

Review

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gument (p. 3), by the end of the book—perhaps responding to her reviewer’s criticism—she acknowledges the possibility that Jordan could in fact be a very easy case (p. 298). At a minimum, Jordan’s extraordinarily high level of dependence on foreign aid does call into question its value as a test case for a budget-security argument, simply by virtue of the obvious pressures it faces to keep the aid pipeline flowing. Moreover, unlike the unruly domestic economies that most states rely on for their money, foreign aid is particularly amenable to political management. As a result, and despite Brand’s argument to the contrary, Jordan’s experiences would seem to offer rather few generalizable conclusions regarding the effects of domestic economic structure on alliance behavior.

Brand’s study is limited by its realist treatment of economics, but the questions raised here should not be permitted to overshadow her considerable accomplishment. Scholars who study the Middle East complain regularly—and with some justification—about the lack of theoretically sophisticated work on the region and about the exclusion of Middle Eastern cases from theoretical debates in the disciplines. In *Jordan’s Inter-Arab Relations*, Brand has delivered a substantial response to these complaints. She has shown quite decisively the benefit of using deep regional expertise to engage theoretical issues, has defined a set of theoretical questions that other scholars can productively pursue, and has thus moved one step further the important task of connecting the Middle East more centrally with the social sciences.

COLETTE ESTABLET AND JEAN-PAUL PASCUAL, *Familles et fortunes à Damas: 450 foyers damascains en 1700* (Damascus: Institut Français de Damas, 1994). Pp. 226.

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Colette Establet and Jean-Paul Pascual’s monograph examines a total of 628 succession inventories (*tarikāt*) from the shari‘a-court registers of Damascus between the years 1686 and 1693 and 1698 and 1717. Out of the 628 inventories, they narrowed their selection still further to a total of 449 because of several problems encountered in the disregarded records. This means, according to their calculations, that their study covers an “honorable” 5.6 percent of the estimated 8,000 families who lived in Damascus at the end of the 17th century. In order to analyze such things as population growth, death patterns, prices, price inflation, debts, money, and the like, Establet and Pascual constructed a statistical model from their sample based on a computerized data base in which each record is devoted to a *tarikā*. No doubt some will question the validity of this sample on the basis of the low number of records, the years involved, or the “scientific” value of the results; my concerns, however, are different: I question the authors’ method in reading the court documents and *tarikāt* records, whatever the volume of the sample is.

Interestingly, even though Establet and Pascual explain at length how they worked out their data and how they ended up with the statistical figures that populate their book, they never explain their reasons for choosing statistical and quantitative analysis as their main grid for systematically organizing *tarikāt* documents. Their choice is not that obvious, considering that *tarikāt* documents were not intended primarily to be informative on currency fluctuations, mortality and polygamy rates, and the like. Rather, one might see these documents as relevant primarily for developing individual biographies of the deceased persons and their families; examining their private use of objects (furniture, outfits, books, etc.); and determining how the deceased related to others through the complex system of credits and loans. The main challenge could well be in studying the *tarikāt* documents as a totality—that

is, in making the assumption that these documents could be approached as texts endowed with meaning, an approach that could also be helpful in understanding what judicial writing is all about.

Having bypassed the textual analysis of individual documents, Establet and Pascual create an enormous problem of contextualization of all their material: Each fact is first torn from its original source document (the “context”), then assigned as a variable to a data-base field, and finally associated within a broader statistical pattern or regularity. In such an approach, it is the broader statistical regularity rather than the document itself that creates the context for the fact—a major methodological flaw that pushes Establet and Pascual toward attempts to integrate their data and findings with other studies from Ottoman history. But this endless search for contextualization and meaning from outside the source documents is not that rewarding, in the final analysis.

There are certainly many findings in the book and interesting correlations between the authors’ results and the results of other researchers in the field (Raymond, Glasman, Rafeq, Masters, Marcus, Jennings, etc.). The major weakness in the work is that no attempt at a systematization of any kind has been pursued, and all the “explanations” provided for the “trends” (or “patterns”) seem disjointed from one another, giving the work an unmistakable feeling of fragmentation. One wonders, for example, whether Establet and Pascual’s correlations and comparisons between their micro-results for the period between the years 1686 and 1717 and other more global studies for the 17th and 18th centuries prepare for a “method” of analysis whose aim is to bridge the gap between the micro level of the shari‘a courts and broader trends for the rest of the Ottoman Empire. But Establet and Pascual never make such a claim, and, despite their numerous references to Braudel’s voluminous *Civilisation matérielle*, they do not seem much interested in his concept of “world economy” (*économie-monde*), or even that of “material civilization” itself, even though all of what they have been describing in their book is the “material civilization” of an Ottoman city. The absence of such an integrative framework makes it even more difficult to make their micro-shari‘a-court findings coherent with respect to more global trends. It is as if the correlations from other studies that Establet and Pascual associate with their own research are there to show that their research is plausible, and that their results make sense. This is not enough to create much historical meaning.

But even a concept of “world economy” would turn upon itself in a movement of total emptiness were it not associated with a concept of “political economy” at the regional level of the documents themselves. What is badly needed is a concept of “political economy” for a “non-disciplinary” society in which kinship (*qarāba*) and socio-professional and religious groupings (*ṭawāʿif*) are crucial, but such an enterprise could not be done properly without massive recourse to the *fiqh* literature for “property” (*mulkiyya*), “money” (*māl*), “value” (*qīma*), and other such concepts.

Another problem is the intentionality of the actions of the social agents themselves, and how those scattered individuals end up being part of a “society” or a “city”—or, in other words, how do they relate as individuals whose actions are constrained by the objective structures of their society? (The general “trends” discovered by the circle of Ottomanists on whom Establet and Pascual heavily rely could be associated with those “objective structures” that structure individual actions.) In Establet and Pascual’s study, the individual actions of the people of Damascus always amount to vectorial sums that seem to fit well with broader trends found elsewhere in the empire. The problem, however, is, as always in the social sciences, to see how these “objective structures” correlate with the individual actions of the social actors. Why is it that most “rules” are only partially followed, and what is it that individuals have in mind when they decide to be polygamous, or to have only one child rather than the “average” of three?