

# OSMANLI ARAŐTIRMALARI VI

Neşir Heyeti — Editorial Board

HALİL İNALCIK — NEJAT GÖYÜNÇ

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# THE JOURNAL OF OTTOMAN STUDIES VI

İstanbul - 1986

Review Article :  
METİN KUNT, THE SULTAN'S SERVANTS: TRANSFORMATION  
OF OTTOMAN PROVINCIAL GOVERNMENT  
1550 - 1650\*

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GENERAL

*Method and Sources*

Kunt introduces this book as a continuation of his interest in the study of the Ottoman households and their importance in the Ottoman polity. Rightly he points out that the role of these households (other than those of the palace) has been thus far neglected. Personal relationships, he asserts, played a critical role in all aspects of social, economic and political life. According to Kunt his research was designed and initiated in order to determine the structure and functioning of Ottoman households and to ascertain the validity of his speculations. But complexity of the topic forced limitations on the author and compelled him to study the men of the sword. He was also forced to limit his scope to one century: 1550-1650. 1600 is the year taken by Ottoman historians, according to Kunt, which signals the change in Ottoman life as evidenced by «cataclysmic events».

THE APPROACH

Without regard to the criticisms which have been levelled by İslamoğlu, Keyder<sup>1</sup>, and Roger Owen, the author adopts Inalcık's

\* New York, Columbia University Press. 1983.

1 Huri İslamoğlu and Chaglar Keyder, «Agenda for Ottoman History». *REVIEW*. I, I (1977).

ideal categories. He makes these the guide for selecting topics and emphases, and these in turn determine his framework of analysis. Thus we are told that Ottoman classical institutions were created under Fatih Mehmed and reached their maturity in mid-sixteenth century, when the empire was involved in a two front war against Iran in the east and Austria in the west. Local unrest on the part of the «college» students (*sukhta*) and massive peasant-led rebellions are also reported. These rebellions were marked by the flight of peasants from the land, called *büyük kachgun*. The revolts were crushed only by a total mobilization of all «state» forces.

Not only does Kunt derive his approach from the ideal categories of Ottoman secondary literature, he actually adopts this approach to historical evidence virtually without alteration. In the process, Kunt, like his secondary sources, is burdened by an important historiographical issue: how to assess and address change.

The cue for their point of departure comes from Ottoman political commentators of the seventeenth century such as Ayn-i Ali, and the anonymous author of *Kitab-i Mustatab*, Kochu Bey, Katib Chelebi, and reformers like Murad IV and Köprülü Mehmed. For these men, salvation of the empire could only be achieved by returning to the institutions and practices of the classical period. Kunt and his predecessors read these Ottoman political commentators, without reconstructing their context, and thus perpetuate their mystification in a series of historical fallacies which suggest these were historical and accurate analyses by detached or uncommitted observers.

Rather than factional political positions articulated at specific junctures in Ottoman history, my alternative to the reading of the same sources aims to determine the kind of «objective» evidence they represent. This becomes feasible once we begin to analyze the significance of the time of «publication» for each example, and when we place each of these authors within his socio-economic and political context. For example, Kochu Bey's *Risale*, a partisan tract that subscribed to the by then radical, though conservative, policy of returning the Ottoman dynasty to the fold of the dominant element in the ruling elite, is a product of social, economic and political change in Ottoman society over a definable period of time. The circumstances

for its writing and the occasions (the accession of sultans Murad and Ibrahim) at which the various «editions» of the *Risale* were pressed into the political debate, help us define the nature of the specific program of reform advocated by Kochu Bey. From both the external circumstances and its internal content and emphases, we can show that this program was advocated and composed by spokesmen for the politically «disenfranchised» elements of the Ottoman ruling class. This is not unique to Kochu Bey, but also the case for one of his predecessors, Mustafa Âli, especially in his *Counsel for Princes*<sup>2</sup>.

Kunt's misinterpretation is not unique in the field. A fairly recent entry by Imber in *E.I.*<sup>3</sup> on Kochu Bey<sup>3</sup> and his *Risale* completely disregards the specific conditions for the production of the two versions of the *Risale*, even though in an earlier version, *IA*, Uluchay accords specific importance to the occasion for their writing, and to the sultan to whom each was dedicated<sup>4</sup>. The Turkish version of the encyclopedia entry indicates that the prescriptions contained in the 1631 version were tried by sultan Murad IV. It should be noted also that the *Risales* were written by a palace functionary, one close to the dynasty, the «mahrem-i esrar», or confidant to Sultan Ibrahim and before him to Murad, at a period in Ottoman history when sultans had come to play a less and less important a role in the actual running of public affairs. Sultan Murad was an active prince who, under prodding, tried to recapture what he thought was his rightful political role to both reign and rule. In this Murad IV actually brings to mind the career of Mustafa II (1695-1703), trained by his mentor (*hoca*) Feyzullah Efendi whom he then invited in 1695 to head the *ibmiye* and play the single most important role in the reign of that sultan.

Nevertheless, the *Encyclopedia of Islam*<sup>2</sup> entry neither challenges Uluchay's point of view nor does it reproduce the latter's argument, except to say that the later version of the *Risale* was written in simple language for Ibrahim who was young, stupid and needed to be educated. By placing his emphasis on the contents of the *Risales*, Imber implies that it is an actual description of the condi-

2 Andreas Tietze, *Mustafa Ali's Counsel for Princes*. Vienna, 1979-82.

3 *Encyclopedia of Islam*<sup>2</sup>. Leiden, 1954-.

