

I

Quantitative Approaches in Research on Ottoman Legal Practice

Our book represents a primarily (though not exclusively) economic and quantitative exploration of Ottoman legal practices based on the court records (sing. *sicil*) of eighteenth-century Kastamonu. We will have opportunities in the rest of this book (specifically, in Parts III and IV) to explain our economic approach to Ottoman legal practice. Here, we discuss why a quantitative orientation toward the court records, aided by non-quantitative approaches and tools of analysis, may be a rewarding one. We accomplish this task first by engaging with existing debates over the best way to analyze Ottoman court records, providing a (belated) response to the arguments against quantitative methodologies. Second, we suggest, with the help of ideas proposed by some of the leading representatives and defenders of quantitative history, how a quantitative orientation may benefit legal historiography in the Ottoman context.

HOW TO STUDY OTTOMAN COURT RECORDS: A METHODOLOGICAL DISCUSSION

The documents housed in court archives reflect the many functions served by courts in the Ottoman Empire. As attested by previous research, Ottoman courts did not function solely as arenas of litigation. They also operated as public notaries and court officials, in particular the *kadis* (or, in their absence, *naibs*, the deputy judges) held administrative responsibilities within their jurisdictions. Thus, in addition to hearing and resolving disputes, court officials recorded contractual agreements in the court ledgers, appraised and divided estates among heirs, received and conveyed government orders to the local populace, supervised the assessment and

14 Quantitative Approaches in Research

collection of local taxes, and participated in provincial administrative and decision-making processes alongside other government functionaries. The content of court archives ranges from litigation summaries to settlement extracts to contractual arrangements, from probate estate inventories to tax records to imperial orders.

Interest in Ottoman court records is not new. Since the 1960s, historians have used these documents primarily for social and economic research, treating them as, in Iris Agmon's phrase (2004b, 173), "a pool of information." Based on this material, historians have learned valuable information about aspects of the society not directly discernible from other types of sources. Court records have helped shed important light on Ottoman material culture, mercantile and commercial activities, wealth levels and distribution, and provincial administration. More recently, scholars have begun using *sicils* to explore how the law was interpreted and practiced in different locales and periods, although this legal focus is still less common than socioeconomic research.

Despite the varied methodological approaches to court records employed in such works, we lack a fully developed critical literature on the relative merits of these methodologies. Given the fact that the court records contain millions of documents on every aspect of Ottoman life for virtually every period and location in the Empire, it is essential for Ottoman *sicil* researchers to think carefully about how they explore this very rich source base. This is what we aim to accomplish in this chapter: We aim to contribute to existing methodological deliberations by engaging the ideas of those few colleagues who have made important observations on the topic, which remain influential and widely acknowledged in *sicil*-based research.

In a 1996 review of Colette Establet and Jean-Paul Pascual's quantitative analysis of Damascene estate inventories (1994), Zouhair Ghazzal appears to have been the first to raise the question of how to study the material found in *sicils*. Ghazzal's later correspondence with André Raymond (1998) in the pages of the *International Journal of Middle East Studies* helped him clarify a number of his original assertions, in particular, his criticism of quantitative approaches. It is, however, Dror Ze'evi who, in an article published in 1998, offered the first comprehensive and systematic discussion of alternative research orientations used by *sicil* researchers. Later, Iris Agmon would take on the topic in multiple studies (2004b, 2006; Agmon and Shahar 2008) and make a number of critical observations. Ghazzal would return to the issue in his 2007 book, in part to reassert and elaborate on some of his original

arguments. In what follows, we critically engage the claims in these works regarding quantitative techniques in the study of Ottoman court records.

Although Zouhair Ghazzal's review of Establet and Pascual's book came first, we begin our discussion with a treatment of Dror Ze'evi's article, which remains one of the most-cited studies in the scholarship on Ottoman court records due to its explicitly methodological orientation, and continues to shape how researchers approach *sicils* as a historical source. According to Ze'evi (1998, 38), the scholarship on Ottoman court records exemplifies "three basic categories or techniques, according to the methodology used." These are quantitative history, narrative history, and microhistory. Narrative history, as Ze'evi describes this "technique," is a fairly common approach in *sicil* studies and consists of "(1) examining the material in the court ledgers referring to the time-frame defined, (2) finding the strands that can be woven into a story, and (3) attempting to recreate a logical sequence of events" (1998, 45). The potential danger of this approach, according to Ze'evi, is the possibility of generating "a false story or a false chain of events" based on a selective and impressionistic reading of the court's archive, which is treated as a "fact quarry." Often, Ze'evi argues, *sicil* researchers "tend to discover a narrative at an early stage [of their research], sometimes by connecting two or three haphazard examples, and then try to look for evidence to substantiate [their] claim[s], while ignoring the pieces of the puzzle that do not fall into place."¹

