

Approaching Ottoman History

AN INTRODUCTION TO THE SOURCES

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INTRODUCTION

In this book I hope to share with my readers the fascination with Ottoman sources, both archival and literary in the wider sense of the word, which have become accessible in growing numbers during the last decade or so. The cataloguing of the Prime Minister's Archives in Istanbul advances rapidly, and various instructive library catalogues have appeared, both in Turkey and abroad. On the basis of this source material it has become possible to question, thoroughly revise, and at times totally abandon, the conventional images of Ottoman history which populated the secondary literature as little as thirty years ago. We no longer regard Ottoman officials as incapable of appreciating the complexities of urban economies, nor do we assume that Ottoman peasants lived merely by bartering essential services and without contact to the money economy. We have come to realise that European trade in the Ottoman Empire, while not insignificant both from an economic and a political point of view, was yet dwarfed by interregional and local commerce, to say nothing of the importation of spices, drugs and fine cottons from India.

Not that our methodological sophistication has at all times corresponded to the promises held out by these new sources, far from it. But some stimulating novelties are visible, such as the growing interface between art history and political history of the Ottoman realm, and an awakening interest in comparative projects shared with Indianists or Europeanists. Many Ottomanist historians now seem less parochially fixated on their particular speciality and willing to share the results of their research with representatives of other fields. Paradoxically, the recent growth in the number of available sources has led to a decline in the previously rather notable tendency of Ottomanists to identify with 'their' texts and the points of view incorporated in them. Many of us indeed have become aware of the

dangers of 'document fetishism'. By this exotic-sounding term we mean the tendency to reproduce more or less verbatim the statements of our primary sources and the associated unwillingness to use logic and/or experience of the relevant milieu to interpret them (Berktay, 1991). More Ottomanist scholars appear to follow research going on in related disciplines – even though we still have a long way to go before Ottoman history becomes a branch of world history *à part entière*.

In certain instances, Europeanists or Indianists have responded to these developments by showing a degree of interest in Ottoman society. International projects treating trade guilds, the business of war or the movements of gold and silver will now often include an Ottomanist historian, even if the latter may still play the role of the odd man/woman out. Collective volumes treating European economic history will not rarely contain contributions by Ottomanist historians, while until quite recently, chapters on the pre- and/or post-Ottoman histories of certain Balkan territories would have been considered sufficient. Hopefully, the present volume will increase this kind of give and take between Ottomanists and historians of Europe, India or even China, by emphasising some of the methodologically most interesting approaches to Ottoman history.

PRIMARY AND SECONDARY SOURCES

The present book deals with the archival and narrative sources available to the Ottomanist historian, and to a degree, with the historiography which scholars have constructed on the basis of this material. Primary sources constitute the first priority, a choice which is obviously open to challenge. For while the available primary sources condition the kinds of questions an historian may usefully ask, it is also true that we read secondary sources, including non-scholarly ones such as newspapers and magazines, long before we ever embark on specialised training. One might therefore argue, with some justification, that our view of the primary sources is conditioned by the secondary material we have read, often without even being conscious of the fact. As a result, it has taken European historians studying Ottoman–Habsburg or Ottoman–Polish relations a long time to get away from the glorification of the Austro–Hungarian Empire or Poland as *antemurale Christianitatis*, and there are some who have not managed this act of distancing down to the recent past (Barker, 1967). Conversely the celebration of *sefer ve zafer* (campaigns and victories) for a long time has been part of Turkish historiography, and scholars who attempt to demolish this paradigm are not having an easy time either. Less obviously, our knowledge of the secondary literature will often condition the primary sources we seek

and find. Materials that nobody believed to exist even a decade or two ago have been located, once an overriding historiographical concern has caused scholars to look for them. As a recent example, we might mention the case of Ottoman women's history, even though the documents unearthed to date still leave many important questions unanswered.¹

A case could thus be made for discussing current and not so current secondary studies before embarking on a discussion of primary sources. This would also involve a recapitulation of the 'basic features' of Ottoman state and society, on a level of abstraction more or less acceptable to social scientists. Or else one might decide to integrate an introduction to primary sources into a discussion of secondary research. Stephen Humphreys' work on Middle Eastern history of the pre-Ottoman period constitutes a particularly successful example of this approach (Humphreys, revised edn 1995). But in the present book the opposite approach has been taken, namely to proceed from primary to secondary sources. As long as we do not pretend that we approach our primary material 'without preconceptions', it seems equally reasonable to start research into Ottoman history by examining chronicles and sultans' orders, coins and accounts of pious foundations. And since the explosion of available sources during the last few decades has constituted one of the main reasons for writing this book at all, primary sources will form the starting point of our quest.

There is also a subjective reason for thus stressing archival records and chronicles. Throughout my work in the archives, I have been fascinated by the unexpected documents that will crop up, either suggesting new answers to old questions, or more likely, leading the researcher on to a new track altogether. This is particularly true of the eighteenth century, but any period will offer its own lot of surprises. As a corollary, carefully elaborated dissertation proposals may turn out to be unworkable in the archives; but usually the researcher will find documents suggesting new approaches, not envisaged when the proposal was written. Under such conditions, the historian may stick to the old plan against increased odds, or else abandon him/herself to the drift of the sources. But for the sake of mental stability, it is good to expect the unexpected, and to regard the unpredictable as part of our common human destiny.