4 *Islam Ansiklopedisi*. Istanbul. 1940-.

tions in the Ottoman empire in the first half of the seventeenth century instead of advocacy by a partisan of the dynasty for the return of the sultan to the center of the political stage. It would also follow that since the «golden age» to which he harks back is not one he had himself experienced, we cannot accept as accurate his picture of earlier practices and conditions.

Imber's emphasis on internal content contrasts with the article in *IA* where the author places equal emphasis on content and the context of production.

Following the former tradition, Kunt takes these «documents» to be simple, innocent and objective assessments of Ottoman conditions by Ottoman contemporary commentators. This interpretation ignores the Ottoman authors' advocacy for return to the classical institutions of earlier eras and disguises changes already effected. Kunt's approach amounts to advocacy for a partisan position, in what was a struggle between contending factions of the Ottoman ruling elite. Consistent with his view that Ottoman contemporaries were objectively reflecting their times, Kunt argues that only in the eighteenth century did commentators begin to see that there had to be other models and premises for change thus missing the «modes» or «mechanisms» for change and adaptations which were evolved during the so-called period of decline. It is one of my theses argued elsewhere that such mechanisms evolved spontaneously over nearly two centuries and that they served as the base for future change, including the *tanzimat* in the nineteenth century and beyond<sup>5</sup>.

#### RESEARCH OBJECTIVE DEFINED :

Kunt's task and our own is the single most studied and debated topic in the field of Ottoman history: the nature of the transformation of the Ottoman empire in the critical years 1550-1650. Kunt defines it this way: «My own purpose here is to isolate one single strand within the complex of changes, namely the role of provincial

5 R. A. Abou-El-Haj, «The Nature of the Ottoman State 1560's-1700's» in I. Bierman, R. A. Abou-El-Haj and Donald Priziosi (Editors), *Power and Urban Structure in Ottoman Society*. (Forthcoming).

governors, and study it against the background of the general transformation in the period 1550-1650». (xv) The social structure which they shared, and that served to perpetuate their power and influence were the households. But for Kunt the study of the household phenomenon is much too broad a subject. He proposes instead to narrow his subject to the governors who held office in this period, considered by him to be the most important stratum of the men of the sword or *ahlu al-sayf*. His method for setting forth this view is through prosopography or group biography. Kunt further narrows his study by limiting himself to an analysis of the «objective conditions of service the governors experienced». By making ever narrower reductions and refinements in his task of research, Kunt hopes: «to study a single element of change in terms of its interaction with other aspects of the general transformation, that is, in terms of how this single element might have been influenced by and how it might have contributed to the general change, it follows that the better defined the single element to be studied the more specific and meaningful the analysis can be». (xvi)

#### *Sources :*

Although prosopography, or group biography, is the author's proclaimed choice for analysis, he depend most heavily on the appointments books. Kunt explains his narrow base for biographical evidence, by disclaiming the usefulness of biographical dictionaries such as the *Sijil-i Osmani* and the chronicles because the first source includes only prominent individuals, and the chronicles include numerous Mehmed Beys and Ahmed Beys with little further specification of their careers or biographies. Any researcher who uses the Ottoman biographical dictionaries will have to agree that their biographical information is much too general to be reliable, especially the *Sijil*, and particularly its entries on less prominent individuals.

However, by widening the documentary evidence, one can partly compensate for these limitations as I found in my own experiment in reconstructing official biographies of individuals who were offered positions of service by the Ottoman court in 1660-1700's with data drawn from a similar set of sources as those available for Kunt's

period. (The approximate number of officials whose biographies were reconstructed is about two thousand, exclusive of the *ilmiye*. The biographical data is reproduced on approximately 20,000 separate entries culled and noted from the above-cited primary sources).

The archival *Mühimme defters* and even the financial, *Maliye defters* bear direct reference to the abstracted individual Mehmed Beys and Ahmed Beys. Along with the chronicles these specify some of the political, administrative and financial activities of those appointed to «office». What this information makes possible is the accumulation of data on each appointment, the actual «service» and activities of each holder at his assigned sanjak or district. A composite picture emerges for the assignments most of them received, and as far as those in Istanbul were concerned, an assessment can be made on how the appointed officials dispatched their responsibilities and the nature of their assignment (e.g. campaign activities, local police actions) dismissal from office and the reasons; and their replacement. Information on the changes in their later official biographies can be obtained: whether their properties were confiscated; their lives forfeited. From a composite biography based on these various sources I was almost invariably able to offer at least one contemporary opinion on why assignments were made or unmade and at what specific, sometimes approximate, dates. (Given this method, there will always be fragments of official biographies. Nevertheless, a sample of relatively complete biographies together with sufficient data on the biographies of holders of office would offer us a good chance to conduct a prosopographic exercise adequate to test the accuracy and biases of such biographical dictionaries like the *Sijil*).

Kunt does not admit even the potential for this kind of reconstruction of official biographies. He does not seem to be interested in following the functions of a particular office by studying the day-to-day official transactions available in these sources. Nor does he seem to be concerned with the biographical or personal details available on these individuals, which are the very stuff out of which political history is reconstructed. In other words Kunt chose a shortcut by selecting the appointments registers and did not utilize the next stage of available biographical information, whether in the form of the sources already noted, or the *sijillat-i shar'iyye*, of the

provinces and districts (these latter, are just as significant as the *Mühimme* and *Maliye* registers, in defining the tasks which the provincial «administrators» actually performed or did not).

