

Quantification and the Economic History of Imperial Russia

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Historians work with sources that are products of specific social, cultural, political, and economic contexts. Thus, understanding how and why sources were produced and why they survived is an essential component of historical scholarship. At the same time, many historians often employ some sort of conceptual framework—implicit or explicit, descriptive or normative—in order to translate the sources into a coherent narrative. Modern economic historians are no different. The sources tend to be quantitative and focused on economic phenomena (with many exceptions), but doing economic history well means interrogating the origins, trustworthiness, and usefulness of the data in question. In doing this, modern economic historians are largely unapologetic about employing the tools—especially statistical—and intellectual apparatus of economics to interrogate their sources, much as social, political, or environmental historians draw on ideas and methods from related disciplines in their own inquiries.¹ This is precisely how we make sense of the historical process of economic development.

In his essay, Alessandro Stanziani takes issue with the application of modern social science methods to research on late Imperial Russian economic development. He argues that the enormous amount of quantitative evidence that contemporary Russian observers produced has been improperly utilized because the early statisticians collecting such data were doing so with specific political or sociological agendas and without access to the empirical tools and conceptual models employed by social scientists today. In this reading, much of the quantitative material produced in late Imperial Russia manifested “opinion”—rather than facts—about how the economy functioned. Therefore, according to Stanziani, scholars who rely on modern analytical methods with these Imperial data tend to ignore the underlying historical context in which they were produced. This leads such scholarship to incorrect conclusions, because these historical sources are not objective representations of the real state of the Imperial Russian economy.

In making this argument, Stanziani finds fault with generations of scholarship that have relied on statistical evidence from the Imperial period, particularly the astounding materials produced by *zemstvo* researchers. Stanziani is critical of Soviet quantitative history and the work of modern economic historians, including several of my own papers on the Imperial economy and an important new study by Andrei Markevich and Ekaterina Zhuravskaya

1. For thoughtful discussions of scholarly practices in modern economic history, see the following papers in the *Journal of Economic History* 75, no. 4 (December 2015): William Collins, “Looking Forward: Positive and Normative Views of Economic History’s Future”: 1228–33; Kris Mitchener, “The 4D Future of Economic History: Digitally-Driven Data Design”: 1234–39; Naomi Lamoreaux, “The Future of Economic History Must Be Interdisciplinary”: 1251–57; and Ran Abramitsky, “Economics and the Modern Economic Historian”: 1240–51.

that investigates the economic impact of serfdom.² However, Stanziani does not appear to take issue with any of the substantive conclusions of these and other studies. For example, it is not clear whether Stanziani actually finds fault with Markevich and Zhuravskaya's identification of serfdom's large and negative impact on the Russian economy prior to 1861. Furthermore, Stanziani is also non-committal as to any specific directions to the biases in zemstvo or other data from the period that he perceives.³ Rather, his argument is really formulated against what he sees as the methodological faults of modern economic historians, especially those that draw explicitly on the statistical tools and theoretical concepts of economics. In this way, his criticism does not seem isolated to Russian economic history, as his main assertions could be made regarding analyses of *any* historical economy.⁴

As a card-carrying economic historian trained in economics and working on Imperial Russia, I do find much to agree with in Stanziani's study, particularly as he describes the motivations and innovative practices of the statisticians of late Imperial Russia. These aspects of the paper echo similar arguments made by Theodore Porter and others regarding the development of empirical social science in Europe over the long 19th century, and they parallel the findings in other works on Imperial Russia by David Darrow and Stanziani, himself.⁵ The intellectual history that Stanziani describes—both higher-level debates over statistical methods and practical issues regarding the execution and interpretation of rural surveys—is both fascinating and important for contextualizing the data generated in the period. As he and others note, Imperial Russia was a foundational environment for much early social scientific research, particularly as to how the peasant economy functioned, but contemporary researchers certainly held opinions that guided their conclusions and, possibly, their data collection efforts. I fully agree with Stanziani that every

2. These studies include Andrei Markevich and Ekaterina Zhuravskaya, "Economic Effects of the Abolition of Serfdom: Evidence from the Russian Empire," (February 1, 2016), at www.ssrn.com/abstract=2514964 (last accessed January 24, 2017); Steven Nafziger, "Peasant Communes and Factor Markets in Late Nineteenth-Century Russia," *Explorations in Economic History* 47, no. 4 (October 2010): 381–402; A.M. Anfimov, *Krest'ianskoe khoziaistvo Evropeiskoi Rossii, 1881–1904* (Moscow, 1980); and I. D. Koval'chenko, *Russkoe krepostnoe krest'ianstvo v pervoi polovine XIX v.* (Moscow, 1967). Several of the Soviet works Stanziani mentions do *not* draw upon the late-19th century sources, especially zemstvo materials, that he is primarily concerned with. I address several additional difficulties with how Stanziani interprets specific scholarly works below.

