One of the most striking characteristics of the Arabic medieval sources, whatever the discipline, is their immense depth. Therefore, it is perfectly legitimate for an author to keep the breadth of study within a tight sphere, as Larkin has done here. The following remarks should be viewed not so much as a criticism but more as a suggestion to give additional emphasis to the literary context of the work.

Although the author acknowledges (p. 6, n. 20) that her sketch of medieval Arabic literary theory (pp. 5–12) is intended as "the briefest of sketches" this reviewer found it lacking for two reasons. First, there appears to be no mention of some of the equally important primary sources written around the time of, or before, al-Jurjānī, which deal with what we might call literary criticism, such as Ibn ʿAbdrabbih's *al-ʿIqd al-Farīd*, or Abū Hilāl's *Kitāb al-Ṣināʿatayn*. Second, and perhaps even more important, there is effectively no mention of the literary criticism of prose which became an important subject from the fourth century A.H., especially. An example of these works is al-Tawḥīdī's *al-Imtāʿ wa l-Muʾānasa* in which one finds important analysis of *Kunstprosa* alongside discussions of poetry. Indeed, the whole relationship between prose and poetry became a major focus of literary debate around this time, but Larkin's assessment only really acknowledges the importance of poetry. Our remarks here may be less valid if it really is the case that only poetry and the Qurʾān, and not secular prose at least, were the pivot of al-Jurjānī's theory of discourse, as the author's examples seem to suggest. But in any case, perhaps a context-setting description such as this should include a broader perspective, or at least give some indication as to why only poetry is dealt with. In this connection it might also have been interesting to see how al-Jurjānī's theories and definitions endured during the following centuries. For example, it might be instructive to reflect on whether it was al-Jurjānī's perception of reference in relation to the term *maʿnā*—"when a speaker uses the name of a particular object in reality, he is referring to the image established in his mind of that reality . . . not to an actual instance of it as he speaks" (p. 66)—that effectively became the standardized one by the eighth century A.H. (Cf. ʿAlī ibn Muḥammad al-Sharīf al-Jurjānī's (d. 816/1413) *Kitāb al-Taʿrīfāt*, ed. Gustav Flügel [Beirut: Librairie du Liban, 1969], 242, where he defines *maʿānī* as "mental images for which expressions were posited, and the image which occurs in the mind for which the expression is intended.")

But overall this is an excellent book on a highly complex subject. The arguments throughout are logical, clear and well supported. Larkin has given much careful thought to translating key terms, and shows an awareness that some terms require a shifted nuance in certain contexts. To mention but one example, I like particularly the translation of *bayān* as "expressive ability" (p. 20) within the context of *ʿilm* "knowledge." Perhaps the most impressive aspect of the book is that she offers some serious challenges (p. 122ff. and elsewhere) to a number of the more authoritative works on al-Jurjānī that have (justifiably) stood their ground for some time, proving that many of al-Jurjānī's arguments were firmly grounded in *kalām*, and that attempts to view him from a modern psychological and literary viewpoint, for instance, should not be exaggerated. This book will appeal to literary theorists, specialists of poetry, philosophers and theologians, and, of course, linguists with an interest in vital theological issues that helped to shape some of the linguistic trends from the fourth century A.H. onwards.

ADRIAN GULLY
UNIVERSITY OF EXETER

Aspects of Ottoman History: Papers from CIEPO IX, Jerusalem. Edited by AMY SINGER and AMNON COHEN. Scripta Hierosolymitana, vol. 35. Jerusalem: THE HEBREW UNIVERSITY, THE MAGNES PRESS, 1994. Pp. 332.