Another approach that Ze'evi identifies is the "microhistorical approach," which he defines as focusing "on a single detailed recorded incident, or on a series of records referring to the same narrowly defined space and time" (1998, 47). However, since microhistorical research entails an examination of detailed documentation on very narrowly defined historical phenomena (such as single event or small numbers of related incidents), court records are not particularly suited for this type of analysis. This is because these documents often lack detailed information on the circumstances surrounding the particular event(s) or the mindsets of the individuals involved (Ze'evi 1998, 48). It is also unusual

¹ Iris Agmon (2004b, 192) considers Ze'evi's description of "narrative history" in *sicil*-based research to be "vague." Although she agrees that it is common for *sicil* researchers to extract small pieces of information from court records to support their narratives in an arbitrary fashion, she considers what Ze'evi calls "narrative history" a rhetorical strategy rather than a methodology.

16 Quantitative Approaches in Research

to find a multitude of documents pertaining to a single event or individual.²

Ze'evi's most comprehensive critique of *sicil* studies pertains to the quantitative approach, which historians began to adopt in the 1960s and 1970s. He suggests that “statistical processing of *sijill* data may seem the most natural use to which we can put these sources” (1998, 39). This is because

the ledgers of the court offer great numbers of ostensibly similar cases over a period of several years. For some types of records – marriages, inheritances, sale deeds – one can easily assemble a voluminous database. What could be more precise than collecting several hundreds of, say, marriage records, and processing the data – the dower paid, the origins of the bride and groom, their social status and so on?

However, Ze'evi questions the reliability of these studies for two main reasons: “First,” he suggests

we have very few clues as to the representativeness of our sample. Seldom do we have any kind of material indicating who came to court and why. . . . The extent to which the sample obtained from court records represents any part of society is an enigma. The kinds of graphs and tables demonstrating social stratification, for example, so popular in quantitative research, might therefore be skewed to such an extent that they only represent the sample itself. (1998, 39–40)

Next, he questions (1998, 40) the ability of researchers with quantitative inclinations to accurately interpret the information found in their sources: “No less problematic for the quantitative mode is the relationship between the written record, the ‘reality’ it speaks about, and prevailing cultural norms. Does the record give actual prices, real values, or is there some other reasoning that determines these values?” Since the court records are opaque sources at best, which do not readily reveal the practices and thought processes that produced them, they may be deceptive. Indeed, Ze'evi insists (1998, 43), “we know almost nothing about the courtroom strategies that lie behind the *sijill* façade. . . . Without such knowledge we have difficulties interpreting the records, and should not accept them at face value.”

Ze'evi admits that many researchers are aware of these problems. In this regard he cites Colette Establet and Jean-Paul Pascual's (1994) use of the probate estate inventories of eighteenth-century Damascus,

² According to Agmon (2004b, 194), “full-fledged microhistory was hardly employed in the field” at the time that Ze'evi published his article and since then; for an exception, see Peirce (2003).

a study in which the authors explicitly address the shortcomings of their research, particularly the uncertainty regarding the representativeness of their sample.³ While Ze’evi calls Establet and Pascual’s analysis “meticulous,” the fact that their study does not satisfactorily answer the problems that he raised about quantitative research based on court records leads him to question the entire purpose of this endeavor: “[I]f that is the case [that is, if it is not clear whether the sample that Establet and Pascual’s study is representative of the entire population] . . . what is the point of such meticulous work? A non-statistical, impressionist approach would have served equally as well” (1998, 44; we return to the implications of this quote at the end of the chapter).