3. He notes that his "aim here is not so much to criticize this [*sic*] data, per se."

4. Stanziani notes how late Imperial policy makers and local officials were especially intent on developing a deeper knowledge of the rural and peasant economies. The advances in statistical methodologies and the sheer amount of resulting quantitative evidence subsequently generated—certainly greater than other comparable economies of the time—suggests that this was the case.

5. Theodore Porter, *The Rise of Statistical Thinking, 1820–1900* (Princeton, 1986); David Darrow, "Statistics and Sufficiency: Toward an Intellectual History of Russia's Rural Crisis," *Continuity and Change* 17, no. 1 (May 2002): 63–96; and Alessandro Stanziani, *L'économie en révolution: Le cas russe, 1870–1930* (Paris, 1998), along with a number of his other works. Stanziani also references numerous specialized studies (in various languages) of the intellectual backdrop to the zemstvo statistical research efforts.

historically-minded scholar—whether interested in Russia or not—must take care to understand how and why one’s data sources came to be.

Much of this account of early Russian social science, however, is not terribly helpful for understanding how the Imperial Russian economy—its institutions, markets, firms, workers, and policies—actually functioned and mattered for the pace and character of economic growth and development. Stanziani documents the history of Russian socio-economic statistical research as part of his larger argument that modern historians cannot view economic life in the past through the lens of modern economics, both in terms of theory and with regards to quantitative methods of analysis. I disagree with this stance for two sets of reasons, which I document in detail over the rest of this note.

The first has to do with the use of modern economic *theory* to conceptualize historical economic relationships and activities. Stanziani views such efforts as inherently biased towards forcing historical phenomena to fit modern ideas. But in making this claim, Stanziani largely misunderstands what the application of economic “models” actually entails for the modern economic historian. Diagnosing what is happening in an economy—historical or otherwise—is greatly aided by the use of simple economics to structure thinking and to generate falsifiable hypotheses that can be evaluated using the sources at hand. Many economic historians of Imperial and Soviet Russia (Paul Gregory, Mark Harrison, Tracy Dennison, myself, and others) are conversant with how contemporaries such as A. A. Chuprov and A. Chaianov viewed economic behavior, particularly among the peasantry. But their implicit and explicit models—and the ideas of other economic writers of the time—can and should be embedded in modern micro and macroeconomic frameworks, thus enabling comparability to other contexts and allowing for rigorous empirical work.⁶ Historically-inclined social science—which Stanziani seems to be generally against—is all about employing theory derived in one context to frame and understand phenomena in the past.

When appropriately applied, such models of economic relationships are not intended to claim a monocausal account of what happened in the past, which is what Stanziani seems to think. Rather, they allow the economic historian to logically lay out the strands of a possible but falsifiable and certainly partial explanation for some observed phenomenon: agricultural productivity, labor mobility, firm investment, interest rate trends, and so on. Then it is up to the analysis of the historical evidence to determine whether that explanation has merit or not. I would argue that much historical research, economic or otherwise, follows something akin to this deductive approach, even if the “model” and the testing are implicit and not derived from the tool kit of modern economics. Of course there is room for more inductive approaches to

6. In the “Measuring Time” section of his paper, Stanziani emphasizes an analytical difference between Marxist/Chaianovian and “market” interpretations of the peasant household economy (in connection to how information on seasonal labor decisions was collected), emphasizing the difference in conclusions that could be drawn under the two models. However, work by *modern* development economists has shown how these two frameworks can be formally linked. See John Strauss, Inderjit Singh, and Lyn Squire, eds., *Agricultural Household Models: Extensions, Applications, and Policy* (Baltimore, 1986).

historical inquiry (let the sources speak, framework free), but I see little need to reject the “economist method” out of hand.

Many economic historians draw insights from the models and methods employed in “development economics” and in “new institutional economics,” because these sub-fields offer explanations for (often evolving) economic phenomena in low-income economies that obviously do not possess frictionless, perfectly competitive markets.⁷ These fields offer an arsenal of possible ways to conceptualize economic relationships in historical economies characterized by significant information asymmetries (between parties), high transportation costs, concentrated economic and political power, and non-“economic” sources of distrust and disbelief. They allow us to identify *possible* sources of efficiency gains or losses (the size of the economic pie), along with the associated winners and losers (who gets what from the pie), generated by specific features of the economy in question. In the Imperial Russian context, Stanziani asserts that, “economic historians and economists tell us which institutions limited the economic growth of Russia and which reforms should have been adopted. This kind of economic history, derived from economics, is highly normative.” I disagree with this assertion; economic history is *not* inherently normative. In making this claim, it seems like Stanziani takes issue more with some abstract notion of what economic historians do than with the conceptual and statistical methods employed (and the conclusions made) in any specific work.⁸ Based on the analyses that have been made of economic phenomena in Imperial Russia and elsewhere, however, I would argue that most modern economic historians employ theoretical ideas drawn from institutional economics and other fields to undertake *positive* analyses of past economic life.⁹ Yes, we might come to conclusions that have normative implications along particular lines, but this is only after clarifying the underlying economic factors at work in the historical context in question, by first positing an explanation, and then employing quantitative or qualitative empirical methods to accept or reject its applicability.¹⁰

