explore the responses of the contemporaneous Ottoman, Russian, and Habsburg empires to the general crisis of the seventeenth century manifested in rural economic dislocation, political unrest, and banditry.

As this brief overview demonstrates, the contributors to the volume employ a variety of methodologies and approaches to the study of empire. While on the one hand this multifaceted approach serves as an introduction to a variety of models of inquiry, on the other it may leave the reader feeling somewhat at loose ends.

Despite the implicit claims to a *unité de problème*, coherence does remain something of an issue. For the historian, what is missing (except within several excellent individual essays) is a *unité de temps* that would allow one to pose questions about global connections and comparative world historical process across empires during a given time period. Nonetheless, this is an important volume that scholars will mine productively for the information most useful to their own lines of inquiry.

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Law and Long-Term Economic Change: A Eurasian Perspective.
Edited by DEBIN MA and JAN LUITEN VAN ZANDEN.
Stanford, Calif.: Stanford University Press, 2011.
376 pp. $65.00 (cloth and e-book).

What are the relations between the law and long-term economic changes? In this anthology, fifteen scholars attempt to answer this ambitious, yet fundamental, question about the history of economic development, to which both Karl Marx and Max Weber also sought an answer. More specifically, these scholars are interested in inquiries such as:

1. How did the so-called rules of the game and legal institutions emerge and evolve, and why were these institutions structured as they were?
2. What were the long-term impacts of these legal institutions on economic development?

Although their approaches differ greatly in terms of regional and institutional focus, the authors seem to agree on the following generalizations. First, legal rules had long-lasting consequences on long-term economic trajectories for different civilizations. Second, sociopolitical power structures determine the nature of legal systems. The state or

social political power structures acted as filters, letting through ideas compatible with the interests of those who controlled them and suppressing the others (pp. 4–5). In other words, law, or legal institutions, reflected the interests of the ruling elite.

Culture matters in the formation of a legal institution. These authors employ a three-layer analytical model—cultural traditions or values, social and political structures, and institutional rules—to delineate the formation process of a legal institution. The first two tiers filtered through the ideas that would be acceptable to those who controlled the institutions. Then the filtered ideas were formalized and institutionalized to become legal regimes. If cultural traditions were more open and adaptive, they argue, legal institutions would gain the vitality to continue. On the other hand, if social and political structures, acting as filters, serve only the narrow interests of the status quo, legal institutions will eventually petrify and become obsolete.

The ripple effects of the Great Divergence debate permeated the study of law and long-term economic change. Two articles in this anthology attempt dialogues with the Great Divergence theory. Their findings, though, are inconclusive. Examining Chinese legal tradition, Ma argues that the divergent legal traditions of Europe and China should bear greater explanatory power with regard to that long-term economic divergence. While Ma is more assertive in rejecting the Great Divergence theory, Kishimoto hesitates to do so outright. She rejects the theory on the ground that the thesis that the Chinese type of ownership may have had the potential to generate modern economic growth is counterfactual. Her evidence only affirms that, "the underpinning Chinese legal culture may not itself hold the key to the unique breakthrough of modern economic growth" (p. 86).

Although less explicit, the findings of the other authors challenge the Great Divergence theory from the perspective of the law and economy. Most articles exploring European legal institutions point out that it took a long time for a legal institution to take shape. The roots of the legal institutions that had favorable effects on modern economic activities often took centuries to evolve. The Great Divergence theory, from this point of view, did not begin in the early modern era; rather, it began to take shape in medieval Europe. For example, the bankruptcy law, which is essential for a capitalist industrial economy, emerged in the thirteenth and fourteenth centuries in northern Italy. Bankruptcy had been a common practice in European business circles, and the French formalized it by writing it into their commercial code of 1673 and 1807 (p. 200). The bankruptcy law then spread to the rest of the world to facilitate the growth of the modern economy. A

similar line of arguments can also be found in the empirical studies on the evolution of the debt litigation institution in Holland (Dijkman), the establishment of commercial conflict settlement institutions in the Low Countries (Gelderblom), and the formation of the London Stock Exchange regulations (Neal). All of these trading rules and legal institutions, which have been crucial to the development of the capitalist industrial economy, took centuries to evolve.