From a postmodernist viewpoint, the approach taken in this book will be considered very conservative. In Europeanist historiography, the last twenty years or so have seen a lively debate on the very foundations of

¹ For examples see Jennings (1975) and Tucker (1985). Their work, which deals with non-elite women, would have been considered impossible forty or fifty years ago.

historical research. It has been proposed that the personal, social or political bias of any writer trying to recover what happened in the past is overwhelmingly strong. Thus it is impossible to relate the divergent stories about any historical event to things 'as they really happened'. As a background for this claim, the historian of historiography may propose a number of factors: as a new generation of scholars has emerged, economic and social history, which formed the cutting edge of historical research in the 1960s and 1970s, was bound to come under attack sooner or later. Moreover while social and economic history certainly is not practised only by Marxists, this field has traditionally attracted socialists, social-democrats and left liberals. As a result, the revival of the Cold War in the early 1980s, economic deregulation and globalisation, in addition to the collapse of 'bureaucratic socialism' in the past decade have left this branch of study wide open to attack. And while certain representatives of the postmodernist paradigm, such as Michel Foucault, have shown a profound interest in history and a social concern for the rights of deviants and handicapped people, many postmodernists were and are specialists of literature with little interest in social phenomena. These scholars are inclined to enlarge the field of their studies by claiming that social conflict and stratification are of scant importance, while annexing both primary and secondary historical sources to the mass of literary material already within their purview. In the perspective of the more extreme postmodernists, the distinction between primary and secondary sources is in itself an illusion. All that remains is a corpus of texts which can refer to each other but never to a reality outside of them (on this debate, see Evans, 1997).

However in the Ottomanist context, this fundamental debate about the legitimacy of history has not so far left any traces. Whether this situation should be taken as yet another sign of the immaturity of our discipline is open to debate. If any Ottomanist historian were to claim that we should limit ourselves to 'stories' without concerning ourselves overmuch with the degree of truth they contain, doubtlessly this approach would be decried on moral and political grounds. Let us consider an example from a different field: extreme historical relativism makes it impossible for Europeanist historians to counter the claims of those who, for instance, propound that the crimes of the Nazis were invented by the latter's opponents (Evans, 1997, pp. 241–2). In a very similar vein, many Ottomanist historians, and that includes the present author, would be very much dismayed by the notion that one cannot argue against the different varieties of nationalist and other mythmaking which all too often beset our discipline. Maybe the immaturity of our field has some hidden virtues after all . . .

SCANNING THE HORIZON: OTTOMAN AND EUROPEAN HISTORY

The undertakings of both Ottomans and Ottomanists only make sense when we relate them to the wider world. We will therefore begin our *tour d'horizon* with the histories of different regions in which the Ottomans were active, both inside and outside the Empire, or which seem especially instructive for comparative purposes. Many students of Ottoman history outside of Turkey have to some extent been trained in European history. Some non-Turkish Ottomanists may first have turned to Ottoman materials in order to obtain a better understanding of historical problems encountered when studying the history of Spain, Russia or the Netherlands. Thus a researcher dealing with sixteenth-century Dutch history may observe that the Spanish armies attempting to conquer the country after its several rebellions (1565–68, 1569–76, 1576–81) behaved in a rather strange manner. Although they were victorious many times, Spanish commanders typically did not follow up their victories but withdrew, and in the end, the Spanish king lost the war. One eminent specialist has tried to explain this enigmatic behaviour by the Spanish crown's Mediterranean wars with the Ottomans (Parker, 1979, pp. 22–35). Whenever the Spanish conquest of the Netherlands appeared imminent, the Ottoman sultans, who were not particularly anxious to see all the resources of the Spanish Empire deployed against them, stepped up the war in the Mediterranean. The Spanish crown, whose supplies of bullion were great but not inexhaustible, saw no alternative but to draw off some of its resources from the Netherlands. As a result, the Dutch rebels were able to maintain themselves. We may feel intrigued enough by this thesis to explore the relations between the Ottoman and Spanish world empires. Remarkably enough, not many scholars have done so, and the 'forgotten frontier' which separated the two empires still largely remains a *terra incognita* (Hess, 1978).

Another example of Ottoman history's allowing us to place European developments into perspective concerns the question of royal absolutism in the sixteenth century through to the early nineteenth. Conventional wisdom has it that sultanic rule was different in kind from European absolutism, if only because in the Ottoman Empire there existed no private property in agricultural lands and no nobility controlling the countryside, which rulers needed to subdue and pacify (Anderson, 1979, pp. 365–366). But recent research has cast doubt on this clear-cut opposition. We have come to understand that particularly seventeenth- and eighteenth-century sultans operated within the constraints of a high-level bureaucracy whose members possessed well-entrenched households. In spite of their apparent

power, these sultans were not nearly as free in their decisions as official ideology postulated (Abou-El-Haj, 1991, p. 44). Remarkably enough, sultanic absolutism really reached its apogee in the nineteenth century, when several sultans sought out the support of European powers to strengthen their rule against rebellious subjects in the capital and provinces (Akarlı, 1988). Under these circumstances, the old question of how early modern European absolutism and sultanic rule compared to one another can be viewed in a new light.