7. On the use of the models and methods of institutional and development economics for understanding historical economies (and vice versa), see Avner Greif, *Institutions and the Path to the Modern Economy: Lessons from Medieval Trade* (Cambridge, 2006); Douglass North, *Institutions, Institutional Change, and Economic Performance* (Cambridge, 1990); Nathan Nunn, “Historical Development,” in Philippe Aghion and Steven Durlauf, eds., *Handbook of Economic Growth*, vol. 2A (Amsterdam, 2014); and the articles mentioned in Footnote 1.

8. Where Stanziani does criticize a specific study, he tends to be incorrect in his diagnoses, as I note below.

9. I suspect that Stanziani would disagree with this statement, but a quick skim through recent years of the flagship *Journal of Economic History* or similar outlets lends support to my claim.

10. For example, in my 2010 paper “Peasant Communes” (see note 2) on the interaction between property rights and rural factor market activity in late Imperial Russia, I draw on models of collective action and peasant household decision-making to formulate simple hypotheses regarding how households might react to exogenous demographic changes under the specific communal constraints postulated by Alexander Gerschenkron and others. I evaluated these hypotheses using *zemstvo* household data from Moscow province and found evidence inconsistent with this standard argument that the communal strongly inhibited peasant labor and land market participation.

The second reason that I disagree with Stanziani's criticism of the application of modern economic history to Imperial Russia centers on the use of the data generated in the period. I am largely sympathetic to Stanziani's cautionary account of the motivations and methods of 19th century researchers. Stanziani seems to also imply, however, that one must throw the baby out with the bathwater when it comes to employing Imperial Russian data with standard statistical tools. Again, this is not because he identifies specific inaccuracies or biases in the data (see note 3) or in the subsequent conclusions of particular studies. Rather, his criticism seems to amount to a series of mistaken interpretations of the empirical *methods* employed by economic historians using the data. This is partly grounded in his distrust of how economic historians bring to bear "models" in order to develop testable hypotheses. This also seems based on some larger difficulties with how Stanziani frames certain aspects of Imperial Russian statistical evidence and empirical work more broadly.

In general, I think it is valuable to employ available, possibly flawed historical data to produce a set of empirical findings regarding economic phenomena. These findings can then be probed and prodded to determine their validity in light of where the data come from, the problems of inaccuracy or unrepresentativeness they might have, and what we are trying to learn from them.¹¹ Some evidence is better than no evidence, and let us debate after we formulate some initial "facts." Of course, there are many ways that biases in the underlying data could generate inaccurate results, as Stanziani suggests. However, the job of the empirical researcher is to employ methods that take possible biases into account, whether by explicitly diagnosing the sign of the bias and correcting for it, or by using statistical tools to minimize its possible effects. For example, Stanziani argues that zemstvo researchers employed their own, often subjective, survey questions and methods in different provinces. Their subsequent findings often led them to extrapolate from very local results to broader, possibly biased conclusions.¹² In my work with zemstvo data, however, I have not only emphasized the *local* applicability of the findings, but also exclusively relied on the raw data in ways that limit exposure to potential biases in the collection effort.¹³ In works where I have analyzed data

11. Indeed, a paper I wrote with Tracy Dennison, "Living Standards in Nineteenth-Century Russia," *Journal of Interdisciplinary History* 43, no. 3 (Winter 2013): 397–441, had the explicit aim of exploring what the available data could tell us about 19th century living standards in two districts, using internal consistency across various indicators as an implicit check on the quality of the sources. While he seems bothered by our use of particular sources, Stanziani never questions our tentative conclusions about the trajectory of rural living standards. Moreover, the work was intentionally exploratory, an emphasis that Stanziani seems to have missed.

12. Stanziani's major example of the possible biases introduced by non-standard and subjective survey methods is from the well-known Tenishev *ethnographic* research program, rather than the more explicitly quantitative and likely more objective data collection done by zemstvo researchers. Moreover, the different responses he notes for similar questions across these (ethnographic) researchers can be interpreted simply as measurement error and addressed in an appropriate statistical framework.