As mentioned, Dror Ze’evi was not the first scholar to criticize Establet and Pascual’s study for methodological reasons. In 1996, two years before the appearance of Ze’evi’s article, Zouhair Ghazzal published a review of Establet and Pascual’s work, in which he criticized the authors for failing to properly contextualize the estate inventories (as found in the court records), which overlaps with Ze’evi’s second, more epistemological criticism directed at quantitative research:

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. (1996, 432)

Ze’evi and Ghazzal’s criticisms should be seen as related to what Agmon has called the “cultural turn” in *sicil* studies since the mid-1990s (Agmon 2004b, *passim*; 2006, 32), which, under the influence of the deconstructionist trends in the 1970s and 1980s (Agmon 2004b, 180), aimed to “turn court records themselves into an object of historical investigation, both as a means for a better understanding of the societies that produced them, and as a result of the understanding that court records are cultural products of these societies” (Agmon 2004b, 191). This approach, popularized by those who favor subjecting court documents to textual analysis and problematizing the processes that produced them, refuses to treat the court records as transparent windows to the past, taking them instead as social, political,

³ Establet and Pascual’s book (1994) examines about 600 probate estate inventories from late seventeenth- and early eighteenth-century Damascus to understand economic characteristics of the urban population in one early-modern Ottoman community.

18 Quantitative Approaches in Research

and legal artifacts that need to be understood in their own terms and contexts.

We find nothing objectionable about demanding a contextually sensitive approach to the Ottoman court, its operations, and records. In fact, we believe that such efforts are bound to produce better, more historically grounded analyses of the material in court records. Nevertheless, we also feel obliged to raise a few points in regards to Ze’evi and Ghazzal’s critical comments on quantitative approaches since they have become influential in the wake of the “cultural turn” in *sicil*-based research. In what follows, we first provide methodological responses to Ze’evi and Ghazzal’s criticisms and then discuss how a quantitative approach can contribute to Ottoman legal history-writing.

IN DEFENSE OF QUANTITATIVE HISTORY BASED
ON SICILS

The task of deciphering “reality” from the written record is a significant problem for *sicil* researchers since Ottoman court records are notoriously terse and formulaic in their descriptions of the court’s actions and court clients’ affairs. In this sense it is difficult to fault Ze’evi for pointing out the discrepancies between anthropological and *sicil*-based representations of legal practice. However, it is curious for Ze’evi to raise this point in his critique of quantitative research, as if it were only quantitative analysis that has the potential to be distorted by the limitations of the source base. To be clear, all methodologies (including narrative and microhistorical approaches) are vulnerable to this problem and there is no easy way to avoid it. *Sicil* researchers must constantly revisit their assumptions and question their own readings of the court records. As Ze’evi suggests, a comparative and cross-disciplinary orientation might help them to develop an alternative, initially counterintuitive interpretation of the sources, and would depend on to their becoming familiar with scholarship beyond the Ottoman context on legal phenomena and the human interaction that surrounds it. And yet the problem that needs to be tackled and resolved by *all* scholars pursuing *sicil*-based research may have little to do with the methodological choices associated with quantitative analysis.

True, Ze’evi’s complaints about the representativeness of the samples used by quantitative scholars are relevant to the type of research they conduct. However, his objections in this regard do not constitute so much a critique of quantitative history as a research orientation per se as they do a critique of poor quantitative history-writing. One does not have to be

a proponent of textual analysis to complain about sample representativeness or to question to what extent a dataset (e.g., one based on the estate inventories that Pascual and Establet study) can act as a proxy for the phenomenon (e.g., wealth levels at times of death) that the historian claims it stands for. In fact, these two often constitute the most common topics of discussion and disagreement among quantitative researchers.

However, when such researchers deliberate on these topics, they often discuss them with reference to *specific* studies and/or findings. They debate whether a specific quantitative study has produced sound results and/or plausible conclusions. They question whether particular calculations are likely to misrepresent the “reality” that they try to approximate. They wonder how such problems influence their conclusions, and consider the best ways to remedy them. It is such difficult, nitty-gritty technical work that requires quantitative expertise that truly improves the researchers’ collective understanding of their resources and the historical implications of their research.

