identified with the state. The other currents of Persian religious culture included Manichaeism, a more explicit and ambitious attempt to found a universal religion than anything that had preceded it. Although not of Persian origin, an early and formative episode in its history – royal protection followed by a severe backlash – took place under Sasanian rule. It is worth noting that even after the backlash, the perceived Persian connection was from the viewpoint of Roman imperial authorities a reason for persecuting Manichaeism more thoroughly than Christianity.

There are, in short, good grounds to include Sasanian Persia among the references for comparative perspectives on the Roman Empire. The specific focus of Scott McDonough’s chapter is on the achievements and characteristics of the Sasanians as the last representatives of the imperial tradition of the Ancient Near East. Their significance in this context has been obscured by an exclusive focus on the confrontation with Rome. There is no doubt that the Sasanian empire was in decisive respects inferior to Rome (for one thing, it had no command of the sea), but that is not a reason to neglect its specific strengths. It ruled over far-flung territories for more than two centuries; moreover, geographical variety was reflected in different patterns of culture and power. The balance between “the military power of the highlands and the economic powerhouses of the plains” was never
easy to maintain, and during the later phase of Sasanian rule, it shifted toward the plains. Some practical measures and ideological models of social reorganization accompanied this shift, and the heretical movement known as Mazdakism – favored by one of the Sasanian kings – seems to have been linked to it. The recentering of the realm was cut short by military disasters, first after a long struggle with Byzantium and then on the new front opened up by Islamic expansion. Sasanian responses to ethnic and religious diversity constituted a more lasting legacy and were of some importance for the new round of Near Eastern empire-building that began with Islam. Despite an enduring close relationship with the Zoroastrian priesthood, the Sasanians moved toward more pluralistic religious and cultural policies than their main adversary had ever envisaged.

As mentioned in the first part of this introduction, the uniquely prominent place accorded to Rome in Western visions of world history reflects a long record of genealogical claims: Western imperial projects, both those striving for sole rule in Europe and those implemented on a more global scale, have invoked Roman models and achievements. Comparative perspectives based on this connection must, however, be qualified in light of the basic fact that none of the self-proclaimed successors ever reached the same level of mastery within their historical world as the Romans had done. At the same time, references to the Roman tradition as well as imaginary amalgamations of ancient and modern patterns were frequent in the cultural repertoire of states developing in the post-imperial arena. As Peter Fibiger Bang notes in his chapter, this ongoing mixture of imitation and adaptation has affected the agenda of modern scholarship: for example, Theodor Mommsen’s analysis of the legal framework of the Roman state – the most ambitious and seminal work of its kind – applied the categories of modern constitutionalism to the Roman experience. Reactions against such approaches have sometimes led to dismissive conclusions about the Roman Empire, seen as an example of more general premodern limits to the reach and the development of state power (David Cohen and J. E. Lendon argue against that line of interpretation). On the other hand (this brings us to the main theme of Bang’s chapter), the focus on Rome’s European posterity has obscured the possibilities of comparison with empires and imperial traditions in other parts of Eurasia. To move in that direction is not to suggest a leveling conception of premodern empires or a uniform picture of their backwardness in relation to modern criteria. Rather, the contrasts and parallels to be clarified will reveal both specific strengths and weaknesses of imperial power, as well as different ways of combining them. Within the Eurasian frame of reference, Islamic empires stand out as particularly important, because of their geopolitical dimensions. The empire that grew out of the initial phase of Islamic expansion incorporated a large part of its East Roman predecessor, and the early modern phase brought most of the remainder under Islamic rule; there were no Islamic conquests in East Asia, but the final stage of Islamic empire-building took place in an environment transformed by the Mongol expansion that had also changed the course of East Asian history. In that sense, the imperial ramifications of the Islamic breakthrough extend across the Eurasian macro-region.
Among Islamic empires, the Ottoman is the most obvious case of Roman connections, and it is Bang’s starting point for a broader survey. The Ottoman claim to the Roman imperial title, after the conquest of Byzantium and on the eve of further expansion, has not been taken as seriously by historians as it merits. This symbolic upgrading was clearly crucial to the self-understanding of the empire in its most ascendant phase. Bang observes in passing that we may even find traces of the relationship between Rome and Persia in Ottoman attitudes to the Persianizing Mughal Empire. But the main point of his argument is to show that the Ottoman version of supreme rulership, which mixed Roman traditions with Islamic and Inner Eurasian ones, is particularly relevant to the comparative imperial history he envisages. The images of universal super-kingship can serve as interpretive keys to the complex power structures they help to integrate. A closer look at prominent cases, with particular reference to Rome and the Mughal Empire, shows the links between multi-faceted representation of imperial power, multiple supporting elites, and diverse cultural traditions. Caution is in order when we try to generalize about Eurasian empires, but one of the most compelling conclusions emerging from Bang’s survey is that “universal power was … a heterogeneous composite and imperial culture a laminated and hierarchical amalgam.” This does not lead to a minimalist view of the power exercised by the rulers and conquerors in question: imperial government was, as the concluding sentence sums it up, “spread thinly, but could be concentrated and applied with great intensity.”