Ottoman history is generally written as if part of a yet to be completed total history with a *telos* toward which political, social, and economic movements are pointing; the more pieces of the puzzle analyzed and accurately studied and the more we know about the internal dynamics of the Empire, then the better the chances for a reconstruction of this still to be written history. In the last three or four decades, and under the influence of state-of-the-art "social history," and the *Annales* in particular, the "document" has become the basic "unit of analysis" of the information available on the Empire (the qāḍīs' *sijill*s and the different brands of *defter*s of the state bureaucracy). However, what is usually of interest to historians is not the document as such, but how the data contained in each document fit within broader serial patterns: series, curves, systematic figures and patterns have become the main target of historians. Thus, historians are not usually interested in, say, the *tarikāt* registers of the Sharīʿa courts for their own sake, but for the data they contain on family and individual wealth, currencies, objects owned by people, and the like, with the hope that such "facts" will fit into something broader and be of significance to the social and economic history of the Empire. This has led to a de facto separation—rarely meditated upon in terms of its significance and historiographical implications—between what the document was "originally intended for" and what historians were constructing out of it. Thus, for example, Barkan was able to give estimates of population growth in major cities and inflation rates during the sixteenth century from a set of data which were originally intended for taxation purposes; other studies of a similar kind, but seldom using different methodological tools, opened up research on land tenure, rent, and taxation problems, population growth, currencies, prices, and inflation rates; cities, their neighborhoods, and guilds, élite groups such as the military or the a^{c}*yān*; etc. Broadly speaking, this type of historiography worked with the following set of premises:

(i) In respect to the more traditional politico-military history, the narrative has shifted from the broad set of political events perceived at the surface, which were often tied up together either according to some naturalistic logic (what comes after is explained by what comes before) or an ultimate *telos* which brought harmony to fragmentation (centralization and decentralization). All this has shifted to another kind of narrative, the socio-economic, based on serial analysis and an empirical study of documents.

(ii) The socio-economic history of the Empire, by opening up the historiography to hitherto unexplored subjects and new themes, does not easily fit the rubric of some ultimate end or *telos*. If, as Norbert Elias once pointed out, state-formation in France "is astonishingly continuous and direct," by contrast, Ottoman state and societies had much more fragmentary and dispersed histories, which might need multi-threaded narratives and plots before reaching any overall embracing theme (if any).

(iii) The old political and military history that has been overshadowed by the new discoveries needs to be reconsidered in the light of the new set of paradigms established by socio-economic history—in particular, the urgent issue of understanding the fundamental nature of sovereign power in the Empire—that is, the legal fiction of the Padishah. In short, the equivalent of Kantorowicz's *King's Two Bodies* is badly needed.[1]

(iv) Historians have been interested in documents for the empirical evidence they might bring to their research; little, however, has been done to understand the nature of these documents, how they are intrinsically organized, and to analyze the discursive formations to which they belong. A study of their discursive formations would show the dispersion of these discourses, and at times, the weakness of their internal coherence. Of course, this would lead to an organization of the material on grounds very different from the way it has been hitherto structured.

The Comité international des études pré-ottomanes et ottomanes (CIEPO) organizes on a regular basis—every two years—a conference where papers on pre-Ottoman and Ottoman studies are read and discussed. At its last, tenth, meeting, in Prague in September 1996, over a hundred papers in four languages were presented. The wide range of topics and themes partly reflects the national origins of scholars and their methodological training. Turkey has probably the largest number of Ottoman scholars, the United States comes easily second, while Israel maintains an aggressive third position. Europe makes an important contribution, too (with France and Germany as the two main contributors), but it is still fragmented along national lines.

Twenty-six of the over one hundred twenty papers (half were by Israeli scholars) from the ninth CIEPO Congress, which convened in Jerusalem in July 1990, were grouped in a single volume and published by the Hebrew University in Jerusalem, thanks to the careful attention of Amy Singer and Amnon Cohen.

The selected papers of CIEPO IX reflect, for better or for worse, some of the progress achieved in Ottoman historiography, and its problems and shortcomings, too. The editors have divided the articles into four parts, with the first one, on towns, being the largest and also the most interesting. The other parts are on early Ottoman history, international relations, and the literary, administrative, and other sources. But the organization could have been slightly different in order to achieve a better coherence for some of the themes. For example, there are several articles on Jerusalem and its *sancak*, which could have been regrouped together in one section.