At first, students with a background in European history thus may feel challenged by questions concerning the relations between the Ottoman Empire and the European world. But in time, emphasis may shift. Earlier historians who studied the rich documentation of the English Levant Company or the Marseilles Chamber of Commerce were concerned with European establishments in Syria, Izmir or Egypt. But more recent work has concentrated on the way in which the masses of numerical data provided by European commercial records can be used to shed light upon the surrounding Ottoman society. Daniel Panzac has thus employed the documentation on ships arriving in Marseilles from the Levant (Panzac, 1985). In the eighteenth century such vessels were permitted to enter this port only after presenting a certificate from the French consul resident in the locality from which the voyage had originated. This certificate informed the authorities of the presence or absence of plague in Izmir, Istanbul or Sayda. As a result, a mass of data has come together in Marseilles from all the major ports of the eastern Mediterranean, and Panzac has used this documentation to reconstruct the course of plague epidemics.

But not only epidemic disease in the Ottoman Empire can be studied by a close analysis of French archival records. By examining the many shipping contracts which have survived in the archives of the former French consulate of Alexandria (Egypt), Panzac has demonstrated that by the middle of the eighteenth century, Muslim merchants still constituted the vast majority of all traders freighting French ships in this port. Older historians had believed that by this late date, Muslim merchants had long since vanished from the scene, allowing Christians to take their places (Panzac, 1992). Tunisian historians equally have made good use of the Marseilles records to reconstruct the commercial history of their country, which in the eighteenth century was still an Ottoman province (Sadok, 1987).

In a sense this use of European archives to elucidate Ottoman history is more demanding than the conventional studies of European–Ottoman relations, since one needs to know a great deal about

Ottoman state and society in order to ask the right questions of Levant Company or *Chambre de Commerce* records. But the results are rewarding, as these kinds of studies allow new insights often unsuspected until quite recently. And once the indispensable background knowledge of Ottoman history is acquired, some students may feel that they might as well specialise in Ottoman history pure and simple.

THE BYZANTINE–OTTOMAN TRANSITION

Western European history apart, one of the fields most closely connected to Ottoman history is its Byzantine counterpart. The special status of this field is in part due to the fact that Byzantine history has benefited from the centuries-old traditions of classicism, so that a large number of the extant sources are available in high-quality editions. Ottoman historians used to working from manuscripts or less than reliable editions will often regard this situation with more than a bit of envy. On the other hand, Byzantine history in the narrow sense of the word came to an end in the fifteenth century, while Ottoman history continued into the twentieth. As a result Ottoman documentation, in which archival materials play a prominent role, can be considered as a variant of early modern and modern recording practices. By contrast Byzantine documentation, which requires the historian to deal with large chunks of narrative history and small archives, fits well into the ‘medieval’ pattern.

But the difficulties Ottomanists and Byzantinists have experienced in relating to one another stem less from the differences in source bases than from the fact that the relevant fields have been ‘adopted’ by Turkish and Greek nationalist historiography respectively. There is nothing inevitable in this. I remember the pipedreams of a Turkish archaeologist working on Anatolia, who once wished that Turkish republican ideology had decided to regard Byzantine civilisation as one of the many ‘autochthonous’ civilisations which had flourished in the Anatolian homeland before the immigration of the Turks. For if that had been the case, money for Byzantinist excavations would have been much more abundant . . . European philhellenism, with its tendency to search for Byzantine ‘influence’ everywhere, has further complicated matters. For as a defensive reaction, ever since Fuat Köprülü’s article of 1931, Turkish historians and, in their wake, foreign Ottomanists have tended to play down links between Byzantines and Ottomans (Köprülü, 1931, reprint 1981).

It is only during the past twenty years or so that a certain number of scholars have made serious efforts to circumnavigate these particular shoals. It may not be entirely due to chance that many of the people involved have

at one time or another been linked to institutions on 'neutral' ground, namely the Dumbarton Oaks Center in Washington DC and the Centre for Byzantine, Ottoman and Modern Greek Studies in Birmingham, England. One means of 'defusing' the conflict at least on the linguistic level has been the ample use of Venetian and Genoese sources. Neither Byzantine nor Ottoman, notarial records and commercial correspondences provide information on economic matters, not amply covered by either the Ottoman or the Byzantine documentation of the fifteenth century. A separate field of 'transition studies' has thus come into existence, with its own conferences and edited volumes (Bryer, Lowry, 1986; concerning the historiography, an important article by Klaus-Peter Matschke to be expected soon, hopefully). A major scholar such as Elizabeth Zachariadou has even devoted her life's work to this topic (Zachariadou, 1985).