Thus Kunt has had to face the inherent advantages and disadvantages of these registers for a prosopographic study. Their main and only apparent advantage is the specification of the date of appointment and dismissal. These books are restricted by the nature of the tasks they were meant to perform. Being abstracts, they do not explain the reasons for appointments and dismissals, so we view a mechanical process, without the historical vicissitudes in each entry. The conflicts and jockeying for place which are at the heart of all «official» appointments and prosopographic studies are left out. The reader is treated to a picture of Ottoman society which is simply a series of mechanical and undynamic processes.

Accepting the given paradigm of ideal institutions and practices and confining evidence to the narrowest type, two limitations imposed by the author on himself, result in a further methodological consequence. Even if we were for a moment, hypothetically, to accept the plausibility of the conclusions drawn by Kunt from his narrow approach and treatment, which we have enough independent evidence to suspect, we miss the richness and complexity of Ottoman society in this period.

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Given the unsophisticated methodology of research programs in the field of Ottoman studies, it can be argued that the practitioners would be better served if, for the moment, we accept faulty conclusions as long as they are based on a complexity of approach to internal change in Ottoman society in the critical century of Kunt's concerns. For the advancement of research in this field, the process of implementing a complex research program is just as important as arriving at the «correct» conclusions. By according complexity to Ottoman society, its internal history would not be subjected to final and irrefutable solutions, such as those Kunt illustrates in his monograph, but would become a proper and serious subject of historical discourse. At a minimum this alternative will rid us of con-



ducting the discussion of Ottoman history on a level of essentialist discourse.

Confinement to skeletal and restricted evidence like the appointment books and direct reading of evidence, as in Kunt's monograph repeats a discredited pattern of interpretation that typifies the defunct Orientalist paradigm. It misleads the researcher into believing that he has arrived at solid and irrefutable final conclusions. In this it poses the same dilemmas as reading a document directly without considering the complexity, complications and uncertainties of the moment it was produced; its context or the author's point of view or motives. The Orientalist view, it may be recalled, adopts the method of reading the sources directly, and therefore attributes to them an accurate and unprejudiced picture of contemporary society.

I am bound to ask why Kunt uses this narrow base for evidence, a minimal, mechanical and inert, type of data, which gives the impression that his discovery of sources came first and his method and approach second. It is bound to exaggerate the significance of the material with the consequent historical distortions. For example, I would contradict the main thesis of his study, with his own data, which suggests diminution in the power and position of the centrally-operated provincial administration. If this is the case, then it is legitimate to conclude that contrary to the author's main thesis, which accords great importance to the provincial governors, his study demonstrates an «institutional» structure that had become, at best, redundant.

In the following sections of this review article each chapter of Kunt's monograph will be treated separately with specific observations and comments.

#### Chapter 1 *The Emergence of the Ottoman State*

This is a matter-of-fact description of early Ottoman history and state development. Its treatment is so unidimensional that we are not made aware of reasons for change. For example there is no sense of struggle, of social groups vying for power, positions and material rewards. This part of the book is mainly a philological

exercise. Understanding Ottoman history is based on a dictionary definition of administrative and institutional practices rather than what these represented specifically in each historical era.

In so doing Kunt follows a prevalent, Western tradition in the field. Nearly every recent Ottoman monograph seems to find it necessary to start with what is essentially a useless task of narrating the story of origins by attributing the process to Islam. To my mind this is partly due to an ambivalence or confusion over what audience is best served by these monographs. The contrast with other historians is instructive. Scholarly works in European history address first specialist in the field and then other historians. At no point do they find it necessary in each one of their monographic works to start in the medieval and early modern practice with either the «creation» or the beginning of the Christian era. Whether Christian or Islamic, these early histories, usually had political and ideological components, explicit in their declarations of the legitimate claims of their «subject», especially when this was the dynastic view of legitimate claims to power.

One argument for concentrating research efforts on origins and derivations is based on the continuity of institutions and practices, but how historically relevant or informing is an attribution that points to nearly a thousand years of continuity, as is the case in Kunt's treatment? Conclusions derived from such an approach will be historically meaningless, a «pious» exercise that leaves unaccounted the variations in the specificity of the continued practice at each chronological point of its occurrence. Changes in utility and purposes of the continuities can hardly be specified in a meaningful way.

For the comparable period, research programs by European historians block one specific and limited period for the study of change and continuity; in the Near Eastern field research harks back to the institutions, practices and concepts of the first century of the Hijrah, the prophet Muhammad and earliest Islam. Unchanged, these are treated as if they had immediate relevance to the period under study. In Kunt's monograph the leap represents one thousand years. The simple existence of a particular practice does not in and of itself signify continuity of the same utility and purpose.

This treatment and approach has been a puzzle that requires explanation. If, as it is claimed, this approach is meant to facilitate a non-specialist audience, it should put them off, since at best it consists in the definition terms treated nearly always out of context, which should provide the connotations and denotations of practices, institutions and terms. The reader is treated to a series of inert dictionary definitions. Nor does the author take the comparative approach to the study of these historically evolved practices and institutions.

As with the production all knowledge, we need to determine who would encourage, subsidize and find this type of writing useful.

## Chapter 2. *Provincial Administration*

The contents of this chapter are based on a formal structure which Kunt takes as given and then marshalls evidence to prove valid. He subscribes to the static picture of his secondary sources, e.g. among others Inalcık. Here as elsewhere in the book, Kunt seems to provide a case for these «authorities» to legitimate his work as he, in his turn, legitimates them, but selectively. Even though Heath Lowry's work especially on the cadastral surveys, has been available, Kunt makes no reference to it. There is no discussion of why, what, how and if the structure changed.

Without explanation he reports, depending mainly for his information on Barkan's *Kanunnameler* treatment of Erzurum (n. 10): «In some cases the *kanunname* stated that the new subjects specifically asked that Ottoman rules and regulations be applied to them rather than maintain earlier usages». He enumerates but does not describe the *kanunnameler* or analyze them, though these were meant for the provinces.