The second part, on early Ottoman history, begins with a paper by Sina Akşin on "The Three Homelands of the Turks." The paper reflects many of the presuppositions common to Ottoman history and is therefore worth looking at in some detail. In a few pages, Akşin does a nice job of providing a very brief sketch of Ottoman history—from the nomadic origins of the early Turks, to the classical period of the Ottoman state, up to its dismantlement and the formation of the Turkish Republic. This is a lot to cover in one short article, but brevity and a skill for generalization has the merit—often without the awareness of the author—of pointing to problems and weaknesses. The first homeland of the Turks was in the broad region north of China, the so-called Andronovo cultural area. These Turkish nomadic herdsmen survived on a basis of limited agriculture, war, and plunder, all at the same time. Akşin points out that the quasi-states established by the early nomadic Turks between 200 B.C. and 840 A.D. could not have led to the "brilliant civilization" which ultra-nationalist Turkish historians have assumed. The Turkish tribes then came in contact with Islam, which they eventually adopted as their own religion, when they moved west to their "second homeland," Transoxania, to the southeast of

[1] Western societies take for granted the historical and conceptual separation between Church and State, a process which was described in Marc Bloch's *Les Rois thaumaturges* and Kantorowicz's *King's Two Bodies*. In Byzantium, however, the king shared also the functions of priesthood and it was legitimate for the temporal power of the state to meddle in the affairs of the Church; see Gilbert Dagron, *Empereur et prêtre: Étude sur le «césaropapisme» byzantin* (Paris: Gallimard, 1996). The Islamic tradition, which also does not separate political and religious powers, could indeed have picked up the concept from the much older Byzantine tradition and modeled it within its own perspective: a vast opportunity of research for Ottoman historiography.

Lake Aral (unfortunately, no maps are provided with the paper). The reasons behind this migration from "Central" Asia are still debated among historians. It was at this stage that the Turkish "tribal states" developed agriculture, urban life, a sense of social stratification, and literary works; but all this was still extremely limited, according to our author.

Basically, the roots of the Ottoman Empire, as we know it, belong to the second migration westward to Anatolia in the eleventh century—the third homeland. The advantages of Anatolia were many: the rainfall and the wheat-growing steppe making possible the sedentarization of the nomadic herdsmen. The rest of the story is better known—the conquest of Rumelia and the rest of the Byzantine territory, until the fall of Constantinople in 1453. At this point, Akşin proposes opening up scholarship on Byzantine culture and heritage, in particular since the early work of some Turkish historians, such as Fuat Köprülü, in a book published in 1931, sealed off the issue by establishing "the absence of such influences" (p. 149). It should be noted here, *en passant*, that thanks to the work of Gilbert Dagron it has become more evident that the Arab and Islamic (and later Ottoman) preoccupation with the distinction between *khilāfa* and *mulk* parallels a Byzantine concern with the notion of "césaropapisme" and the theory of the "two powers"—the temporal and the religious. The Ottomans, like the Byzantines and Arabs before them, opted for a non-separation between the two.

Besides this openness to the Byzantines and the Christian populations in Anatolia and Rumelia, it is not clear what Akşin proposes regarding the nature of the early Turkish nomadic tribes and their need for conquest. How did these tribes relate to Islam as a religion? Should we accept in this case Ibn Khaldūn's thesis that religion has no effect on a tribe without the *ᶜaṣabiyya* which brings it together? What kind of "House" (*bayt*) was that of Osman, and what were its "subservience" (*istitbāᶜ*) strategies? Besides their importance for the early tribal history of the Turks, such questions could be relevant for an understanding of the "classical" Ottoman state. Unfortunately, at this point, Akşin wants to describe Ottoman state formation and the sedentarization of the nomadic tribes as a "development of feudalism." Feudalization, which went into effect with the extensive application of the *timar* system, is perceived as developing in "an inverse relation" (p. 150) with nomadism. It is indeed highly inappropriate to describe a system in which the majority of land was state-owned, and coupled with the existence of a centralized bureaucracy, as "feudal." In fact, "feudalism" in its European version, presupposes small "monopoly" formations being absorbed by larger ones—which as a process, is almost antithetical to anything witnessed in the Ottoman Empire; in Europe, it indeed led to the absolutist states and their court societies. Besides that, it is debatable whether the establishment of an Ottoman state and the relative success of the implementation of the *timar* system were in an "inverse relation" with the tribal nomadism of the House of Osman. Middle Eastern institutions seem to be Wagnerian in nature: it takes them a long time to disappear—notice that *timars* were still available until the nineteenth century, the century of reforms from the *iltizām* system!