The articles focus on non-European legal traditions in an attempt to explain why they failed to facilitate modern growth. They show that the absence of legal institutions of the European type in Asia and the Islamic world hindered these areas in generating the same economic growth as Europe did. Their findings suggest that the traditional legal institutions outside Europe, such as property litigation in Ming China (Zurndorfer) and the legal institutions of the Islamic world (Coşgel; Miura), all fell short of facilitating a modern economic growth similar to that in Europe because the legal institutions in these regions did not share the unique characteristics of the European legal tradition. Even the long British colonial rule over India did not change Indian legal tradition in a fashion substantive enough to generate modern growth (Roy; Swamy). In these cases, the first- and second-tier filters were working against positive changes. The status quo was reluctant to change. Their control over the political and social structures and their power over defining social values filtered out the possibility of changes that had the potential to benefit the economy in the long run, but that would also undermine their own long held interests. The findings in these cases dispute the so-called "would have been otherwise" thesis of the Great Divergence. Without the unique characteristics imbedded in Europe's long legal tradition, from these authors' point of view, the divergence between the East and the West continues.

The only exception is Japan. Similar to T. C. Smith's work on the modernization debate a half century ago, the question "Why Japan?" must be addressed to maintain the logical consistency of the anti–Great Divergence argument. Japanese legal tradition differed greatly from that in Europe even as late as the nineteenth century. While other regions, such as India, China, and the Islamic world, had trouble adapting to a growth-generating legal institution, why did Japan have little trouble transforming its economy? Japan is unique, Haley argues. Although not identical to the European legal traditions, Japan's legal tradition did share an important feature that helped to answer the question of "Why Japan?" By the twelfth and thirteenth centuries Japan had developed an "embryonic private law order" involving rou-

tine adjudication of claims by private parties. This development, Haley argues, allowed Japan to successfully replicate the European experience (pp. 20–21, 40).

The attempt to argue against the Great Divergence theory seems to miss the point. The theory does not argue that the legal systems in China and England were similar until 1750. Nor does it argue that the laws in India, China, and Turkey would be able to sustain European-style economic growth. Its argument has, rather, centered on the issue of economic productivity. To argue that the productivity of England and of the Yangzi Delta were similar does not necessarily imply that the legal systems of the two cultures were similar, because, as all the authors agree, culture matters. It would be naïve to assume that even without a set of European-style commercial codes, bankruptcy laws, and stock exchange regulations, India, China, or Turkey would have been able to generate their modern growth. It would, however, be equally naïve to assume that "transmission or translation" of the British law would enable India, China, or Turkey to replicate significant modern economic growth as it did in Europe. In fact, Roy's study shows that the outcome of transmission or translation was the opposite of what had taken place in England.

Property rights, enforceable contracts, and trading rules alone do not explain modern growth. The Great Divergence theory argues that modern growth is, above all, about technological change and a radical shift in resource constraints. The most significant changes were the vast increase in energy supplies and the decreasing importance of land. Without these, efficient markets and laws that regulate it are simply not going to generate modern growth. The argument against the Great Divergence theory in this anthology seems to rely on a counterfactual thesis that Kenneth Pomeranz did not explicitly propose in his work.

Finally, there are a few editorial issues the editors might consider clarifying. The most confusing one is in the article "Foundations of Private Law in Medieval Europe and Japan," where the author points out that "at least by the fourteenth century, these suits were being tried in Kamakura before the monchiijo under direct control of the bakufu." It is unclear what *monchiijo* refers to. Should it rather be read as *monchujo*? (問注所もんちゅうじょ; p. 39).

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