In this chapter as elsewhere Kunt seems to be puzzled by the ad hoc nature of «Ottoman fiscal practice». But one has to ask, whether we can really determine income, and how significant would that determination be? As Kunt acknowledges: «... it cannot be done, for it is practically impossible to determine the component parts of the standard yield in a sufficient number of cases to allow a meaningful

statistical analysis. We can tell from the examples cited above, that the *sancak hasili*, the core portion of the revenues granted to all incumbents at a certain *sancak*, did exist, but its composition is not given»<sup>18</sup>.

Again on page 19, Kunt says: «As the standard yield of Ottoman *sancaks* cannot be determined, we have to base our observations on the nature of the *sancakbeyi's has* on evidence more readily available, that is on the *has* entries in summary registers (*icmal defters*)».

Kunt begins the chapter with the definition of «*dirlik*,» (livelihood), and follows with a discussion of provincial administration, but he does not treat the office itself as revenue, although he discusses points in that direction. Here is one of the issues in this work that cry out for a comparative history which offers a whole discourse on the question of office as income. Had the question been raised, the discussion would have taken place on a specifically historical level rather than the technical one which this chapter seems to represent. Even though he is using archival sources to illustrate these technical points, Kunt does not guide his readers to what is new in his treatment, leaving even specialist readers at a loss as to what it is that is novel, different or even significant about this analysis.

Without specification and chronology, we are left with the idea that the the fundamental method of tax extraction fits all periods without much change. Yet, if we take seriously Âli's observations about the changes in the Ottoman «feudal» system in the second half of the sixteenth century, we note that by 1560's the beneficiaries of the system had changed so considerably that the whole *sipahi-timar* system was in jeopardy and collapsed in the first half of the seventeenth century or by the time Kochu had written his *Risale*. Since Kunt had blocked out the years 1550's-1650's as the chronological limit for his study, and these hundred years approximately fit into the period of these two *nasihatname* writers, we can say with a certain amount of assurance that the ideal picture which he draws of the land tenure system does not fit the specific period of his study.

Kunt's puzzlement about the ad hoc nature of contemporary Ottoman fiscal practice results from his formal view of the revenue

extraction system from land, which is manifest in the basic unit of *timar* (fief), and its several varieties, e.g. *ziamets*, etc. (It would be worthwhile here to compare Ottoman fiscal practice in this period with contemporary practice in either France or England; methods of bookkeeping and accounting, and their accuracy. In other words, do the given figures in these comparable periods always add up correctly or are there discrepancies as one finds in the Ottoman records even for the fifteenth and sixteenth centuries). I have argued elsewhere that the beneficiaries of this system were mainly a ruling class who viewed this income as a natural right of their class position. If this is so, the so-called (official) financial or *Maliye* books would be the same type one would find in a private bookkeeping operation, where the exactitude of accounting is neither a desirable end nor an issue, as long as approximation has been entered or anticipated. (This could explain the nature of entries in the *tahrir* registers discussed in Heath Lowry's forthcoming paper, «Whither *Defterology*.») In other words figures which do not add do not mean researchers have to give up. What is needed is a change in approach to the evidence in order to properly interpret it.

### Chapter 3. *The Umera Status*

There is no analysis here. The data is described in the main, with Kunt accepting as given the picture drawn by others and illustrating its presumed validity in part by his own research data. At one point this deteriorates into cliches that are made to give meaning to the author's data. For example, Kunt uses the Islamic law of inheritance and the text of the provisions of the law in a skeletal outline: daughters inherit half of what brothers do, mothers inherit one eighth, etc. to illustrate the distribution of wealth of the «*umera*».

Consistent with the author's initial assumptions, office is again treated formally and assumed to be part and parcel of a state structure. The hypothesis that office was treated as an investment or as an income by the ruling class or merchants is neither raised nor tested.

When it comes to the discussion of the perpetuation of wealth in particular families, Kunt asserts that (?original) wealth may have

been passed on to the third generation only. But may we also ask: what happened to the overall socio-economic position of these «families» beyond the third generation? Did it sink socially or economically? Or did it continue to hold its latest status and managed through endowments to have members move into other «careers», or socio-economic endeavors, such as the *ilmiye* or profession of the «learned», commerce, or trade? What is the significance of the phenomenon which Kunt tries to point out, namely that wealth was passed on to the third generation?

Throughout his discussion, Kunt leaves out the crucial question of the policy of confiscations or *musadara*. The method was presumed to be critical in reducing official and private wealth, yet there is no reference to it in either his text or in his index.

#### Chapter 4. *The Structure of the Military-Administrative Career*

When Kunt is faced with variations from the ideal picture, he cannot understand or explain their significance, and thus reduces his own observations to a «non-system»:

«The changes in the *umera* career lines that we have been noting are reflected in chart 4.1 which should be compared with chart 3.1. The striking thing indicates that the measured flexibility of the system in the mid-sixteenth century had turned into a non-system with regular lines because almost anybody could move to almost any part of the structure. But within this general confusion some important new elements should be underlined...» (67), for one the central administration officers was seen to be taking more and more of the provincial governorships: «As the central administration officers came to take over the *umera* ranks, it became increasingly difficult for lesser provincial officers to be promoted: only a quarter of the *sancakbeyis* were from their ranks and only a quarter of the *beyberbeyi* were chosen from among the *sancakbeyi* in the seventeenth century. What these shifts mean in terms of the general transformation of the empire we will be in a better position to understand after we review other aspects of the *umera* career»<sup>67</sup>.

