romance about men and their ideas, one that attempts to get beneath the easy plausibility and dry factuality of our concept of “history” to something more substantially profound about the human condition that cannot be found in facts alone (for example, Katō’s hypothesis that Jōzan never married because of a tragic love for another boy when he was still young). At the end of the essay, the mysterious old man has mysteriously vanished: he is of course, as we had suspected all along from his oddly personal insights into Jōzan’s motivations, the spirit of Jōzan and his creation, its genius loci, hovering over his life’s work, and provides an appropriate ending to the fascinating story of Shisendō and its creator.


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The appearance of an anthology that draws Edo and Paris together within a single editorial scope is a major scholarly event. As McClain, Merriman, and Ugawa point out, these two metropolises of the early modern world have seldom been studied side by side, yet their commonalities are compelling. Both were administrative headquarters of unprecedented size within their respective countries, serving new dynastic regimes of unprecedented ambition and reach. Both the Bourbons and the Tokugawa peppered their capitals with monumental architecture, and both made other bold interventions in urban space as well. Yet neither king nor shogun was able to dictate single-handedly what went on in his capital. From the beginning, both sovereigns enlisted the burghers to administer many of their own affairs. Both likewise struck compromises in providing for urban needs: taking a direct hand in setting up waterworks and in policing the capital, for instance, while leaving food provisioning largely in the hands of merchants. Finally, both the French monarchy and the Japanese shogunate saw their capitals slip partially out of their control over time, as commoners assumed more responsibility for urban services, took over certain boulevards or blocks for their own purposes, created zones where persons of different statuses could mingle, and, with increasing frequency, rebelled in the streets.

The exploration of these parallels forms the heart of *Edo and Paris.* Intrigued by the “startling” similarity between these cities on opposite sides of the globe, the editors brought together a group of specialists to determine “what was common and what was culturally specific about the early-modern experience in two geographically separated societies” (xvi). They chose to focus on the seventeenth and eighteenth centuries, a period described in terms of a synchronous trajectory of state development (“from the 1590s, when the Bourbons and Tokugawa rose to power, until the 1780s and 1790s, when the monarchy fell and the collapse of the Kansei reforms marked the beginning of several difficult decades that culminated in the Meiji Restoration”) (xvii).¹ Structurally as well as

¹ The earlier date is more easily justified than the latter, and the editors heavily emphasize the parallels of the earlier part of the period. Not surprisingly, however, individual contributions tend to slip out of this time-frame, whose somewhat strained political symmetry does not accurately reflect the early modern experience of either city. The French contributions characteristically push backwards into the sixteenth century, while several essays on Edo trace trajectories that continue well into the nineteenth. An explicit discussion of this slippage would have enhanced the book.
textually, the volume underscores the notion of parallel evolution in two far-flung “capitals of absolutism” during these centuries. Nine contributions on Edo are paired with six on Paris (along with two fully comparative pieces) under the broad headings of “Governance,” “Space,” “Provisioning,” “Culture,” and “Resistance.” While the individual articles address disparate issues, the summations provided by the editors consistently emphasize the parallels. The same message is conveyed visually by a series of paired maps and illustrations, beginning with the views of the Seine and Sumida riverfronts that grace the book’s cover.

Many readers will surely share the editors’ pleasure in probing the historical commonalities between Edo and Paris. Yet those in search of rigorous debate about the reasons for these commonalities are likely to come away disappointed. Edo and Paris is not organized around the kind of pointed historical questions that would elicit such a debate. What links the essays is not a shared problematic but a broad, open-ended theme: the notion of a dialectic between “the city from above” and “the city from below.” (The closing essay is appropriately entitled not “Conclusions” but “Visions of the City.”) Alleging that “scholars are still but imperfectly aware of the significance of the comparative dimensions of the early-modern experience in France and Japan” (37), McClain and Merriman see their effort as “an initial expedition into uncharted territory” (38). Their stated goals are accordingly diffuse: “to open up new ways of looking at the history of Edo and Paris, shoguns and kings, cities and power” (38).

Approached on these terms, Edo and Paris has much to offer. The historian of early modern Japan who works through the individual essays on Paris is bound to find material that spurs comparative thinking. Roget Chartier, for instance, discusses a seventeenth century edict that transformed “the town of wood into a city of stone” (144): a transformation that renders fire-damage—a recurrent theme in the essays on Edo—a virtual non-issue for the historians of Paris. Steven Kaplan, likewise, creates a vivid verbal map of grain procurement following a harvest shortfall in the 1730s. Parisians, he tells us, were able to secure food “from all corners of the Western world: rice from Carolina, wheat and flour from Philadelphia, various grains from Archangel, Riga, Koenigsberg, Hamburg, Danzig, Amsterdam, London, Livorno, Genoa, Sicily, Marseille, the Levant” (193). This impressive roster of place-names serves as a sharp reminder of how comparatively limited the shogun’s options were in times of dearth.