SCANNING THE HORIZON: OTTOMAN AND ASIAN HISTORY

The study of Ottoman involvement with its Asian neighbours, as well as comparative research into the major Asian empires, constitute relatively new branches of historical endeavour, and researchers concerned with them are still trying to find their feet. As long ago as 1948, Halil İnalcık drew attention to the sixteenth-century attempts of Ottoman governments to maintain liaison with the Central Asian khanates and impose themselves as protectors of the Sunni pilgrims to Mecca originating from that particular region (İnalcık, 1948). For the sixteenth century, İnalcık assumed that Ottoman sultans and their advisers had systematically designed a 'northern policy'. This suggestion did not find favour with the French Central Asianists Alexandre Benningsen and Chantal Lemerrier-Quelquejay, who preferred to think in terms of *ad hoc* reactions to specific challenges (Benningsen and Lemerrier-Quelquejay, 1976). On the other hand, the idea that sixteenth-century Ottoman sultans developed a coherent 'southern policy' has been adopted by many more scholars. From Cengiz Orhonlu to N. R. Farooqi and Palmira Brummett, historians have pointed out that the Ottoman struggle against the Portuguese in the Indian Ocean, the conquest of the Mamluk sultanate, the establishment of bases on the African coasts of the Red Sea as well as the Indian Ocean and last not least, the control of the Hijaz and Yemen were closely linked as part of a political 'grand design' (Orhonlu, 1974; Farooqi, 1986; Brummett, 1994). Under Süleyman the Magnificent (1520–66) the Ottoman state was apparently poised for the conquest of the coastlands of the Indian Ocean. However naval units sent against the Portuguese were lost and it proved difficult to secure long-term control of Yemen and the coasts of western India. Ottoman statesmen then

retreated from the Indian Ocean, concentrating instead on aims closer at hand, such as the conquest of Cyprus and the struggle against the Habsburgs. This set of priorities was to involve the Ottomans in the political struggles of southeastern Europe, while the 'grand design' of controlling both the Mediterranean and the Indian Ocean receded into the background (Özbaran, 1994).

But more modest links between the Ottoman Empire and India endured none the less. In this context, evidence on the important trade between India and the Ottoman provinces of Syria and Egypt has attracted historiographical attention. André Raymond, K.N. Chaudhuri, Halil İnalcık, Halil Sahillioğlu and Katsumi Fukazawa have highlighted the importation of cotton textiles, rice and spices, and the imitation of Indian fabrics by local producers unwilling to relinquish their accustomed market shares (Raymond, 1973–74; Chaudhuri, 1985; İnalcık, 1960a and b; Sahillioğlu, 1985b; Fukazawa, 1987). These studies have also shown that even in the seventeenth and eighteenth centuries, the Indian Ocean trade was by no means a monopoly of the Dutch and English, but that Arab-Ottoman and Indian merchants continued to maintain direct contacts.

A long frontier was shared by the Ottomans and their rivals the Safavid rulers of Iran, which in spite of many wars and conquests, in its northern section corresponds roughly to the present-day frontier between Iran and Turkey. Yet even though Persian was a recognised medium of literary expression at the Ottoman court, and Turkish-speaking tribes played a major role in sixteenth-century Iran, studies of Ottoman–Iranian interactions have remained quite limited in number and scope. A major difficulty stems from the fact that the archives of the Iranian dynasties have for the most part been destroyed, so that it is much easier to reconstruct Ottoman views of Iran than Iranian views of the Ottoman Empire. Many scholars who have approached the topic therefore have studied Ottoman–Iranian relations within the Ottoman context. Bekir Kütükoğlu has discussed the wars and diplomatic crises of the later sixteenth century (Kütükoğlu, 1962). Cornell Fleischer has included an interesting discussion of Iranian immigrants in his path-breaking study of Mustafa Âli (Fleischer, 1986, p. 154). In a fascinating study of the inscriptions of the Süleymaniye, Gülru Necipoğlu has demonstrated how Süleyman the Magnificent had himself depicted as champion of Sunni Islam against Shi'ism (Necipoğlu, 1989). One of the few instances in which historians working on early modern Iran have branched out into the Ottoman Empire concerns the trade of the Armenians, whose far-flung network included Istanbul and Aleppo (Ferrier, 1973).

Even less is known about Ottoman links to China, which for the

most part were not direct but mediated through ports in present-day India and Indonesia. While it has long been known that many Ottoman Sultans collected Chinese porcelain, only the recent excavations of Saraçhane/Istanbul have demonstrated that at least by the seventeenth century, Chinese cups had become something of an item of mass consumption in the Ottoman capital. It has even been surmised that the decline of Iznik fayence owed something to the competition of this Chinese import (Atasoy and Raby, 1989, p. 285). By contrast, the mutual discovery of Ottomans and Japanese was very much a late nineteenth and early twentieth-century phenomenon, and thus belongs to the cultural history of the Hamidian and particularly the Young Turkish period. Through the work of Selçuk Esenbel and her colleagues, we have been allowed the first glimpses of this fascinating story (Esenbel, 1995).

While the study of Ottoman linkages to Asian empires is thus advancing, albeit haltingly, comparative ventures are still fraught with a great deal of difficulty. Again different source bases complicate matters. Ottomanist historians will place a probably exaggerated emphasis upon the archives of the Ottoman central government, while historians of the Safavid period deal mainly with chronicles and local archives. Indianist historians, when addressing economic concerns, have become very expert at extracting information from Portuguese, Dutch and British archives. When dealing with the eighteenth century, this material will be supplemented with information derived from local and even private archives.

