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## BOOK REVIEWS

SA'ĪD, 'Abdullah Ibrāhīm. *Ashkāl al-Mulkiyya wa Anwā' al-Arādi fī Mutaṣarrifiyyāt Jabal Lubnān wa Sahlū'l-Biqā'*, 1861-1914. Beirut: Maktabat Bisān, 1995. Pp. 374.

VAN LEEUWEN, Richard. *Notables and Clergy in Mount Lebanon, The Khāzin Sheikhs and the Maronite Church (1736-1840)*. Leiden: E.J. Brill, 1994. Pp. 290; NLG 130,-/US\$ 84.-.

Ottoman Lebanon was an unusual case within Bilād al-Shām and the Arab East. At the political and socio-economic levels, Mount Lebanon had set for itself, since the beginning of the Ottoman conquest in 1516, a pattern different from the rest of the Empire: (i) Its ruling families were not the traditional *ashraf* families that ruled from within the cities of the Empire—a system that the Ottomans had inherited from the Arabs and adapted to their own needs; the notables of Mount Lebanon were neither descendants of the Prophet nor, necessarily, Sunnī Ḥanafis, dominant in the urban parts of the Empire among its “leading families”; in addition, some of Mount Lebanon’s “rulers,” such as the Druze Fakhr al-Dīn al-Ma‘nī II and Bashīr II the Great, whose family had converted from the Sunnī to the Maronite faith, enjoyed considerable political and economic “autonomy,” something that, in four centuries of Ottoman rule in Bilād al-Shām, only Ḍāhīr al-‘Umar had dared to assert in Palestine, and only for a very limited period of time; (ii) Because of the relative “autonomy” of its rulers, the complexity of its confessional “minority” system, and the limitations imposed by the nature of the land, the *timār/sipāhī* taxation system (whereby the élite Ottoman cavalymen [*sipāhis*] were granted parcels of land [*timars*] for tax collection) never developed in Mount Lebanon and the *iltizām* system (a tax-farming system whereby parcels of land were granted by auction on a one- to three-year basis, usually to an *a‘yān* family) was adopted right from the beginning (it took the entire seventeenth century for the *iltizām* system to be reinforced elsewhere in the Empire); (iii) The Maronite church, which was linked to Rome in the mid-sixteenth century, went through a process of “rationalization” and, since the early eighteenth century, provided Mount Lebanon with the ideological superstructure it needed; (iv) Finally, the nobility was not separated from its peasantry on an urban-rural basis as was the case in many parts of the Empire; instead, the physical proximity of the nobility and their peasants, and the role of the Maronite church in providing support for the poorer peasant classes (among whom a large number of the clergy were recruited), provided the peasantry with a “political consciousness” unusual in the Arab East: the Lebanese peasants organized “revolts” against their *muqāṭa‘jis* on several occasions in the nineteenth century.

One would expect such a rich historical setting to have attracted many researchers, students of the Ottoman Empire and the Mashriq in particular. Surprisingly, not much has been done since the 1960s when Chevallier and

Ḥarīq produced the first serious works of synthesis on Mount Lebanon. It is only recently, probably due to the technical “end” of the civil war and the *pax Syriaca* in the region, that historiographical activity has been renewed. Unfortunately, the current work lacks the vitality and intelligence of the old masters.

‘Abdullah Ibrāhīm Sa‘īd sets for himself the difficult task of exploring the different types of property in Mount Lebanon and the Biqā’ valley and their evolution from 1861 until 1914. He observes that the Ottomans followed the same principle in acquiring state lands as did the Arab-Muslim conquerors in the seventh century and thereafter: once a land was acquired by conquest, it became state owned (*arāḍī amīriyya* or simply *mīrī* in Ottoman terminology); “private” land (*mulk*) was either subject to the *‘ushr*, if owned by Muslims, or to the *kharāj*, if owned by non-Muslims; other categories of land included *mawāt* (uncultivated lands that did not produce any revenues and were therefore regarded as “dead” by the state), and *waqf*. The early Arab conquerors who had established their authority over Syria, Iraq and Egypt and who were entrenched in garrison cities (*amṣār*) exploited their state-owned lands through the *iqṭā‘* system. In practice, this required giving large parcels of lands (*muqāṭa‘āt*) for exploitation mainly to individuals from the conquering armies, thereby contributing to the establishment of a land-owning military bureaucracy, a feature that characterized all subsequent Islamic empires.