«While the regionalism of the *sancakbeyi* appointments remained stable from the sixteenth to the seventeenth centuries, the dura-

tion of office was increasingly shorter (table 4.9)». (70) Rise in provincialism? Then Kunt picks up another point: «this development was a subject of complaint in the seventeenth century; indeed, some Ottoman political Commentators cited shorter terms of office as a basic ill of the system». This cites Kochu Bey. It illustrates, however, the point made in the preliminary remarks of this essay that Kunt reads and uses this primary source directly without alerting the reader to Kochu Bey's class and point of view, critical for our understanding since he was on the periphery at that point and saw change as a continuing threat to the old order from which he hoped to benefit. He therefore saw change as threatening and equated it with decline when in fact the decline he was describing was in most instances confined to his fortune and those of his class.

Kunt seeks a rationale for what were political decisions: e.g. «Incidentally, the fact that in all three periods length of office was not so much in annual intervals but rather that the *sancakbeyi* could be dismissed at any time of year indicates that appointments were not effected in any particular season», (71) and misses the significance of his evidence: «... the data for the two later periods, there are many omissions, making it sometimes impossible to identify all the offices the *sancakbeyi* held unless they were in immediate succession. More longer intervals were unidentified and thus the change from around 1570 on was even greater than table 4.8 indicates.» (72): Shouldn't this indicate that the provincial administration was becoming less significant, less relevant?

Or he makes ad hoc interpretations unwarranted by his material. Applying modern concept of income, i.e. salary from office, he says: «Staying a shorter term at a given post and having to spend months, even years, before another appointment, obviously hurt the *sancakbeyi* financially», an assumption by Kunt not supported by the data. There are no references up to this point to retirement or *teka'ud*, or *ber vech-i arpalik teka'ud*, which came from *sancak* incomes. The phenomenon is brought up later, however.

The author also seems to argue against his own interpretations. While on the one hand he suggests that regionalism developed, using the increasing number of officers reappointed, he also says: «it seems quite definite that a basic principle of *sancakbeyi* appointments

was not to allow an officer to remain at one post for any length of time: even around 1570, 70 % of the *sancakbeyis* were dismissed within three years». (p. 72) What interpretation can be offered for this seemingly contradictory phenomenon Doesn't this suggest again that the whole provincial administrative structure was considered increasingly irrelevant and was consequently diminishing in importance?

Kunt describes the phenomenon that there were more candidates than offices, as « ... a congestion in the *umera* career occurred». (p 76) A highway image, with overcrowding, but no explanation.

Finally in the last paragraph, the author refers to the end of the *devshirme*, but we find recorded as late as 1703 that Ahmed III threatened to revert to the *devshirme*, as he was about to dissolve the *bostancı* corps.

#### Chapter 5 *Transformation of Provincial Administration*

Kunt takes a monetarist view, essentially external when he describes inflation in the Ottoman markets flooded with silver from the Spanish empire in the Americas. (p. 77) But what were the internal Ottoman conditions for the inflation? In this instance, Kunt subscribes to the school that views change as a phenomenon motivated by external factors. This tendency avoids a serious consideration of internal Ottoman history and the arduous task of reconstructing specific historical periods.

Here Kunt refers to Akhisari, but does not identify his social position and so his observations are read as though they represented an accurate and neutral assessment of Ottoman conditions (p. 79).

At one point the author describes a shift in status of *timars* and their decline in number. This in itself would indicate a deminution in the importance of the military dimension of the provincial administration (p. 80).

At another point in the chapter Kunt indicates that the *Kurush* replaced the *akche* in *Maliye* register usage. This is not so, for in the *Maliye* registers of late seventeenth and early eighteenth cen-



turies the *akche* was still one of the primary methods of financial reckoning. That does not mean that the *kurush* was not used, only that the *akche* was not discarded. (p. 81)

Kunt comes close to favoring a class analysis of the phenomenon of state when he asserts: «In other words, what is good for the *umera* is good for the state. This was not Hasan Pasha's personal view but a basic element in Ottoman political thinking: we have seen that the official income assigned to the *umera* was intended to support their retinues». (pp. 84-85) A more cogent interpretation would see it as an indication of the attitude of a ruling class; it represents the good of the «state», and what serves the state interest also serves his class. For this class there is no distinction between the two.

Kunt maintains (p. 87) that the late registers do not give *has* amounts, etc., but the *Maliye defters* are full of these and the *Mühimme registers* also have them. He seems reluctant to work out the day to day official transactions necessary to determine continuities and changes in procedure, practice and regulations.

On page 88 Kunt suggests that the was not overly concerned whether the provincial cavalry participated in campaigns, yet the issue is writ large in the *Mühimme defteri* for the late seventeenth century which complain that *sipahis* and *timar* holders did not show up. Some were removed for not responding to the call to join the campaigns.

The author finally turns to what in «Ottoman historiography (is referred to) as the Abaza Hasan Pasha revolt (1658). It took all the resources of the state to suppress this revolt and re-establish a stable relation with the governors thereafter. Nevertheless, the changes that had occurred since the sixteenth century were irreversible». (p. 93) But where is the analysis?:

1. Why did the *beylerbeyis* revolt?  
Were they being treated as redundant?
2. What was the number of *beylerbeyis* in this revolt?
3. Doesn't the success of the state in suppression of the revolt indicate that a military alternative had already been devised or was in the making.