Images culled from diverse sources by varying methods are notoriously difficult to compare. But a more serious barrier results from a widespread lack of information concerning 'the other side'. Many Ottomanist or Indianist historians have a reasonable background in European history. But an Ottomanist with even an amateurish interest in Indian history is still a rarity, and the same applies to Indianists knowing something of Ottoman history. Yet even though it may still be premature to study the Ottoman Empire in the context of Asian history, this seems promising in the long run (Togan, 1992).

OTTOMAN HISTORY AND THE SOCIAL SCIENCES

As our next step, we will take a brief look at the manner in which Ottomanists and social scientists have related – or refrained from relating – to one another. Ottomanist historians have almost never generated the paradigms with which they work. Thus a discussion of their relationship to social scientists will lead us to a short and simplistic overview of some of the

debates concerning larger issues of historiography originally outside of the Ottomanist field, but in which Ottomanists have become involved. Occasionally we will encounter economists, social anthropologists and sociologists developing an interest in Ottoman history. This is not so common now as it was a decade or two ago, since presently the social sciences are less interested in regional peculiarities than they used to be, and the American tradition of area studies is under massive attack.

Among the generation of scholars active in Turkey today, many distinguished historians have a background in economics, sociology or regional planning. Students and scholars with a social science background will usually want to know about the processes which formed modern Turkish society, and Şevket Pamuk has published a book which attempts to answer these queries (Pamuk, 1988). Rural property relations, craftsmen inside and outside the guilds, monetary flows or the role of religion in politics will be of special interest to historians with a competence in development economics, business administration or sociology. The studies of Huri Islamoğlu, Murat Çızakça and Şerif Mardin constitute notable examples of this tendency, and Ilber Ortaylı's lively and sometimes sarcastic studies of Istanbul's urban history probably have been informed by this author's background in urban planning (Islamoğlu, 1994; Çızakça, 1996; Mardin, 1962; Ortaylı, 1986).

Social anthropologists doing fieldwork on villages and small towns of present-day Turkey will have somewhat different priorities. Rather than trying to make sense of Ottoman history from a social science point of view, they will be looking for historical background information on the places they study. Something similar applies to architectural historians, particularly those working on vernacular architecture. Anthropologists and architectural historians will often find that there are very few 'academic' studies on the places they are dealing with. They are therefore obliged to make extensive use of monographs written in the 1930s and 1940s by historical amateurs. Social anthropologists and architectural historians in this situation may turn to an Ottomanist in order to find out something about the reliability of such studies, and the possibility of locating hard-to-find primary sources. These questions are potentially enriching to the Ottomanist historian, who otherwise is in constant danger of losing him-/herself in the parochial concerns of our discipline.

Viewed from another angle, social anthropologists, whose primary concern is with the present and the recent past, are often charmed by the idea that the history of this or that village can be traced back all the way to the fifteenth or sixteenth century. Some of them have tried to mine oral

history for accounts of nineteenth-century settlement processes. Or else the realisation that famous local specialities such as the fruit of Malatya or the hazelnuts of the eastern Black Sea coast have been cultivated in these places for several centuries may help to give field studies a sense of historical depth. Now that few social scientists would claim that 'history is bunk', there exists a real possibility of cooperation between Ottomanists and field anthropologists, which the former will ignore at their own peril (Benedict, 1974, pp. 74–90).

OTTOMAN STATE, OTTOMAN SOCIETY

In the 1980s, Europeanist historians and historical sociologists began to react against the historiography of the previous twenty years, during which economic and social history had held pride of place, by *Bringing the State Back In* (see Tilly, 1985). Ottomanists in due course also became interested in this historiographical current. Only in their case, the situation was somewhat paradoxical, as Ottomanist historiography had always been strongly state-centred, and at first glance, there seemed to be little reason for bringing 'in' what had never been 'out' in the first place. But here appearances are misleading, as 'traditional' Ottomanist state-centredness was overdetermined by the example of new-style Marxian historiography on the one hand, and non-Marxist theories of state formation on the other. Comparativists such as Perry Anderson emphasised that the locus of decisive class struggles was always the state (Anderson, 2nd edn 1979, p.11). For Ottomanist historians who, in Anderson's wake, grappled with the question of state structure in a Marxian sense, a major concern was the degree to which a given state bureaucracy was able to operate independently from the surrounding society (Haldon, 1993, pp. 140 ff.). In the European context, this question often had been phrased differently, namely the issue had been to what extent the state bureaucracy was independent from a landholding or mercantile ruling class.² Only in the Ottoman context landholding was closely controlled by the state, so that there existed no ruling class outside of the state apparatus, and this required a rephrasing of the *problématique*. Factionalism within the Ottoman bureaucracy being highly developed, one might usefully debate to what extent the state apparatus was independent of or else controlled by such factions within the ruling class (Kunt, 1974; Fleischer, 1986 pp. 159–161 and elsewhere).