Akşin seems to impose a similar teleological outlook on the rest of Ottoman history. The *timar* was replaced by the *iltizām*, which led, in the eighteenth and nineteenth centuries, to "the rise of a feudal class in the Ottoman Empire—the *âyan*" (p. 151). How could the *aᶜyān* represent a "feudal class" when they received the *mīrī*—that is, the "rent"—on behalf of the state? The problem with this approach, besides employing categories which could not possibly work for the Empire, is that the question is never raised as to what these social and economic movements led to in the final analysis. What did the power of the urban *aᶜyān-multazim* class contribute to city life? Another equally important problem with the "feudal" approach is that "feudalism" stays too long—until the nineteenth century. When remnants of "capitalism" finally emerge—basically, through the movement of land commercialization after the 1858 Land Code—they seem to have been triggered more by a world capitalist economy than by anything "internal."

Speaking of the *aᶜyān*, Dror Ze'evi nicely sums up, in his paper, what the research on this élite group has amounted to. Such research owes a lot to Hourani's Weberian classification of the notables as an urban "patriciate" group whose primary source of income was the land tenure system of the Empire. In his 1968 article on the "Politics of the Notables," Hourani emphasizes the multiple activities of this élite group: as men of knowledge, *ᶜulamāᵓ*, as "intermediaries" between Istanbul and the local cities, as *multazims*, and merchants, etc.

Weber was primarily interested in "forms of legitimation" which need to be supplemented, through empirical research, by actual forms of domination: how, for example, did the *aᶜyān* of the Ottoman cities concretely establish their control over *specific* urban and rural "networks"? Not surprisingly, the "debt (*dayn*)," among other things, proved to be a helpful tool in establishing such networks. Weber comes back to his notables in the last part of *Economy and Society*, devoted to the city, only to acknowledge—and this applies in particular to Arab-Ottoman-Islamic cities—that the power of the notables does not help much in consolidating either a sense of "community" or a perception of what the political, legal, social, and economic problems of the city are. In short, the administration of notables in many Oriental cities *fragments* their populations along networks of allegiance—an essential aspect of the Weberian problematic which many admirers of Hourani (and Hourani himself in the first place) totally missed. This fragmented aspect is mostly visible in the literature the notables produced. Indeed, the *aᶜyān* were the only fraction of society who left an abundant literature: biographies, sufi manuals, histories, personal autobiographical essays, etc., in addition to their extensive contribution in the *majālis* of the Tanẓīmāt.

As Hüsamettin Mehmedov points out in his paper on the *mufassal defterleri* (detailed survey registers) of the early eighteenth century, an understanding of the meaning of the terms *aşiret*, *taife*, *oymak*, and *cemaat* is an essential aspect for constructing the Ottoman concept of population, which in itself is a prerequisite for understanding taxation and rent; in other words, we are once more in the field of discursive formations. How did, for example, a term—a "concept"—like *aşiret* have its meaning "dispersed" between the bureaucratic *defters*, the *a*ᶜ*yān* or "literary" texts, or the Sharīᶜa court records? What kind of "object" did the use of the term construct in each case? Such questions would have been helpful in conceptualizing many of the statistical figures provided in Uziel Schmelz's study on the Ottoman Census of 1905 for the city of Jerusalem (or Yehoshua Ben-Arieh's long contribution on the *sancak* of Jerusalem in the 1870s): by the turn of the century, if not earlier, the Ottomans moved, in classifying their populations, from the earlier notions of *ḥane* and *cemaat* to that of the "individual," *nafs*. Under the influence of Western values, and to insure a better control by the state of its populations—or of its "societies"—a new era began, at all levels, with the vague notion of the "individual."