## RECAPITULATION AND CONCLUSION :

A. «Toward the end of the century, however, as there emerged a surplus of candidates for *sancakbey* posts, officers from the center were relatively more successful, even when they arrived without previous experience». Again, there is description without analysis. How do we explain the down-grading of previously significant posts? This downgrading may reflect a reduction in the status and importance of the palace household whose graduates were kicked out to the provinces. By the end of the seventeenth century and early eighteenth, a provincial appointment was not considered such a great honor and was in fact turned down by palace graduates, e.g. Silihdar Mehmed Ağa, the chronicler!

B. «The imperial household came to greater importance as its graduates left to occupy the highest positions in central and provincial administration». (p. 97) This is a curious kind of conclusion, since in fact all along Kunt seems to show that the provincial administration was being downgraded even in his early period. The imperial household could not be rising in influence and declining in importance at the same time. Evidence for most of the second half of the seventeenth century, shows that the central administration positions were given less and less frequently to imperial household graduates (even under Fazıl Ahmed Pasha!). By the end of the century more than half went to others than those graduated from the imperial household, indicating a consistent pattern of decline in the dynasty's and imperial household's political influence and importance.

C. «Unfil recently it has been customary to speak in Ottoman historiography of the 'decline' of the empire from the late sixteenth century ... Perhaps the magnitude of the crisis around 1600 and the efforts of contemporary historians to understand its nature have led to the fallacious notion of a long, monotonous, and continuous decline. What is needed, instead, is a closer look at changing patterns in more specific time periods». (p. 98)

Again, these contemporaries speak from a partisan position, conditioned by their loss in the competition to be the chief beneficiaries of the system so they were not describing an objective reality, so much as one relative to their perception of their self-interest.

D. Kunt (p. 98) is not able to devise his own terms or words to describe these phenomena, further indicated by his treatment of the shift from *dirlik* to *beylerbeyi* where the latter supervised revenue collection, some of it kicked back to the treasury. This he regards not as decline, but actually as modernization, «in the sense that it was a shift from a «feudal» arrangement to a monetary one». Thus he concludes: «Furthermore, the shift was intended to increase the power of the central government, another feature of the 'modern' state.» But what are the features of the «modern» state? The term «modernization,» in this context, is taken to mean the modern state, rather than the paradigm «modernization».

E. «However, the new *beylerbeyis*, secure in their provinces, did sometimes challenge the central authority. Event more important, the *beylerbeyi* came to depend increasingly on the cooperation of local --and unofficial-- elites, either as agents (*musellim*) or as aides in revenue collection, thereby contributing to the rise of a powerful group of provincial notables (*ayan*) in the eighteenth century.» (p. 98)

But there are always local elites in place, otherwise the provincial administration would have been impossible to carry out. Because the system worked on the basis of alliances with local elites, who had yet-to-be-defined functions. This is a partial explanation for continuing the *kanuns* (regulations) of the Ottoman domains before they were incorporated into the empire, e.g. the *liva kanunnameler* for the sixteenth century. We are not dealing with a modern state with a fully rationalized administration and bureaucracy.

F. Kunt concludes this part with :

«A quite different feature of the process we have been studying had much more significant consequences for the prosperity of Ottoman society in general. With the rise of households, both as a requirement of the state and as a political necessity in competition for office, a large part of the economic resources of the empire was tied to political struggles. Capital accumulation was largely intended for political aims, rather than for economic investments. This, more than anything else, determined the future course of Ottoman rivalry with Europe at the threshold of the industrial revolution and led to a decline of Ottoman fortunes relative to the increasing might of an

industrialized and imperialistic Europe in the nineteenth century.»  
(p. 99)

But, one has to distinguish investment and who invests. Is it economic theory to assume automatically that only state officials invest? In the seventeenth and eighteenth centuries there seem to have been a concentration of holdings in the hands of merchants. Here we see the most explicit view of office as income, offices sold, bought, or even bequeathed like a commodity. If only officials were investors, we would distinguish a «class» when distinction of class and economic function, although insisted upon by Ottoman observers, had become obsolete.

#### SUMMARY AND AFTERTHOUGHTS :

##### I. *Overall* :

1. No sense is given of human beings acting in any capacity, as groups or even as individuals.
2. In the analysis of the historical, political data there is no sense of politics as conflict and therefore there is no sense of struggle.
3. There is no effort at any point to explain why change occurred.
4. The approach displays no dynamics.

##### II. *Specifically* :

1. In his approach Kunt starts with the wrong end, for instead of seeing his work as the pioneer one which could change not only our understanding of the one hundred years which he studied, but also to correct the existing approaches to this period, he takes the approaches as given, and in fact seems anxious to prove them to be right (keeps confirming Inalcık's «findings»). He does not seem to see any conflict between his findings and the existing systems of explanation of the same period. If indeed he had nothing new to add, then one wonders what his whole study represents.

2. The topic he studies, is so narrow, that it is hard to figure out what is being proved and what its significance is. Kunt actually argues in contradiction of the meaning of his evidence. He maintains that the evidence indicates something significant about the provincial administration and the evidence which he cites seems to indicate that the *timar* system and the provincial administrative system fell in importance. Yet he insists that his research is suggestive of the rise of the *ayan* in the eighteenth century! What took the old provincial administrative system's place, and how? In other words, it is not sufficient to think that the neglect of the old system was an accident.

Because the evidence is so narrow one can hardly generalize from it and it leads to the contradictory interpretations which the author commits, but does not see or realize. In some instances the evidence flagrantly contradicts his line of interpretation, but he does not see it as his task to undertake an explanation of these contradictions within his system of explanations, so that they do not vitiate his argument. The narrowness of the topic and the evidence lends credence to the contention of this critic that Kunt «discovered» the materials and decided to explore their significance, ending up forcing the evidence from these materials into the existing paradigms.

