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A REPLY TO TIMUR KURAN

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Scholars are justified in complaining that Ottoman studies, from its maturation in the 1950s until today, has been far too integrated into social science disciplines. Traditionally, political and economic history has dominated the field at the expense of cultural history, literary studies, and the history of art, architecture, and material culture. The recent juncture of social science concerns and the sensitivities of the humanities is a welcome but long overdue development. Social science disciplines have long held sway over the field, but scholars of Ottoman history have always exercised rigor and meticulous care in editing and publishing historical documents and literary texts. Editors often discuss the literary, philological, and codicological problems of historical documents in great detail, the conceptual universe in which the documents exist, and their limits and possibilities. Of course, "editing" often includes textual criticism, the conscientious work of transliteration, and, sometimes, translation. Despite numerous invaluable editions of archival and nonarchival material drawn from the Ottoman centuries, we still lack substantial statistical information and data to answer major questions that historians and social scientists have long asked. In this respect, what Timur Kuran (and his team) initiated marks an important step. They published a massive compilation of ten volumes of documents selected from the two qadi courts of Istanbul during the 17th century.

In the August 2015 issue of *IJMES*, I briefly reviewed this compilation and raised questions about its contents. Kuran's response to my review appears in the present issue. Here I would like to further extend and clarify my critiques. My criticism of the compilation assumes three main orientations: first, the method used to exclude some documents and include others; second, the way the editor employs certain historical categories; and third, specific problems in editing, such as the presentation of certain dates, documents, categories, and translations. I will focus my criticism on the method and use of historical categories, and will not repeat or elaborate on what I wrote in my original review concerning this third problem.

In my review, I argued that there were no clear methodological guidelines explaining why the editor selected the fifteen *defter*s and not others from the courts of Istanbul, and why he included certain documents but not others. In other words, how did the editor decide what to include and exclude? In his response, Kuran sought to explain why and how the team, with the assistance of a group of scholars from the Islamic Research Center in Istanbul, picked these fifteen codices. This explanation is very helpful; I wish it had been available in the compilation's introduction. Without such an explanation, there is no way for readers to know how and why the editors made the decisions they did. My other criticism of this method mainly comes from the following paragraph:

> Our twelve volumes [vols. 11 and 12 had not been published when I wrote the review] do not cover all of the 10,080 cases found in the 15 registers. Rather, they cover subsets of these cases that *concern subjects of particular interest to economic and political historians and that are especially*

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likely to provide new insights into the workings and dynamics of the Ottoman economy [italics are mine]. Non-commercial cases are left out entirely. Also omitted are entries focused on routine matters of sale, inheritance, marriage and debt. (vol. 1, pp. 51–52)

Here, Kuran explains how and why the editor and his team chose to exclude around half the documents in the fifteen *defter*s. As I argued in my original review, this framing is problematic. How can we be sure that "routine" matters, such as sales, inheritance, marriage, and debts, would not interest economic and political historians? What is a "routine matter"? One matter might be routine for one scholar, but unique for another. Even if a particular subject is routine, this does not render it unimportant for understanding certain problems, including what is routine and what is not routine. Or, how can we be sure that, for example, matters of inheritance, marriage, or debt are not at all related to "state–subject relations" or "communal matters," which are the titles of two of the volumes? In his response, Kuran writes that their "goal was to exclude cases involving ordinary interpersonal transactions (as opposed to communal matters)." What, precisely, are "ordinary interpersonal" matters as opposed to "communal" ones? I do not think an interpersonal–communal division for qadi court records is, by any ones means, useful. Simply the phraseology in the text, the personal names, gender, family connections, or identities of the parties or witnesses, could offer substantial insights into how the community operated. Above all, a qadi court is a communal arena (one might argue that all cases in the qadi court are public), even if the documents are related to "personal" matters. But even if we accept the editor's binary—interpersonal versus communal—readers must understand, in great detail and with the help of analytical terms, how he established this division.

When an editor claims to present information from primary sources that will concern economic and social historians, substantive and analytical reasons should be given for what exactly the collection includes and excludes. The editor's parameters for inclusion/exclusion pose a major risk that the statistical analyses we see in each volume's introduction would be problematic and vague.

My second major critique concerns the way that certain categories, such as state officials (*devlet görevlileri*), are used. In the early modern period, there was not necessarily a clear boundary separating state from society, or a state official from someone who was not a state official. Office holders, scribes, holders of tax-farming contracts, waqf trustees and intendants, janissaries, qadis, and their proxies (*na'ib*s, etc.) provide us with a great spectrum of individuals who, in one way or another, were involved in what we can call the business of governance. Some were organized in a hierarchical fashion, while others were not. In the 17th century in particular, with the expansion of mechanisms of outsourcing and tax farming, the division between state and society, or governance and business, became especially blurry. The old term *askeri* did not refer exclusively to "state officials," but to holders of certain privileges. Furthermore, should we consider a *nazır* of the waqf of the Ottoman dynasty as a state officer, or as an agent of the imperial waqf administration? How should we consider the relationships between imperial waqfs and the imperial bureaucracy? How to interpret the janissaries? Was a janissary a state official or part of the corps, which was heavily integrated into the markets and guilds of Istanbul? A janissary could appear in the court as a soldier or, sometimes, as a guild member. But even when a janissary appeared as a guild member, he might have

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benefitted from his janissary identity, as part of the military establishment. Therefore, any unqualified division between state and society, similar to any unqualified division of the individual and the communal, will not always and transparently be meaningful in the Ottoman context as in many other premodern contexts. To tackle the ambiguities of statehood, perhaps the editor might consult Gilles Veinstein's article, "Sur les nâ'ib ottoman (XVème-XVIème siècle)" (*Jerusalem Studies in Arabic and Islam* 25 [2001]: 247–67).

Finally, I would like to return to the relationship between the social sciences and history in Ottoman studies. As I mentioned in the first paragraph, the current dialogue and fruitful exchange between historians and social scientists in Ottoman studies is greatly encouraging. Many young historians in the field today, including myself, have a background in the social sciences. We should contribute rigorously to the intensification and expansion of interdisciplinary initiatives. Massive research enterprises will only come through genuine dialogue, criticism, and collaboration. The compilation edited by Timur Kuran and his team is a valuable contribution. Therefore, it merits genuine critique. I hope this exchange between Kuran and myself will trigger fresh discussions about how to approach historical sources in a way that is mindful of the questions posed by the social sciences and beneficial to a broader public of scholars and about how we might use these sources without ignoring their conceptual universe and the historical categories as well as limits and possibilities they offer. These sensitivities should not be taken as the priorities of historians in stark contrast to scholars in the "analytical social sciences." On the contrary, they constitute analytical concerns that all of us should share.

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