The Ottomans obviously were very much attracted to a system of land tenure and taxation that kept the majority of cultivable lands state owned and that eliminated the need to spend large sums of cash on their army—the bulk of their cavalymen were granted *timārs* in lieu of salaries. By the late seventeenth century, the *timār* system was gradually and successfully replaced by the *iltizām*, a more “modern” system of taxation and rent that separated once and for all the military functions from those of tax and rent collection.

Sa‘īd’s discussion of the period before the middle of the nineteenth century takes up roughly the first half of his book; he devotes the rest of it to an examination of forms of property—most of the old forms (such as *iqṭā‘*, *kharāj*, *mawāt*, etc.) have survived albeit with modified connotations reflecting changing socio-economic conditions—from the establishment of the *Mutaṣarrifiyya* regime in Mount Lebanon in 1861 until the breakup of the First World War. The *Mutaṣarrifiyya* regime was created after the failure of the two *qā’immaqāms* system which was implemented right after the Egyptian withdrawal in 1840. This system divided the territory of Mount Lebanon into two separate administrative units—a Maronite one in the North and a Druze one in the South—each known as a *qā’immaqāmiyya*. It came to an end during the 1860 civil war. Sa‘īd’s decision to begin his exposé in 1861 no doubt was prompted less by an interest in the *Mutaṣarrifiyya*’s political successes or failures than by a desire to understand the economic and social implications of the Land Code of 1858, which “transformed land into a commodity that is bought and sold on the real estate market” (p. 138).

Toward the end of the *Mutaṣarrifiyya* regime in Mount Lebanon (Sa‘īd does not specify a date here but he seems to be referring to the turn of this century), 32.6% of the total land surface was privately owned (*mulk*); 6.3%

was *waqf*; 15.4% was *mīrī*; 14.3% was *mushāʿ*, that is, collectively owned by the inhabitants of villages and their peasants; and finally, 31.4% was classified as *mawāt*. Considering that *mushāʿ*, *mīrī*, and *mawāt* were all state-owned lands, 61.1% of the total land surface of Mount Lebanon was not in private hands, at least formally.

Saʿīd examines each one of these categories (and their respective sub-categories) separately and traces their evolution throughout the *Mutaṣarrifiyya* period, relying heavily on major Ottoman legal texts such as the *Dustūr* (in its Arabic translation by Nawfal, 1883-1884) and the *Majalla* (based on the Arabic edition of Salīm Bāz). Despite Saʿīd's use of some Shariʿa court records from Damascus and Bayt al-Dīn, his reliance on "modern" Ottoman legal texts poses some problems (Saʿīd does not seem much at home with the Ḥanafī *fiqh* texts of the Ottoman period, in particular those written by Ibn Nujaym, al-Ramlī, and Ibn ʿAbidīn). For example, with regard to the two categories of *mulk* and *mīrī*, Saʿīd notes that the text of the 1858 Land Code distinguishes four types of *mulk* lands, the fourth being *arāḍī kharājīyya*, that is, lands subject to the *kharāj* taxation system, as in the Umayyad and early Abbasid periods. He then concludes:

Since the lands in Mount Lebanon were *arāḍī kharājīyya* that were subject to either the *kharāj* or the *māl mīrī* as a lump sum (*maqṭūʿ*), they were considered *mulk* lands by custom and habit (*bi'l-ʿurf wa'l-ʿādah*), [because] the rulers of the *muqātaʿī* epoch had abused the labor of their peasants while improving these properties and changing their status to *mulk* lands at the individual, family, and group levels (p. 181).