The essays on Edo are richly rewarding as well. The range of topics is sufficiently broad, and the material sufficiently fresh, to challenge every corner of the Tokugawa field, from political and economic to cultural and intellectual history. Among the more provocative generalizations encountered along the way are William Kelly’s notion that “societal management” in the Edo era was marked by “a curious combination of technical stasis, organizational transformation, and cultural elaboration” (314); Anne Walthall’s pinpointing of the 1780s as the beginning of “an apparent waning of state power” (albeit one that “should not be seen as linear, continuous, or smooth”) (413, 427); and Jurgis Elisonas’s finding that a full century after Sekigahara, “Edo was still without a literary identity” (285). And the two comparative essays bristle with suggestive passages, including Henry Smith’s remarks about the “far more privatized nature of the city in Japan than in Western Europe” (347, 352), and Ikegami and Tilly’s observation that trade restrictions may not have been the main brake on Japanese merchants’ power. It was rather the missed “opportunity to engage in the lucrative business of war,” they argue, that rendered Edo’s merchants “unable to break through the Tokugawa system of domination” (439).
Such a brief set of highlights does not begin to do justice to the empirical material in *Edo and Paris*. Rather than pursue the variety of diverse leads suggested by the individual essays, however, I want to focus here on some of the methodological issues raised by the volume as a whole. My starting point is the conviction that, if the authors of this anthology are indeed scouts on an intellectual frontier, their initial foray should teach us something about how to equip ourselves for subsequent expeditions. To that end, and in the interest of furthering the debate on early modernity that was begun on the EMJ Network last year, I would like to suggest four ways in which the approach developed in this volume will need to be refined if we are to move further toward a nuanced vision of Japan in the early modern world.

1. A first area I would target for improvement is the vocabulary of social analysis. As the editors of *Edo and Paris* freely acknowledge, the two-part model of social structure implied in their recurrent formulation of “the city from above” and “the city from below” is far from adequate. For starters, reserving the term “above” for the ruler and his government (the monarchy, the shogunate) means that even the wealthiest urban elites—including privileged merchants and master artisans—are thrown in with laborers, beggars, entertainers, and the like as part of the city from “below.” But the real issue is not where the authors have chosen to draw the line. Rather, it is that such crude categories as “above” and “below” are simply too reductive to illuminate a complex social formation. As Kelly insists, “the multiple divisions within the Edo elites and among Edo commoners were as important as whatever solidarities they represented vis-a-vis one another” (325). The notion of “middlemen” (or “intermediaries”), which is central to Sharon Kettering’s article on municipal authority in Paris, helps to nuance this binary model in some contexts. But privileged groups inhabited a variety of distinct social positions, which need to be analytically differentiated from one another. For future comparative social inquiry, a more sophisticated approximation of the social formation is in order.

2. Forging a more complex model of social dynamics in the city will require tackling a second methodological challenge: defining the early modern experience in a more multi-dimensional vocabulary than that of “absolutism.” This terminology would seem to be limited in two ways. First, it speaks exclusively to the realm of politics. This makes a precariously narrow basis for launching a multi-party expedition into new historical terrain; interdisciplinary dialogue surely requires a more interdisciplinary vocabulary. Secondly, even within the political realm, the notion of absolutism identifies a complex state configuration solely in terms of the authority exercised by the sovereign. Compare this, for instance, to the proposition that Tokugawa Japan and Bourbon France were “agrarian-bureaucratic states,” defined by a conjunction of “an agrarian, urbanizing, commercial economy and a semicentralized,

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2 In their concluding essay, McClain and Ugawa note that “just as there was neither one Edo nor one Paris, neither was there a single ‘above’ or a single ‘below’” (457). But their antidote to the notion of a monolithic category is to posit an indistinct maze, where “wealth, status, occupation, gender, age, and any number of other attributes divided the populations and multiplied their visions of what the city ought to be” (458). While all of these attributes were undoubtedly important, listing them does little to clarify the social processes that shaped these cities.