On the other hand, by attributing shortcomings to an entire genre of research (“quantitative history based on Ottoman court records”), rather than specific examples of it, Ze’evi shirks his responsibility to approach the topic constructively – that is, by carefully engaging individual studies on their own terms, by telling his readers why and to what extent their results may be defective, and by making suggestions for improving or correcting them. Is it the case that *all* quantitative studies based on court records are susceptible to the problem of representativeness? Given the large corpus of such studies, it is curious that the only work other than Establet and Pascual’s book that Ze’evi (1998, 40, fn. 12) explicitly identifies as flawed is a short and self-consciously tentative book chapter by Judith Tucker (1991), in which the author makes observations on marriage patterns in eighteenth- and nineteenth-century Nablus, based on 107 marriage registrations as found in nine *sicil* registers spanning about 65 years.⁴

⁴ One wonders, for example, what Ze’evi has to say about Ronald Jennings’ largely quantitative study on Ottoman women (1975), which Ze’evi himself cites at one point in his article (1998, 36, fn. 2). Jennings’ study examines more than 10,000 entries from 12 court ledgers of Kayseri that cover about 25 years from 1600 to 1625. Within this sample, Jennings identifies about 1,800 entries that demonstrate the multiple ways in which women participated in court proceedings, and subjects them to detailed statistical analysis. This is quite a large sample for an urban center with a population of about 20,000 individuals in the early seventeenth century. In addition, Jennings offers in his discussion valuable clues about the contents and shortcomings of the court records, allowing the reader to reach her own conclusions about their representativeness. Finally, the analysis of the material in Kayseri court ledgers is complemented by Jennings’ observations on

20 Quantitative Approaches in Research

Moving on to the second (epistemological) critique of quantitative research based on *sicils*, we find in Ghazzal's objections a more articulate justification of the doubts regarding the credibility of quantitative history. According to Ghazzal, as he explains in his response to André Raymond's rebuttal of his original review,⁵ the main problem with subjecting estate inventories (and, by extension, other types of court records) to a quantitative analysis is that the economic and/or demographic orientations associated with this type of research are inherently inappropriate for these sources:

Establet and Pascual confront each document with an a priori agenda and with categories that did not emerge from a questioning of probate records. Thus, for example, *tarikāt* [pl. for *tereke*; probate estate inventories that Pascual and Establet explore] records were obviously not drafted by judges and their scribes in order to keep track of currency fluctuations, or the age of deceased individuals, or their sex and occupation for that matter. Our two historians decide on an a priori basis and in a line of reasoning *exterior* to the documents themselves that such questions and statistical categories are essential for an understanding of these societies. (emphasis in original, 1998, 474)

Instead, Ghazzal argues, “[w]hat is badly needed is a concept of ‘political economy’ for a ‘non-disciplinary’ society in which kinship (*qarāba*) and socio-professional and religious groupings (*tawā’if*) are crucial, but such an enterprise could not be done properly without massive recourse to the *fiqh* [jurisprudence] literature for ‘property’ (*mulkiyya*), ‘money’ (*māl*), ‘value’ (*qīma*), and other such concepts” (Ghazzal 1996, 432; cf. Agmon and Shahar 2008, 13). Ghazzal provides a more comprehensive discussion of this topic in his recent book, *The Grammars of Adjudication* (2007). Here as well, Ghazzal accuses (2007, 4) the existing literature on Ottoman socioeconomic history of being anachronistic:

It is generally assumed that a legal system operates in conjunction with a socio-economic one. Yet ... the encounter with an “economic” sphere as such is ... problematic. In fact, and this should come as no surprise, there were no

women's involvement in legal processes in other urban centers in Anatolia (Amasya, Karaman, and Trabzon) during the same period, which is invaluable in documenting contextual variations. While it is possible to doubt some of the findings and conclusions Jennings offers in this article, it is difficult to dismiss the study in its entirety just because it is quantitative.

⁵ Ghazzal's criticism of Establet and Pascual's work led André Raymond, a mentor to Establet and Pascual, to respond two years later (1998). In this rebuttal, Raymond attempted to make a case for quantitative methodology in the study of Ottoman court records, without necessarily addressing Ghazzal's methodological concerns (1998). Also see Agmon (2004b, 190) and Agmon and Shahar (2008, 12) for a discussion of this exchange.

In Defense of Quantitative History Based on *Sicils*

21

autonomous discourses of “economy” or “political economy” in Ottoman societies. If “economics” is a form of social inquiry peculiar to capitalist societies, then it is indeed no surprise that Ottoman societies failed to produce such a literature. Yet, much of contemporary Ottoman historiography has specifically centered on what is labeled as the “socio-economic” without addressing the fundamental issue of a lack of an indigenous literature on the “economic.” But considering the impossibility of such a literature, the issue then becomes of whether the “socio-economic” could be addressed on its own, as if endowed with its own rationale and mode of existence.