This is one of several passages in the book that creates confusion. First, the *arāḍī kharājīyya* still existed as a category in the Ḥanafī *fiqh* books of the Ottoman period and this distinction was kept, by tradition, in later nineteenth-century "secular" texts that regulated the status of land during the *Tanzīmāt* period; beyond this formal acceptance of *kharāj*, in conformity with the Islamic tradition, the concept lost a great deal of its practical importance in the Ottoman period. Abū Yūsuf tells us that *kharāj* was the "tax" imposed on the conquered lands "owned" by non-Muslims, a practice that became redundant in the land tenure system during the Ottoman period. For the nineteenth century, in particular, and in the thousands of Shariʿa court documents that I have examined from Beirut and Damascus, I have not encountered the category of *arāḍī kharājīyya* even once: the texts mention the *māl mīrī* or, strictly speaking, the "rent"; and they mention the "right of *iltizām*" that the *multazim* paid on an annual basis (in cash or kind) when he auctioned the land, that is, the "tax." The great majority of agricultural lands around the big cities were *mīrī*, and even though they were often "associated" for long periods of time with *aʿyān-multazim* (tax-farming notables') families, it would be misleading to associate them with the category of *mulk*, even after the Land Code of 1858, for the simple reason that their "rent," the *māl mīrī*, continued to pour into the state's treasury. Even more confusing in the above quoted passage is Saʿīd's association of his *mīrī-kharāj-mulk* distinctions with the statistics he presents: were the 32.6% of lands that he qualifies as *mulk* "purely *mulk*," or were they *mīrī* lands considered *mulk* by "custom and habit"? In the latter case, what about the 15.4% of *mīrī* lands? Why weren't they regarded as *mulk* by the same "custom and habit"?

There would have been less confusion if Sa'īd had not taken some of the Ottoman legal texts at face value and if he had made greater use of the Shari'a court records to confront the legal texts with their underlying "reality." In his bibliography, Sa'īd lists two major sources of Shari'a documents: Damascus from 1871 to 1890, and Bayt al-Dīn from 1882 to 1894. Unfortunately, Sa'īd's use of these sources is sporadic at best. There is no systematic analysis of even a single document in the book. The Shari'a documents of Bayt al-Dīn, in particular, are a missed opportunity: it would have been important to analyze how the Druze *qadā'* functioned and to determine whether it differed from the Sunnī *qadā'* and how "minority" groups in Bilād al-Shām conducted their legal business.

Sa'īd's main thesis is that the *Mutaṣarrifiyya* period witnessed a wide movement of land commercialization and the transfer of *mīrī* lands into "private" hands. Although probably accurate, the thesis is hard to defend from official legal texts alone, and it lacks the descriptive quality that only the Shari'a court records would have made possible. These sources also would have provided more concrete descriptions of the manner in which the *muqāṭa'jī* families operated and how they related to their peasantry. Finally, Sa'īd uses a wide range of Arabic, French, and English "secondary sources" that provide him with essential statistics and information; yet, for some reason, he fails to acknowledge the work of leading historians on the land issue in the Ottoman Empire (Barkan, Inalcik, and Karpas, among others), an omission that creates several gaps and errors in Sa'īd's narrative.

Although concerned with an earlier period that ends with the demise of the Shihāb family as a political entity and coincides with the end of Egyptian rule in Bilād al-Shām, Richard van Leeuwen's main thesis is close to Sa'īd's in many respects. The similarity between the two books lies in the central assumption "that the developments in Mount Lebanon from the end of the 16th century onwards were shaped by increasing interaction with the world market" (p. 235). Beyond this fundamental assumption, van Leeuwen's study differs from Sa'īd's in the way he connects the land issue (in particular *waqf*) to other major institutions, such as the "nobility" and the Maronite church. In van Leeuwen's account, contacts between Mount Lebanon and economic centers in Europe began as early as the Ottoman conquests, when the export of silk led to the privileged relationships that Fakhr al-Dīn Ma'n II enjoyed with the grand duke of Tuscany. The cultivation of raw silk and its exportation to Europe were to remain Mount Lebanon's favorite "economy" which sustained its contacts with Europe. This state of affairs culminated during the rule of Bashir II Shihāb (1790-1840).