Among Ottomanist historians outside the Marxian tradition and interested in state formation, the work of Charles Tilly has held special

² For a comparison, in these terms, between the Ottoman and Byzantine states see Haldon, 1993.

attraction (Tilly, 1992). This is in part due to his lively polemical style and his stress on the conflict between ordinary people and the states/ruling classes lording it over them. For scholars striving for a new understanding of the role of war in Ottoman politics, the major advantage is that Tilly has placed war-making along with capital accumulation at the centre of his theory of state formation. Moreover this historical sociologist has done concrete work on early modern France. As a large-scale kingdom with a ruling class strongly oriented toward the exploitation of rural society, France before 1789 also has exerted a special attraction on Ottomanists interested in comparative perspectives. Unfortunately, Tilly does not seem particularly interested in the Ottoman case, emphasising as he does urban capitalism as a variable determining the level of coercion exercised by a given state. This concern has led him to concentrate upon intra-European history, and where the present is concerned, on military régimes and US politics in the Third World.

NATIONALIST HISTORIOGRAPHY

Tilly's 'national state' is an organisation controlling a multiplicity of cities and regions, and possessing a strong bureaucratic armature with a degree of autonomy from the society governed (Tilly, 1992, pp. 2–3). It is thus not coterminous with the nation-state, which Tilly regards as a state whose people share a 'strong religious, linguistic and symbolic identity' (p. 3). According to this definition, France, Germany or England never could count as nation-states, even though it is likely that at least many French historians would disagree with Tilly's claim. The difficulty lies in the fact that the ruling classes of most states which are no nation states in Tilly's sense, still claim to govern a nation state, and a large part of the population tends to agree with them.

Most of the Ottoman successor states are certainly defined in their respective national ideologies as nation states, and have produced national historiographies in which the Ottoman period has been accommodated, often with a good deal of difficulty. Many studies could be written about the image of the Ottoman period in the scholarly and popular historiography of this or that present-day state. Yet reflection on different possibilities of conceptualising the Ottoman impact is a fairly new phenomenon. In part, this questioning is due to disillusion. Several rival ideologies and agendas have come to address the fact that in many instances, the establishment of national states and (would-be) nation states has not made life any easier for the populations involved. This challenge has encouraged reflection on the

merits of the nation-state *per se*. In Turkey and the Arab states, some version of Islam has served as a common ground from which historians may question the 'national enmities' which seem to be an indispensable concomitant of the nation-state. Among secularists, the fate of national minorities at the hands of a would-be nation-state often has led to a degree of disillusionment with nationalist ideals, and the civil war in former Yugoslavia has strengthened this feeling.

On the positive side, historians disillusioned with the nation state model have discovered the advantages of plural societies. For a long time, the limited amount of interaction between different ethno-religious communities, or even just populations sharing the same urban space, was considered an irremediable defect of Ottoman society. But this evaluation has now changed. On the one hand, recent research has shown that intra-urban interaction was often more intensive than had been assumed earlier (Faroghi, 1987, pp. 157 ff.). More importantly, the willingness with which empires such as the Ottoman down to the eighteenth century accommodated separate and unequal communities has gained in respectability. Or at least this is true when compared to the murderous attempts at 'national unification' which have been undertaken throughout the nineteenth and twentieth centuries.

In the Turkish context, historians critical of nationalist historiography have often aimed their darts at the presumed uniqueness of Ottoman society, for which Turkish possesses the pretty formulation *biz bize benzeriz*.³ Of course the claim to the uniqueness of a 'pre-existing national essence' is common enough among nationalists the world around. Many of the various state formation theories recently developed, for all their obvious differences, aim at demonstrating the fallacy of nationalist essentialism, by showing the contingency both of national boundaries and of the consciousness associated with them (B. Anderson, 1983). Ottomanists attacking the nationalist notion of uniqueness often do so in the name of making Ottoman history accessible to international comparison (Berktaş, 1991). For only when we acknowledge that an empire such as the Ottoman shared significant features with its neighbours, can we make sense of certain peculiarities which no one would wish to deny. The existence of a peasant base, consisting of individual peasant households running their own enterprises (as opposed to rural labourers working landholdings managed by outsiders)

³ We resemble (no one but) ourselves.

constitutes the basic feature. All comparative work involving Ottoman society must begin from this point (Inalcık, 1994; Berktaş, 1992).

THE ORIENTALIST TRAP

Apart from the dubious claims resulting from nationalism, orientalism constitutes the major trap into which, given prevailing cultural assumptions, many Ottomanist historians are likely to fall. The pervasiveness of orientalist assumptions in secondary studies down to the present day has been shown to us by the critical work of Edward Said and his students. Orientalism involves a persistent tendency to define the Islamic world as the eternal 'other' and an unwillingness to concede that Middle Eastern societies have a history and dynamic of their own. In some instances, such a dynamic may be conceded, but then it is assumed that Middle Eastern history is something *sui generis* and not amenable to historical comparison. It has often been claimed that 'original observation' as opposed to reliance on authority characterised European high culture since the Renaissance. Yet orientalism also involves an excessive reliance on literary sources from long bygone times, so that ancient prejudices get carried over from one generation to the next without much regard for historical realities (Said, 1978, pp. 202 ff.). When discussing the European sources on Ottoman history, this problem must never be left out of sight.