According to Ghazzal (2007, 4), “not only the ‘economic’ could not be addressed outside the ‘legal,’ but both spheres, in turn, cannot co-exist outside the discursive totalities of Ottoman societies.” The respective roles of the economic and the legal may be considered only “in conjunction with the religious, moral, political, and hence with the linguistic components” of the society (Ghazzal 2007, 5). This is where the *fiqh* becomes relevant for Ghazzal:

Considering that Ottoman societies did not produce an economic literature as a form of inquiry, by contrast the *fiqh* as a form of jurisprudential knowledge could pose itself as a domain whose mode of inquiry was total, even encompassing an implicit economic rationale. To begin with . . . scholars who worked with the domain of the *fiqh* did so on the basis of a total experience, and with an awareness that both the juridical norms and the customary practices of a particular society could only be formulated within a linguistic framework, one which in effect begins with God’s discourse (*khitab*) as a source of normative rules and open for interpretation through the multifaceted enterprise of the *fiqh*. It is within such a framework that property rights and contractual rights would find their place – one where *ex cathedra* rights had a prime importance. But property and contracts notwithstanding, the “economic” would have to be formulated through the juridical norms of the *fiqh* and the process of judicial decision-making.

For the purposes of our book, Ghazzal’s points constitute a methodological challenge since they emphasize how quantitative approaches, shaped by assumptions that originate from specific (Western?) academic disciplines (in particular, economics and demography), distort the findings of *sicil* researchers who attempt to utilize them because these assumptions are not appropriate to explore the material in Islamic legal sources. On our part, there are two ways to respond to this challenge. One is to show that the techniques (quantitative or otherwise) that we utilize in this book are based on a thorough understanding of the contents of our sources and the processes that produced them, despite our interest in the research orientation associated with the Law and Economics scholarship. We accomplish this task in subsequent chapters. The second one is to critique Ghazzal’s reification of the *fiqh*, which is what we attempt in the

22 Quantitative Approaches in Research

rest of this subsection. Before we do so, however, we should point out that such reifications of specific aspects of Islamic culture are common tendencies among Islamic researchers who tend to doubt most attempts of comparative and interdisciplinary scholarship that involve aspects of their fields of study. In this sense the arguments we make below have broader implications than what may initially appear to our readers.

The Grammars of Adjudication, where we find an elaborate characterization of Ghazzal's views on the topic, is a fascinating discussion of Ottoman law that draws on the work of Foucault and Geertz. Because, according to Ghazzal, the *fiqh* discursively and substantively shaped every aspect of life, a “thick description” of most aspects of Ottoman culture, be it legal, economic, or, political, can be produced only by decoding its relationship to jurisprudence. That is, by figuring out how it was constituted by and also came to reflect the norms of the Ottoman *fiqh*. In this sense, according to Ghazzal, the *fiqh* offers a language that should be used to decode the culture because it encompasses its essence.

There is no question that a competent understanding of the legal theory is necessary for a researcher to conduct research based on court records. It is, however, one thing to insist that an adequate understanding of the legal terminology and concepts is necessary for a competent use of historical documents, which is consistent with the analysis presented in this book; it is another to suggest the transcendent influence of the *fiqh* on other aspects of Islamic life and civilization, or to claim that only a primarily *fiqh*-based approach to court records can provide an accurate understanding of *all* layers of their historical meaning. A call for a methodology that is not “exterior to the documents” imagines the constitutive power of the *fiqh* as such, conceives of an Islamic “economy” or “politics” or “sociology” that is *sui generis*, in the way that certain interpretations of Foucault and Geertz's thought characterize societal and historical structures largely as embodiments of discursive practices and cultural meanings. It is unfortunate that Ghazzal does not elaborate on the unique characteristics of Ottoman economic thinking or demonstrate precisely and empirically how the researchers that he criticizes as being uninformed misunderstand or mischaracterize it.

It is also unclear how one can *prove* that legal/economic/political knowledge and practice were solely or even mostly a product of jurisprudence. Nor does Ghazzal elaborate on the limits of this relationship. In the case of court-prepared probate estate inventories, what about contextual variations in the legal and economic concepts that Ghazzal might be unaware of? What about the role of customary practices that are not