Out of the deliberately tiny selection of articles I have chosen from this rich book, a theme emerged, which might not have been the explicit theme of this range of articles, concerning the discursive nature of the sources used, whether "literary" texts or any kind of "documents," for that matter. Ottoman historiography is in general suspicious of "literary" texts, as if their "ideological" nature makes them less reliable, and thus the choice for many historians has been on the side of "documents," whether of a bureaucratic or legal nature, or consular reports. The result has been the creation of an *Annales*-type of history with a focus on population, land, cities, guilds, and the like; but very little is available on the intellectual and literary trends in the Empire, the legal and political systems, the arts, architecture, etc. There are two broad assumptions at work here. The first is that "documents" are more "reliable" as a source for writing history because they are assessed in terms of their empirical value; and the second is that "literary texts" and "documents" are in essence significantly different and there does not seem to be much hope in bringing the two together. Yet, the "documents," like the "literary texts," are essentially *discursive practices*, and both should be studied with great care and assessed in terms of the *discursive formations* which structure their content and syntax. The emphasis on the discursive could also shed new light on the notion of the individual "subject," since statements in their essence have this power to individualize; it would also make more manifest the dispersion of elements which thus far Ottoman historiography has done its best to avoid.

Zouhair Ghazzal

Loyola University, Chicago

The Eye of the Beholder: The Life of Muḥammad as Viewed by the Early Muslims. A Textual Analysis. By Uri Rubin. Studies in Late Antiquity and Early Islam, 5. Princeton: The Darwin Press, 1995. Pp. ix + 289. $27.50.

Separating biography from hagiography—history from fiction, in the words of Rubin—has been the major historiographical challenge faced by those writing about the life of Muḥammad, according to their own explanations. In his standard treatment of the subject, W. M. Watt, for example, has spoken of the tendentious elements found in the Muslim accounts of Muḥammad and the need for modern historians to use their critical senses to eliminate those factors considered "impossible" within a historical framework.

In his new book, Uri Rubin moves this discussion of Muslim retellings of the life of Muḥammad onto an entirely new plane. In a manner similar to that broached by John Wansbrough in *The Sectarian Milieu* (Oxford, 1978), Rubin disavows any interest in the life of Muḥammad in a historical sense and focuses on how the Muslim accounts reflect "the self-image of medieval Islamic society" (p. 3). It is not his intention to make any attempt to distinguish between "legendary" and "historical" elements in the stories (as opposed to earlier attempts to speak of the Muslim image of Muḥammad, written by people such as Tor Andrae and Annemarie Schimmel).

A key methodological element employed by Rubin, which he suggests separates his work from that of Wansbrough, is found in his emphasis on the citation of a multitude of sources, an approach which has become associated with the work of M. J. Kister. Rubin's review of the sources from which the accounts of the life of Muḥammad may be culled (pp. 5–17) emphasizes the wide diversity of material which must be taken into account, ranging as it does from *ḥadīth*, *sīra*, *ta*ᵓ*rīkh* and *tafsīr*.

Rubin's book focuses on certain aspects of the retelling of the life of Muḥammad within the early years of the prophet's emergence in Mecca (the promise is made of another book dealing with the Medinan period). He identifies five themes for attention, and each is given its own section: attestation, preparation, revelation, persecution, and salvation.

One emphasis within Rubin's reading of the story of Muḥammad is seen in the conviction that early Muslims needed to assert that their prophet was from the same line as those believed in by Jews and Christians. This situation of inter-religious confrontation is witnessed in the emphasis found in the stories on the "annunciation" of Muḥammad, a motif familiar within the biblical tradition and through which Muslims made their appeal. "This was supposed to convince the People of the Book who refused to recognize Muḥammad as a prophet like their own" (p. 21). Ideas of the Paraclete, of the *ummī* prophet (in Rubin's reading, the prophet of the nation [*goy*] who is illiterate), and of the simple attestation of a prophet