Many elements of the later orientalist world view were originally formulated by seventeenth and eighteenth-century travellers, among whom Jean-Baptiste Tavernier is probably one of the best known (Tavernier, ed. Yérasimos, 1981). This was an enormously enterprising merchant and jeweller, who traversed the Ottoman Empire several times on his way to India.⁴ His travelogue contains a rather dubious account of what he considered 'oriental despotism', a concept which ever since has continued to bedevil European political thought. But his work is still indispensable, for he has produced an extensive account of life on the seventeenth-century Ottoman roads which complements the work of his Ottoman contemporary and fellow traveller Evliya Çelebi (Evliya Çelebi, 1896 to 1938).

Political conceptions apart, a further problem posed by European travellers of the period is the fact that they normally had read their predecessors' work. In some instances, maybe when they had lost their own notes, they might even piece together their accounts out of unacknowledged quotes. This way of proceeding may cause the modern user some disagreeable surprises: when working on Eriwan (Revan), in the seventeenth

⁴ He was to leave France after the toleration of the Protestants had been revoked in 1685 and die, over eighty years old, in the attempt to reach India by way of Russia.

century an Ottoman–Iranian border town, it took me a while to find out that Joseph Pitton de Tournefort, a highly respected French botanist who visited Anatolia and the Caucasus around 1700, had bodily ‘lifted’ his account from the works of Jean Chardin and Tavernier. To be sure, Pitton de Tournefort mentions the work of these authors; but there is no indication that practically *all* his statements on Revan were derived from his predecessors (Tournefort, ed. Yérasimos, 1982, vol. II, pp. 221–4; Tavernier, ed. Yérasimos, 1981, vol. I, pp. 82–6; Chardin, 1711, vol. II, p. 218ff). This tendency to copy ‘ancient authorities’ is far from dead today, even though today’s scholars are more careful about acknowledging their sources. Of course the inclination to accept travellers’ claims at face value reinforces the ahistorical tendency of much European writing on the Middle East, which Edward Said has justly attacked. Given these circumstances, it is tempting to disregard the testimony of European travellers altogether. But since many kinds of information that we urgently need have been preserved only by these authors, we will have to learn to use their work, albeit with a great deal of caution.

As we have seen, there exists a degree of continuity between the travellers of the seventeenth, eighteenth and nineteenth centuries on the one hand, and the nineteenth-century orientalists on the other. Certain scholars, such the Prussian A. J. Mordtmann Sen. (1811–1879) or later the Czech Alois Musil (1868–1944) were in fact both academics and travellers (Mordtmann, ed. Babinger, 1925). Some of these people might combine scholarly and diplomatic roles, the latter of which in some cases shaded off into espionage activity. Thus the famous Dutch Islamicist Christian Snouck Hurgronje at the end of the nineteenth century, even managed to stay in Mecca for a while as the guest of the reigning Sherif. He had been sent by his government to unearth information about the Javanese and other Dutch subjects of present-day Indonesia who went on pilgrimage to Mecca in increasing numbers at the end of the nineteenth century. It was assumed that there they might become imbued with Pan-islamic sentiments, and upon their return, constitute a danger to the Dutch administration of the islands (Snouck Hurgronje, 1931, pp. 290–2). Obviously it is not possible in a short introductory volume to discuss the political ramifications of the work of all Ottomanists of the recent past. But the examples mentioned here and there should at least make us sensitive to the issue.

MODES OF PRODUCTION AND WORLD SYSTEMS THEORY

Paradigms of present-day scholarship are even more difficult to fairly describe than those of the past; as the owl of Minerva flies in the dark,

the main lines of a paradigm usually become visible only when it is already on the way out. However we will at least briefly refer to the role of Ottoman studies in the discussion concerning the 'Asian Mode of Production' (AMP). This Marxian concept, which in the 1960s regained relevance particularly among French anthropologists, also found adherents among Turkish economists and social scientists (Divitçioğlu, 1967). At the same time, another group of political intellectuals explored the possibilities of the concept of 'feudalism'. Certain Ottoman economic historians, some of whom had also used the AMP as a framework for their research, equally involved themselves with the 'world systems' paradigm put forward by Immanuel Wallerstein and his school (İslamoğlu-Inan, 1987).

The debate concerning the relative merits of the 'Asian Mode of Production' and 'feudalism' has been marred by numerous complications and misunderstandings. Extraneous considerations of left-wing political strategy tended to get mixed up in the debate to the great disadvantage of its intellectual content. Apart from that, the fate of AMP in international discussion, outside the Ottoman context, has also caused difficulties for Ottomanists. The concept had originally gained favour as a means of counteracting a notion of history which assumed that all societies necessarily would have to pass through the same sequence of 'stages'. This idea of historical stages had been widespread in nineteenth and early twentieth-century social theory, both within Marxism and outside of it. By assuming that certain societies were neither 'primitive', 'feudal' nor 'capitalist', social theory of a Marxian bent regained a degree of flexibility which had been lost during the Stalinist period. However after about 1980, the AMP was sharply attacked by many scholars as eurocentric, and as a result, most Turkish adherents of AMP gave up or at least strongly modified their views.⁵

As to the 'feudalism' side of the debate, many participants have been unaware that two different meanings can be attached to this particular term. One of them is inapplicable to Ottoman history because it involves things such as vassalage and feudal homage, which never existed in the Ottoman world. But the other usage simply refers to a society of peasants managing their own farms, and thus having direct access to their means of livelihood. Such peasants, who can directly reproduce themselves with the products of their own labour, can be made to hand over part of their produce only by non-economic coercion (Berktaş, 1985). This broad use of the term 'feudal' is applicable to a great many human societies of the pre-industrial age.