When one adds up the substantive part of the book, there are about forty printed pages. The rest is mainly introductory of the worst kind. Ultimately the reader has the feeling that the results from this great effort are so meagre, and Kunt's level of generalization is so low that it is hard to justify the publication of this study as a book.

When it comes to accounting for change, Kunt is unable to explain its dynamics. He is unable to accord priority to one over another cause since he has not set up a hierarchy of priorities with which to explain why and how change took place. In his study he remains fixated on the symptoms. Kunt does not assume nor is he able to demonstrate that there are underlying causes for change. It is only such a hierarchical organization of the causes of change that makes possible the testing of the coherence and the cogency of an historical argument.

Since in practice Kunt does not do this, he is saddled with fundamental problems. When he proceeds, he is unable to differentiate the symptoms from the underlying causes. Furthermore, once he has noted and isolated a trend, because he does not see the underlying cause, he is unable to judge its significance. Thus, for example, the central thesis of his book is that the staffing of the top levels of the provincial administration (i.e. the *umera*, *beylerbeyiler*) came from the graduates of the Imperial household, but he had not bothered to determine the underlying cause for this new trend in recruitment and assignments. However, he is not deterred from according meaning and significance to the phenomenon of change. He sees the turning to the Imperial household recruits to man the top provincial administrative posts as a trend which portends an added significance and a new importance to the Imperial household. But this conclusion is not warranted either by the body of evidence he cites for the period 1550-1650 nor by the one culled from the administrative appointment practices for the subsequent period 1670-1700. All that Kunt's evidence would warrant as a generalization is that the Imperial household graduates were more directly assigned to *umera* or *beylerbeyi* provincial posts. His analysis does not warrant his conclusion that the Imperial household has gained in importance, all it does is suggest simply that in this one hundred year period there is a more frequent resort to the Imperial household for appointments of *umera*/*beylerbeyi* rank.

The author's conclusion is especially not warranted as there is more convincing evidence in favour of the position that the Imperial household was actually losing in the struggle to maintain its political and economic initiative. Two interrelated examples can be cited to demonstrate this point. Although evidence for it exists from at least the late sixteenth century and further evidence better illustrates it from the seventeenth, there was a studied and purposeful tendency to discourage the rearing and cultivation of princes with the proper qualities for administration and leadership. There are exceptions but these nearly always prove the the rule rather than disprove it. Most often in these few exceptions the princes who turned out to be strong rulers were elevated because they were the only ones of age to be «elected». In one instance, the extreme measures which the

dominant elements in the ruling class in Ottoman society were willing to take to get their kind of princely «nominee» on the throne further illustrates this point. On that occasion, they even contemplated abandonment of the Ottoman dynastic line in favour of a prince from the Crimean royal house\*. (\* The Giray dynasty, as heir to Cengiz Kan, served as the royal back up in case the Ottoman line vanished.) Although in the contemporary literature (and later on this is picked up nearly intact and unaltered in modern scholarship) the explanation for the trend to isolate the princes is excused on the grounds that it would forestall the debilitating and unnecessary internecine and fratricidal conflicts among members of the dynasty in their bid for attaining the throne, the very phenomenon of turning them into virtual prisoners of the *kafes* was intended by the dominant elements of the ruling class to drastically diminish the ability of members of the dynasty to have a direct role in the conduct of public affairs.

The new uses for the *kafes* were paralleled by a related and equally significant trend, namely the virtual «appointment» and deposition of princes and sultans at the will of the dominant elements of the ruling class. This trend is especially well illustrated from evidence for the seventeenth century.

A collateral process which also contradicts Kunt's assertion of the growing significance and importance of the Imperial Household can be seen in scattered evidence of administrative and recruitment practices from the latter sixteenth century and from systematic and more telling evidence from the second half of the seventeenth on the background of recruits for high office at center. In the latter case, at least statistically, it can be shown that a significant trend had been established wherein a progressively growing number of recruits for high office came from households that were in competition with the Imperial one. Here, it could be argued that this trend was not confined only and specifically to the second half of the seventeenth century, but actually had its beginnings in the very period of Kunt's own period of study, namely the second half of the sixteenth century (as demonstrated by Âli, see especially in Tietze edition and translation). Even historical logic would confirm the idea suggested here, for it would be safe to argue that for this period at least trends do not

have the capacity of reversing themselves 180 degrees in less than a couple of decades and therefore signify their completely opposite meaning. When pursued one further step, the argument we are trying to develop indicates an opposite trend from the one which Kunt sees. Contrary to the author's conclusion which is predicated on the assumption that the Imperial household had gained in importance, we have demonstrated here and elsewhere that the Imperial household was declining in importance.

The validity of our argument has to be tested by its capacity to provide both a consistent and a comprehensive resolution to the apparent contradiction between Kunt's contention and our own. Contrary to Kunt's contention and consistent with other evidence from the sixteenth and seventeenth centuries, we can explain the more direct recruitment of Imperial household graduates into the provincial administration as a phenomenon that does not portend the growing importance of the Imperial household for this period. Rather, it is a symptom of the decline in the dynasty and palace's political and economic importance. Thus the trend in recruitment policy that Kunt noted in his research suggests that by their appointment to the provincial administration the Imperial household graduates were being deprived of their chances as others, namely recruits from grandee households, were taking the majority of the politically important and financially lucrative posts. This dimension of the interpretation is confirmed from two sets of evidence from the second half of the seventeenth century.