Comparative and Genealogical Reflections

The reflections summarized above stressed the need for comparative historical approaches and at the same time the underdeveloped state of the art in the particular field to be explored here. Comparative history depends on conceptual frameworks and theoretical perspectives, more explicitly and decisively so than narrative history in the traditional mold; it is therefore appropriate to add some comments on this aspect of our problem. Trends and traditions in social theory vary in regard to their interest in Greek and Roman antiquity, as well as their ability to make sense of its historical experience. A strong Greco-Roman connection was characteristic of classical sociology (this applies less obviously to Durkheim than to Marx and Weber, but as analysts of his work have shown, the image of traditional society in his early major work on the division of social labor can be traced back to Fustel de Coulanges’s interpretation of the ancient city). The post-classical phase, especially the middle decades of the twentieth century, was much less attuned to that background. But for our purposes, the revival of the classics from the 1960s onward seems more important in virtue of its role in a more general renaissance of historical sociology, than because of any direct return to Marxian or Weberian views on the ancient world. Closer links between basic concepts and historical perspectives are the first precondition for any theoretical input into the understanding of specific historical periods, and the Greco-Roman trajectory is, if anything, a
particularly pertinent case. The strong influence of evolutionistic models, one of the main obstacles that historical sociology had to overcome, also obstructed understanding of Greco-Roman historical patterns – very different from the leveling image of traditional society preferred by evolutionary theorists – as well as of their divided legacies. Conversely, a closer analysis of the classical background to European history could lead to doubts about evolutionary schemes. For example, Perry Anderson’s reconstruction of the European path to modernity resulted in a major departure from traditional Marxist views: it was the whole sequence of antiquity and feudalism, rather than a self-contained “transition from feudalism to capitalism,” that had to be taken into account to explain the outcome and the prospects of the European trajectory (Anderson 1974a, 1974b). This was one of the most interesting neo-Marxist research programs formulated at the time, but it was never developed further.

The historical turn of sociology has adapted to a variety of conceptual frameworks. Among these alternatives, the civilizational approach has some distinctive features. It stresses the plurality of historical trajectories, envisages the most important ones as large-scale and long-term formations, and allows for changing relationships between cultural, political, and organizational factors. Johann P. Arnason’s chapter explores civilizational aspects of Roman history. One of the most obvious specific features is the centrality of the political sphere, but not in the same sense as in the Greek city-states for which this claim has also been made. In the Greek case, the cultural focus on politics was compatible with exceptional creativity in other fields but not conducive to durable political arrangements; neither a shared model of political order nor a unified structure of political power emerged from internal Greek developments. In the Roman case, a uniquely successful ascent from city-state to empire overshadowed everything else. The civilizational significance of this process should be seen in light of the particular place and role attributed to the city in the Roman imaginary, and this core component of political culture was in turn backed up by distinctive forms and orientations of religious life.

As noted in the first section of this introduction, Rome has traditionally been seen as an ancestor or an early incarnation of Europe, and this perceived connection has much to do with the historical prominence of the Roman record. Several versions of the genealogical claim may be distinguished. The most sweeping one equates classical antiquity with the first phase of European civilization, and Rome becomes a part of that sequence. Although this view still seems to retain some ideological appeal, it is now hardly represented in scholarly debates: its massive underlying assumptions of cultural continuity have proved too vulnerable to criticism. A more limited version argues that Roman expansion north of the Mediterranean region – beginning with the conquest of Gaul – was a decisive step toward Europeanization. While there is no denying the epoch-making importance of this move into new territories, it cannot be regarded as more than a precondition – one of several – for the emergence of Europe as a historical formation. The most sustainable variant of the claim centers on Western Christendom as one of the three successor civilizations of the Roman Empire, and on its eminent, though not
exclusive, role in the creation of Europe. But as the interpretation thus moves closer to the historical record, it also exposes itself more directly to historical criticism. The road from the Roman world to a recognizable version of Europe must be traced through multiple paths (such as the trajectory recapitulated in Becher’s chapter); more research and debate on the input from other civilizational currents beside the Western Christian one – Byzantine, Jewish, and Islamic – will also be needed. From a long-term overall perspective, Europe must be understood as a multi-civilizational constellation, however dominant one of its components may have been.

But the questions to be confronted are not only historical. Any construction of genealogical links between Rome and Europe is bound to pose problems of a more theoretical kind. In particular, claims about historical continuity must be judged in light of contemporary discussions about the interpretive, innovative, and institution-building capacities of social actors. This is the perspective explored in Peter Wagner’s chapter and brought to bear on specific aspects of the Roman legacy, supposedly central to the making of Europe. As a first step, Wagner proposes to disaggregate the question of civilizational affiliation into four major and separate issues. The problem of institutional continuity above all has to do with Roman law and its multiple uses in medieval and modern Europe. Assumptions about cultural continuity are primarily linked to the central role of Christianity and to the transformations of Christian themes and ideas in later stages of European culture. A third question concerns the patterns of legitimate authority foreshadowed by the late antique combination of imperial supremacy with a varying and never clearly defined autonomy of the papacy, and developed much further through the medieval separation of empire and papacy. Finally, claims referring to a Roman input into the European traditions of individualism must be considered. Wagner regards the debates on all four subjects as inconclusive, and argues that we must not only relate the issues to broader contexts of historical transformations but also to the ongoing theoretical clarification of relations between human action, historical situations, and cultural resources. The question of the Roman legacy – or, more precisely, legacies – remains important and relevant to other central problems of European history, but it should be tackled in more pluralistic terms and seen as a part of a more complex research program. Wagner’s conclusion, disputed by some other contributors to the volume but certainly located within a shared universe of discourse, is that a civilizational view of the Rome–Europe relationship is premature; what we need at this stage is comparative and interpretive history, unburdened by traditional but now unconvincing assumptions.

References