Van Leeuwen discerns an interesting parallel between the integration of Mount Lebanon into a "world-economy" and the evolution of other institutions such as the nobility and the Maronite church. He chose to analyze the Khāzin family largely because it was the only one that, throughout the seventeenth century, established itself as a strong *muqāṭa'jī* family and completely dominated the Maronite church. However, its political and religious power deteriorated rapidly in the eighteenth century (because of divisions within the

family, property fragmentation, and an inability to cope with the emerging forces of “capitalism” and mercantilism, etc.). The weakening of the Khāzins left their political rivals, the Shihābs, more powerful than ever and transformed the Shihābs into the most influential political family in Mount Lebanon. Particularly during the fifty-year rule of Bashir II, the Shihābs adopted a strategy different than that of the Khāzins: they did not provide the Maronite church with any clergy or patriarchs and they controlled this religious institution from the “outside,” leaving the church relatively “autonomous”; the Shihābs exercised greater control over Druze territory and, in 1800, Bashir II decided to build his own palace in Bayt al-Dīn, where the Druze had their most important *muqāta‘āt* and fiefs; finally, “Lebanese politics” became better integrated with that of the Bilād al-Shām and the Ottoman Empire in general, a process that, ironically, made impossible a “return” of Bashir II once his protector, Muḥammad ‘Alī of Egypt, withdrew in 1840.

Van Leeuwen’s material is rich and his thesis is strong and coherent, although not without problems. First, given the fact that van Leeuwen stresses the “dynamic element in society” (he regards the *waqf*-institution as an example of this “dynamism”) over “the concept of a ‘stagnant’ Islamic society” (p. 3), his implicit assumption that practically every major “change” in the society of Mount Lebanon was triggered by outside factors such as the capitalistic world-economy is surprising. Only in the conclusion (pp. 235-242)—where the author lists “changes” such as the status of the “economy” of Mount Lebanon, the Khāzins’ gaining control of the Maronite church and their demise in the eighteenth century, the internal transformation and restructuring of the church—does the reader discover that no major “change” qualified as internal, that is, as stemming from transformations within the major institutions of this society. I am not suggesting that “change” should be internal, but rather asking whether the author has adequately formulated the problem of institutional transformations and “world-economy.” (It might indeed turn out that “change” was the result more of the forces of capitalism than indigenous institutional changes.)

For purely analytical purposes, this question can be divided into two parts: (i) How is it possible to describe the *modus operandi* of traditional institutions like the nobility and the Maronite church and the meaning of “change” (or “stagnation”) in this context? (ii) How does the concept of “world-economy” articulate with notions of indigenous institutional change? The two questions are interconnected because a concept of “world-economy” that does not include prior knowledge of the “mode of functioning” of traditional institutions would operate in a vacuum: How is it possible to know whether any “change” occurred without prior knowledge of exactly what it was such change had impacted upon?

Van Leeuwen is concerned with several institutional frameworks—the nobility, the church, and the *waqf*—but his descriptions of these institutions are fragmented and never achieve the coherence of a theory that would describe how major institutions in a particular society operate, function and work together as a totality. Consider the “nobility” as an example of institutional change and/or stagnation. Because it was not structured on the principle

of “descent from the Prophet,” the Lebanese nobility created enormous barriers for families that wanted to join the group of notables. In this regard, control of “territory” was extremely important, and the peasantry did not suffer from the urban-rural division that lay at the basis of the social and geographic distance between the Empire’s urban *a’yān* and their peasants. In Mount Lebanon, because of the proximity between the rulers and the ruled, control of the peasantry was a more direct process; relationships among the notable clans were strained and bloody, and the hierarchy within the group, at the head of which stood the *hākīm*, was maintained through a system of political marriages among families with a different status.