⁵ Compare Abou-El-Haj's criticism of Anderson: Abou-El-Haj (1991), pp. 105–6; Anderson (1979), pp. 361–94. Keyder (1987) contains an interesting development on continuity between the Byzantine, Ottoman and early Republican periods which appears to result from the author's previous espousal of AMP.

Debate therefore has centred upon the question whether peasants yielding part of their produce to a central state in the shape of taxes, and not as rent to a feudal lord, can properly be included in the 'feudal' category. For the specialist Ottomanist historian, the main advantage of this debate is probably the stimulus it has given to comparative history.

While the 'feudalism debate' has during the last few years lost many of its political implications, this does not apply to the discussion connected with the concept of a world system, associated primarily with the name of Immanuel Wallerstein (Wallerstein, 1974, 1980, 1989). The manner in which the Ottoman Empire was 'incorporated' into a world system, dominated by a capitalist Europe and later by the United States, continues to be of political relevance. As a result, this linkage of Ottoman history with the debate on world systems theory as an explanatory model continues to make many historians uncomfortable. But from a professional point of view, the links of certain Ottomanist historians to the Wallersteinian school also have had very positive consequences. While Ottoman history in the past few decades was often an arcane endeavour of little interest to anybody but its practitioners, the debate on world systems theory has made it much easier for Ottomanists to enter a broader historical discussion. And that should be an appreciable advantage to students now entering the field.⁶

DESIGNING RESEARCH IN OTTOMAN HISTORY: SOME PRELIMINARY REFLECTIONS

As has become apparent from the preceding paragraphs, working as an historian implies questioning our own motives. This is easier said than done, for usually we need a certain distance before motivations, our own or those of our predecessors, will become visible to us. Political changes in the world we inhabit are often a precondition for changes of scholarly orientation; but these changes constitute merely a necessary and not a sufficient condition. Only the anticolonial struggles of the post-World War II period have made it possible for us to question the views of many European and American practitioners of orientalism in the nineteenth century and early twentieth. It has taken quite some time and intellectual effort to acknowledge the simple fact that many of these people were either colonial administrators themselves or else trained such officials, and that these political concerns had an impact on their scholarship (Said, 1978). Obviously the generation presently active in Ottoman history has its own political agendas, and will in its turn be questioned by its successors, in all likelihood with

⁶ Among the historians with a strong interest in the Wallersteinian paradigm, one might name Murat Çizakça, Huricihan Islamoğlu-Inan, Reşat Kasaba, Çağlar Keyder and Donald Quataert.

unflattering results. But an even worse thing is also ‘on the cards’ – our work may be considered too insignificant to be worth demolishing . . .

Reflection on our activities as historians will start where the student planning a paper or thesis normally begins. During the first stage of his/ her work, the prospective author may have only a very vague notion of the topic to be covered. Certain people will be fascinated with a given primary source and decide to make it the basis of their study. Thus the correspondence of a vizier or sultan’s mother, or else the diary of a dervish, may become the basis for a combined historical and philological examination. The text is edited, translated, or if that is not feasible, summarised and annotated. At a later stage, this explicated text will serve as a means of understanding processes of communication within the Ottoman Palace, the prerogatives of royal women, or the process of education within a dervish convent. This type of study is perfectly legitimate; however it involves a good deal of philology, and many ambitious researchers prefer to look for something ‘more theoretical’. This will generally involve a *problématique* derived from current research as reflected in the secondary literature, both Ottomanist and non-Ottomanist. A student may recognise that Ottoman notables of the eighteenth century, even though they wished to govern the localities they controlled with as little outside interference as possible, for the most part did not attempt to set up independent states. Starting from this observation, he/she may launch an inquiry into the mechanisms, such as tax-farming, which integrated provincial notables into the fabric of the Ottoman state (Salzmann, 1993). Moreover a student may first familiarise him/herself with the problem-oriented historiography practised by the more respected representatives of the profession today. But as a second step, he/she may decide to return to the interpretation of a given primary source with questions derived from sophisticated *problématiques*. Cemal Kafadar’s work on Ottoman diaries and letters springs to mind in this context (Kafadar, 1989, 1992).

TIME, SPACE AND TOPICS

As an introductory volume, the present text makes no claim to comprehensiveness. Even less do I aim to recreate the all-knowing impartiality and invisibility so typical of narrators in certain nineteenth-century novels. My own competences, and even more, blind spots, have had a visible impact upon the composition of this book. Many more examples come from the sixteenth, seventeenth and early eighteenth centuries than from the periods before and afterward. For in my own work I have concentrated upon this particular period, which we will often call ‘early modern’. Where the nineteenth century is concerned, more is said about the