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1 Both the Bourbon kings and the Tokugawa shoguns are defined as absolute rulers on the grounds that each “embraced in principle and often exercised in practice an unprecedented degree of power” (5). While this claim is substantiated empirically in this and later passages by reference to the particular achievements of each state (in adjudication, taxation, monopolizing violence, and the like), neither the historiography of absolutism nor its problems as an analytical category are explicitly addressed.
partly bureaucratic, dynastic state, in which hereditary monarchs ruled with the aid of appointed officials. Such a formulation shifts attention from the ruler per se to the political-economic constellation as a whole. While the merits of any characterization can be debated (this one too, for instance, says nothing about cultural processes), the notion of "agrarian-bureaucratic states" does at least point toward the sort of broader terminology that will be needed to bring a multidimensional social world into view.

3. A third problem with this book's framework is a disjuncture between the restricted geographical scope of the evidence and the broader claims of the conclusion. Focusing narrowly on two metropolitan areas can certainly illuminate contrasts and parallels between them. But the editors here want to go further, asserting that the like ultimately outweighs the unlike. While acknowledging a series of important differences between the two cities, they insist in the end that any contrasts between governance and urbanism in France and Japan "specify differences of degree and magnitude, not of kind," for "in both societies the trajectory arched toward absolutism." Not that either sovereign ruled single-handedly; "while absolutist state power was growing [?], neither Tokugawa shogun nor Bourbon king ever became omnipotent ... and indirect rule emerged as a defining component of the early-modern state in both France and Japan" (464). Yet whatever the compromises negotiated between rulers and residents, they argue, both Edo and Paris can be seen in the last analysis as members of a single species, "capitals of absolutism."

This conclusion begs a comparison that is never broached within the framework of the book. For judging whether the similarities between Edo and Paris were ultimately greater than the differences implies an ability to compare these two capitals with other cities, in other times or places. If the reader is to be convinced that the contrasts between Edo and Paris were, indeed, "differences of degree and magnitude, not of kind," the authors would have to plot Edo and Paris on a wider spectrum of urban formations and their relations to state power. But apart from some suggestive passages to this end in the two explicitly comparative essays, no such discussion is broached within the covers of this book. The relevant horizon of comparison is never specified.

I would submit that the logical comparative framework is that of early modern state capitals across Eurasia. As Jack Goldstone has noted, the economic and political configuration of the early modern state system as a whole "was quite distinct from the largely local, subsistence, nonurban economies and feudal polities that preceded it, and from the industrial economies and democratic polities that followed." And this was true not just in France and Japan, but throughout the rimlands of Eurasia. To be sure, the degree of commercialization and urbanization varied greatly from one region to another, as did the degree of power (de facto as well as de jure) wielded by the early modern sovereigns. Given this variation, it is indeed possible that Edo and Paris had more in common with each other than with contemporary agrarian-bureaucratic capitals elsewhere. But to determine that would require embedding this study in a context broad enough to illuminate what, exactly, distinguished these two cities from their contemporaries. Absent that sort of context, the assertion of commonality with which the book ends can be little more than a tautological reiteration of the "startling similarities" with which it began.

4. Specifying a broader comparative context would in turn enable future researchers to ask more pointed historical questions.

Goldstone, Revolution and Rebellion, 41
What was it about the early modern world, after all, that prompted both the Bourbons and the Tokugawa—and so many of their contemporaries—to invest in massive administrative headquarters? What did these unruly capitals do to justify the expense of their provisioning and policing? The answer surely turns on their function in the wider space of the state, itself a product of a competitive international state system. Cities concentrated the resources that were needed to transform a new dynasty into the effective administrator of a sprawling and sometimes rebellious nation. What those resources were, and how the sovereign used his capital to control the realm (in the face of challenges from both within and without), are never systematically addressed in this book. Edo and Paris are approached here essentially as objects of state power, rather than as means to further that power. The preface’s insight that they were built in the first place as tools with which to project the king’s or shogun’s authority outward is largely lost in the intramural focus of the succeeding essays. As a result, while the parallels that prompted the study are variously explored, they have yet to be explained.

Interestingly, the one extended explanatory passage by the editors analyzes not the similarities but the differences between the two capitals. Looking back in their closing essay on the volume as a whole, McClain and Ugawa note evidence of “significant and instructive variations between the two countries concerning styles of urbanism and absolutist governance.” At the heart of those differences, in their view, is a consistent finding “that the Tokugawa shoguns wielded greater power than did the Bourbon kings” (460). In the tussle between those “above” and those “below” within the capital, the rulers of Edo are seen as consistently stronger than their counterparts in Paris.