The first comes from a recent study which Kunt himself has conducted since the initial research for his book *Sultan's Servants* (the Turkish version of which was published in 1978). In 1981, Kunt published under the title *Bir Osmanlı Valisinin Yıllık Gelir-Gideri Diyarbekir, 1670-71* (Istanbul) or Annual Income and Expenditures of an Ottoman Governor: Diyarbekir, 1670-71, the «budgets» of Vezir Omer Pasha (d. 1102/1690) who had graduated directly from the Imperial household to a provincial governorship. In this monographic and mainly documentary study Kunt raises questions about trends in the career of this graduate of the Imperial household for which he should have been capable of finding answers in interpretations suggested by his earlier work (the book under review).



From Omer's biography we know that he had reached the rank of *silihdar* in the Imperial household (*enderun*) before he was assigned to the governorship of Egypt in 1072/1661-62. All this official's subsequent appointments were confined to provincial governorships in the East. During this period of Omer's service the Ottomans experienced one of the most critical periods in their history when great determining events took place in the West (European provinces and in defensive wars with European powers). Omer remained outside this arena and in these specially formative years. This prompts Kunt to raise the following questions about Omer Pasha's career :

«Why was Omer away from the major arenas of Ottoman history during those decades when the power and might of the empire were directed against the enemies in central and eastern Europe?» Then he raises a further question which is actually suggestive of an answer :

«Was it because he was not valued as a commander that he did not take part in the campaigns on the western and northern fronts? Or did he remain on the eastern flank because he was deemed a competent administrator, able to protect the empire from any potential threats there? In other words, does he go unmentioned [*not true*] in the Ottoman chronicles because he was a minor figure, or did he miss the chance to appear on the stage of history, stationed in the east as a trustworthy guardian of the empire?» (p. 31)

We should note right away that these questions imply unwarranted assumptions about Omer and this period. The author takes it for granted that Omer was a good officer. This is then followed by puzzlement for the lack of utilization of Omer's talents. Further Kunt assumes that soldiers were the most important elements in Ottoman society at this point and that soldiering was the most highly valued quality. Finally, he asserts erroneously that Omer Pasha goes unmentioned in the chronicles.

Omer was a palace graduate. Not untypical of the trend which Kunt in his study of the sultans servants so amply demonstrated for Imperial household graduates for the earlier period, he was sent directly into provincial administrative posts. Typical of such appointments, Omer was kept at that level and only in the East. This represented in part the trend of downgrading palace graduates. However,

the political downgrading of the Imperial household graduates does not mean their impoverishment, or outright ouster from the ruling class. Members of the elites do not get eliminated in that fashion. As with the military, the palace graduates remain as part of the Ottoman polity though with a more or less curtailed capacity to influence events. They are not however members of the dominant elite elements, in that sense they are more or less marginalized, though not totally without making bids to return to power.

Kunt, erroneously suggests that a measure of the great importance which the provincial governors who were Imperial household graduates attained, and in this case Omer specifically, is the great income and therefore accumulated wealth which they seem to have acquired (in contrast to income of earlier governors). But just as easily because he belonged to the ruling elite, he was not made destitute, Omer's great wealth should not be taken to represent a great surprise: Keeping his post required a great amount of wealth. Money had to be sent directly as a subsidy to the treasury and a lot spent on agents in the capital so he could retain his position in the pool of future appointees for provincial posts, etc.

As evidence for the contention that Omer (and therefore other palace graduates) was not totally marginalized, but had potential for playing an important role at the center, is the fact that Omer was mentioned by a contemporary chronicler as a potential candidate for grand vezir in the rebellion of 1099. (Date: 28, R. III., 1099, in Silihdar, *Tarih*, II, 331.)

The Imperial household continued to play an important role at the center, though not the dominant one, (it did occasionally: e.g. for a short period under Mustafa II) and its graduates continued to form part of the pool of available recruits for high office at the center in the second half of the seventeenth century. Moreover, Imperial household graduates were beginning to find unattractive «public» service (and specifically in the provinces). This is expressed by those who were offered provincial governorates when they excused their refusal to accept the «nomination» that they feared failure to meet their obligations with equity and justice. This has to be taken as no more than a pious posture that hid the hazardous and downgraded status of provincial appointments.

We had already noted on several occasions that Kunt has the tendency to mistake symptoms for primary dynamic as demonstrated from the way he interprets and accepts others' interpretations of the recorded «reactions» of contemporary Ottoman observers of the so-called phenomenon of «decline». This is illustrated by his direct reading of Ayn-i Ali, Âli, Kochu Bey, Akhisari. He gives the impression that he accepts Lewis' view of these authors as observers of Ottoman decline.

Perhaps the most damaging consequences of Kunt's fixation on the symptoms are methodological and historiographical. By dwelling on the symptoms, the author does not bind himself to the necessary task of studying and analysing the transformation of the so-called «traditional» society. Thus Kunt forgoes the arduous but necessary task of seriously pursuing the study and analysis of contemporary day to day life or the transactions of government. He seems to justify this neglect in part by claiming a lack of available sources, when these actually are available even for the period he has marked out for his research and study.

Kunt is at a loss to find the proper language with which to describe the framework and phenomenon that he «discovered.» In the end, he resorts to such anachronistic and ill defined terms as «modern» and «modernization» to describe the transformations of the sixteenth and seventeenth centuries. He begins by accepting the given frameworks of understanding and interpretation of Ottoman history, and seems eager and anxious to prove them correct. Had this ended merely in an exercise of mutual congratulations it would not have had serious consequences, but in fact it defeats Kunt's own scientific purposes of discovering and suggesting new ways of understanding Ottoman history. This is shown especially by the author's inability to account for the evidence whose interpretations contradict those he espouses with uncritical and unrestrained admiration.