13. For example, see Nafziger, "Peasant Communes," which restricts its analysis *and conclusions* to Moscow province. Stanziani (note 46) asserts that modern economic researchers—myself included—who have employed zemstvo data have been "a-critical" in their use of the data. Some of this complaint seems to assume that we rely on the zem-

across geographic units, the information in question—budgets, basic counts of people, land, and so on—was not subject to the types of biased collection efforts that Stanziani is concerned with.¹⁴ Moreover, using geographic variation in a multi-variate regression framework as a way to test a hypothesis actually leverages differences in reported economic behavior across space.

As was hinted at in this discussion, it appears that Stanziani misconstrues or misunderstands the empirical work of several recent studies in Russian economic history, including my own. For example, in discussing the paper of Markevich and Zhuravskaya, Stanziani frames as problematic their use of data, particularly with respect to their measure of emancipation-linked land reform (the share of outstanding redemption obligations paid in a year). However, Stanziani appears not to understand either the panel data methodology that Markevich and Zhuravskaya employ, or the particular use of the redemption data as an *instrumental variable*, that is, an indicator of the pace of the reform process that allows the authors to test for a causal linkage between land redemption and economic outcomes. The appropriateness of this variable as an exogenous source of variation in land reform is potentially questionable, but this is only a small part of Markevich and Zhuravskaya's paper. Stanziani largely ignores the significance or conclusions of their larger set of empirical findings, which (negatively) evaluates the economic effects of serfdom in a causal framework.

Stanziani concludes his study by asserting that combining more “accurate” studies of the data (of which he gives examples but no objective definition or outside verification) with historical studies of how the underlying information was produced will “enable real historical scrutiny of social and economic dynamics that can open our minds instead of supplying pre-determined answers.”¹⁵ Of course, there is value in carefully accounting for how and why the data we use was produced. But the use of economic theory and statistical methods by modern economic historians does not generate “pre-determined” answers, as I have tried to emphasize in this essay. Indeed, it seems like Stanziani's rejection of recent research in the field is subject to its own “pre-determination” caused by his misplaced dismissal of the methodological approaches of social-science history.¹⁶

stvo researcher conclusions, however, when in fact my work and the research of others have exclusively relied on the raw data (and have grappled with issues of how representative and possibly biased such data are). See my “Peasant Communes;” see also: Nafziger, “Serfdom, Emancipation, and Off-Farm Labour Mobility in Tsarist Russia,” *Economic History of Developing Regions* 27, no.1 (March 2012): 1–37.

14. For example, see Nafziger, “Did Ivan's Vote Matter? The Political Economy of Local Democracy in Tsarist Russia,” *European Review of Economic History* 15, no. 3 (December 2011): 393–441.

15. Stanziani cites the work of Steven Hoch as representative of “accurate” data usage—for example, see “On Good Numbers and Bad: Malthus, Population Trend, and Peasant Standard of Living in Late Imperial Russia,” *Slavic Review* 53, no.1 (Spring 1994): 41–75. While I admire Hoch's pioneering scholarship, there is little to distinguish its “accuracy” from other works of quantitative history other than that it tends not to employ the methods of economics. Moreover, Hoch's geographic coverage—and, hence, the larger applicability of his micro-level findings—is generally quite limited.

16. On other occasions—as in his apparent rejection of Paul Gregory's optimistic interpretation of late Imperial rural economic growth—Stanziani misunderstands what the

Alessandro Stanziani has written a fascinating historical account of the creation, usage, and meaning—both intellectually and politically—of economic statistics in late Imperial Russia. In accomplishing this, Stanziani is largely speaking to a different set of issues than most modern economic historians, myself included, tend to address. Unlike his concerns with the social context of data collection, our interests lie in documenting and understanding what actually transpired in the Imperial Russian economy. In doing so, myself and others utilize modern economic theory and empirical methods to help make sense of what we see in the data, often by explicitly taking advantage of the heterogeneity in economic indicators across households, estates, villages, firms, districts, and provinces. Economic historians are very cognizant of how “measurement error” in these data may or may not impact our findings, but this is an issue that afflicts all historical economies (and all non-experimental social science) and one that we have methods to address—indeed, that is the essence of statistical inference. Fundamentally, it is impossible to understand the economic development of Imperial Russia without carefully employing the methods of modern economics to analyze the astonishing wealth of empirical material generated in the period. Indeed, it is precisely this trove of quantitative information—*zemstvo* and otherwise—that makes the historical Russian economy such an important context for understanding the factors that limit and enable economic development, whether in the past or the present.

author has done. In that case, Stanziani confuses the *level* of rural incomes with Gregory’s emphasis on the *growth rate*.