The concept of “world-economy” (*économie-monde*) developed by Braudel for the sixteenth century and thereafter requires a prior knowledge of all the social structures, markets, networks (*réseaux*), etc., that make possible the existence of a particular political economy, be it capitalism or something else. What is needed for Mount Lebanon is a concept of political economy that would prepare us to see when and where this “integration” with capitalism took place. Although van Leeuwen does some of this work, it is scattered and hard to follow; the lack of a coherent theory means that many of the “facts” about the notables, their families, and the church are presented, for the most part, chronologically, and the organization of the book does not help create coherence: the author begins with a brief formal chapter on the Ottoman “economy” before moving to what he calls “regional differentiation,” disputing the idea of notables undermining the central state authority because “a certain degree of decentralization had always existed within the Ottoman administration, and in fact constituted one of its mainstays” (p. 16). Van Leeuwen then devotes several parts of the book to the “concept of *waqf*,” a theme that recurs frequently. Although he brings new insights to the *waqf*-institution (which, as noted, he regards as a factor of social dynamism and change) and to Maronite *waqfs* in particular, he does so without a general elaboration of a concept of property, land tenure, taxation, and the like. The emphasis on *waqf* comes as a surprise to the reader, because no material is presented indicating where *waqf* stood in comparison to other forms of property such as the *mulk* and the *mirī* (judging from Sa’id’s numbers, *waqf* was only 6.3% of the total land surface of Mount Lebanon early in this century, ranking fourth after *mulk*, *mirī*, and *mushā’*; and even if *waqfs* were stronger in the first half of the nineteenth century, it is unlikely that the movement of land “commercialization” and “privatization” would have eaten up so much *waqf* property, especially if *waqf* was a factor of social stability and change). It would have been desirable for van Leeuwen to select a couple of *waqf* documents and analyze them, which brings me once again to the crucial issue of document analysis and interpretation.

Van Leeuwen’s most original sources are the documents preserved in the archive of the Holy Congregation for the Propagation of the Faith in Rome (referred to as the “Propaganda”); he also relies on the archive of the Khāzin family, monastery archives, and the like. Because van Leeuwen’s “reading” of documents (like Sa’id’s) consists in picking up factual information here and there and paraphrasing its contents—as opposed to more in-depth textual

analysis—the novelty and originality of the Propaganda archives is poorly represented and is actually buried beneath the mass of secondary literature used by the author. The Propaganda archives are in fact crucial for understanding the nineteenth-century legal reforms. In theory, the Maronites had to follow the Ḥanafī legal code adopted throughout the Empire, which, for them, meant several concessions in terms of property rights. First, in Ḥanafī jurisprudence, Christian *waqfs* erected to support the maintenance of Christian religious institutions were, in principle, not allowed; second, the Qurʾānic rule of inheritance which gives a woman half the share of a man was seen by Christians as a threat to the integrity of property. Van Leeuwen details the “legal reforms” in a single short section (7.1) from the correspondence between the Maronite patriarchate and the Propaganda. Briefly, the issue centers on the role and function of the Maronite qāḍīs and their appointments, and the complex relations between the imposed and predominant Turkish “Civil Law” (in the patriarch’s terminology) on the one hand, and Church Law, on the other, both, according to the Patriarch, derived ultimately from Justinian’s *Codex*. Van Leeuwen observes that the patriarch, by accepting the implementation of Ottoman Law in a “secularized” version, acted from a position of strength because his purpose was to give the Maronite qāḍīs in Mount Lebanon greater “autonomy” rather than having them rely on the Ḥanafī courts of Beirut, Sidon, and Tripoli. It is unfortunate, however, that van Leeuwen never directly cites the original texts (indeed, I could not determine the language(s) of the Propaganda archives); he reports and paraphrases the requests and their replies as if each correspondence had a single coherent meaning.

The focus in the writing of history has shifted back to the role of the document-as-text as a basis for the construction of historical narratives (see, for example, the alarming *Annales* editorial in December 1989 and consider the significant change in the title of the journal in January 1994 to *Annales: Histoire et Sciences Sociales*). What a textual analysis of documents introduces is a *hermeneutics of suspicion* that is badly needed in Middle Eastern studies. I would have wished in particular that both Saʿīd and van Leeuwen had integrated more thoroughly into their works a fuller textual analysis of all the primary documents they had to rely upon, be they Shariʿa court documents, family archives or the archives of the Maronite church. This would have given them (and us, as readers) a unique opportunity to see the inside of the complex textual construction of each document; how each one of these documents constructs a “reality,” and let *us* experience this reality, first through their eyes and then through ours. The document-as-text, instead of simply paraphrasing the document as if it had a single message, would offer us a more complex hermeneutical process linking the “past” with the “present.”

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