This surprising observation is followed by a terse but far-reaching discussion of the salient reasons for the relative strength of the Japanese regime. At the national level, the Tokugawa are said to have enjoyed a more decisive military victory over their domestic rivals than did the Bourbons. This gave them a stronger fiscal base, which in turn helped them create a more pliant civil bureaucracy; “the shogun’s enormous wealth, a consequence of Ieyasu’s great military victories, seems to have given the Tokugawa regime economic leverage of the sort the Bourbons could only dream about.” At the international level, meanwhile, the shogunate was spared chronic warfare, whose price for the French monarchy included “an eventual attenuation of state power as merchant-financiers made the crown dependent on them for credit.” Finally, at the level of the city itself, the Tokugawa had essentially a blank slate to work with; merchants, aristocrats, and religious institutions had more entrenched privileges in 600-year-old Paris than in the new city of Edo. “Perhaps for that reason, the Tokugawa shoguns demonstrated a greater ability to dictate the use of space than did the Bourbon kings” (462). This perceptive passage is one of the most valuable contributions of the volume. But it is almost lost in the larger framework of the book, which subordinates all contrasts to an assertion of overarching likeness.

The kinds of methodological refinements proposed above—namely, specifying the social dynamics in the city, broadening the conception of the early modern configuration, embedding the Edo-Paris comparison in a wider geographical context, and asking focused questions about historical process—should allow future researchers to evaluate that assertion more fully. But I suspect that such methodological changes would also alter the terms of analysis in fundamental ways. In particular, what is represented here as the shared “weakness” of these absolutist rulers—their need to compromise repeatedly with capitalists—might well be revealed as a shared

4 The outstanding exception is the essay by Eiko Ikegami and Charles Tilly, which keeps the capital’s function within the wider polity constantly in view.
strength of their respective regimes. Precisely because both the French and Japanese rulers were chronically short of money and had to make continual concessions to merchants, both the Bourbon kings and the Tokugawa shoguns inadvertently opened space for commercial development and industrial innovation. Not all early modern economies fared so well; as Edward Fox has noted, land-based kingdoms have often misunderstood the nature of mercantile wealth, and ended up killing the goose in their efforts to seize its golden eggs. But with the Bourbons and the Tokugawa, even the designation of a privileged elite failed to stifle economic competition. New generations of upstarts managed repeatedly to break the privileges of established groups, circumventing monopolies with sufficient success to secure tacit recognition and even (in some cases) legal privileges of their own. This process is well known for Tokugawa Japan. That similar upsets occurred in Paris as well is suggested by Steven Kaplan’s research on the unlicensed workshops of the Faubourg Saint-Antoine, where hundreds of “false workers” broke away from the constraints of the guilds “to organize their production and their distribution more rationally, to divide work efficiently, to master the seasonal and cyclical rhythms, to realize various savings in labor and money” (378). In a word, royal prerogatives in Paris as in Edo did not entirely strangle flexibility and innovation; in contrast to their frustrated contemporaries in St. Petersburg or Madrid, non-licensed merchants in Edo and Paris seem to have found ways to break into the charmed circle of profit. Why this historical variation occurred—and what its implications were for subsequent industrial or political development—would seem to be important questions for future research.

It is questions like these that lie at the core of comparative history—a genre whose goal is “not merely to find analogies or generalities in historical experience [but] to find causal explanations of historical events.” On the whole, Edo and Paris eschews such causal explanations. In the clearest formulation of their goals, the editors of this collection frame it rather as a preliminary exercise, designed “to develop a set of empirical findings, questions, and conceptual approaches that could more clearly define the similarities, as well as the disjunctions, in regard to political power and urban developments in Japan and France” (xvi). In this preliminary task, they have admirably succeeded. Moreover, by bringing together leading scholars on Edo and Paris, James McClain and his colleagues have taken a major step toward breaking the parochialism of Tokugawa and Bourbon history. But if the full potential of the Edo-Paris comparison is to be realized, the analytical tools used in this initial exploration must be refined. Those who would push our understanding of early modernity further will need to sharpen the terms, broaden the context, and tackle more provocative questions about historical process.


Goldstone, *Revolution and Rebellion*, 52; emphasis in the original. The rest of this passage reads, “Given that historical variation reveals both continuity and change, comparative history proceeds by asking which elements of the historical record were crucial. Thus to study merely the history of two cities, or of two countries, is to practice parallel, but not comparative, history.”