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Response to Comments on *Strange Parallels*

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I AM GRATEFUL TO *The Journal of Asian Studies* for organizing this forum and to the seven contributors for taking the time to read my volume(s) and to wrestle with the issues raised therein. The resultant essays fall into two categories. Eric Tagliacozzo, Carolyn Cartier, and John Wills, Jr., examine Asia or Eurasia broadly conceived, whereas Robert Hellyer, Victor Mair, Ramya Sreenivasan, and Leonard Andaya weigh the applicability of my work to particular Asian regions. I consider each set of contributions in turn.

All three generalist critiques find valid insights in *Strange Parallels*, but unsurprisingly, they do so from different thematic vantage points. In his learned, searching, extraordinarily generous commentary, Tagliacozzo explores three overarching themes: political chronology, political and economic processes promoting consolidation, and external vs. internal agency. He begins by considering my contrast between, on the one hand, political and cultural integration starting in the mid-first millennium CE in the protected zone, represented by mainland Southeast Asia, much of Europe, and Japan; and, on the other hand, integration beginning in the second or first millennium BCE in the exposed zone, illustrated by China and South Asia. In the main he supports this zonal distinction in chronology, as well as my periodization for individual subregions.

He then interrogates those dynamics that underlay Asian state formation, with particular attention to the post-1500, what is increasingly termed "early modern," era. *Pace* historians who would lump together China and Japan as part of a coherent East Asian path to modernity, he supports my emphasis on Chinese-Japanese differences not only in chronology, but also in internal process, territorial and demographic scale, and the role of external conquest elites. He also reviews sympathetically the thesis that political trajectories in mainland Southeast Asia after 1550 diverged substantially from those in the islands.

However, Tagliacozzo argues that my interest in proto-national units might well have been balanced by greater attention to those networks of trade, culture, and diplomacy that ignored modern boundaries. In Southeast Asia in particular, he suggests, I should have paid more heed to interstitial spaces between empires – "overlapping spheres" – where dual political allegiances were common and culture was notably eclectic. His point is well taken. If across Eurasia between c. 1300 and 1800 the construction of proto-national/imperial

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cultures battened on the ever more rapid circulation of goods, texts, and people and constituted the dominant trend, similar, but crosscutting processes simultaneously bolstered local identities along as yet ill-defined frontiers. What is more, particularly in imperial cores, rising literacy and new communications circuits tended to give universal religious affiliations a novel salience and coherence that also defied national compartmentalization. Volume Two's discussion of France, Russia, and Japan considered these contrary currents in some detail. But in Volume One on mainland Southeast Asia, I surely could have been more forthcoming – all the more so because among the regions under review, it was arguably in Southeast Asia that frontiers were most hazy and universal religious solidarities most fully articulated.

Finally, Tagliacozzo invites us to reconsider the fluid, tangled relation between "foreign" and "local" "motors of change" across Eurasia. By definition, of course, non-local factors alone can explain why between c. 800 and 1800, and with increasing force after 1600, Eurasian regions with little or no direct contact followed a broadly similar economic and political rhythm. Drawing on his immense knowledge of Asian trade, Tagliacozzo argues persuasively that in both South and Southeast Asia after c. 1600 the often barely visible, but innovative, even subversive, activities of foreign traders deserve more attention than they have generally received. At the same time he agrees that "non-local factors" must be interpreted liberally to include elements – such as climate change, disease, and Inner Asian incursions – that were either independent of maritime trade or only tangentially related to such trade.

Forced to compress into a few sentences my own views on external agency, I would argue, in line with my introductory essay in this issue, that for much of Europe and Southeast Asia between c. 800 and 1450 the single most critical synchronizing force was climate, namely, the agriculturally beneficent Medieval Climate Anomaly (c. 900 to 1250/1300) followed by the onset of the far less propitious Little Ice Age. In these centuries, when the volume of long-distance exchange was yet modest but climatic fluctuations were pronounced, climate created complex feedback loops with trade, smallpox domestication, local institutions, and the Black Death to generate across much of Eurasia a two-stage cycle of expansion and collapse (Lieberman, 2011).[1] Between c. 1450 and 1800, when climatic swings became less pronounced, synchronization drew strength, first, from post-crisis demographic and political recovery, and second, from the rapid growth of multilateral, primarily maritime, trade. Along the sealanes moved bullion, which was critical to commercial intensification; European-style firearms, which pyramided the advantages of the wealthiest, most innovative polities; and New World crops, which spurred growth in terrains ill-suited for

[1]In part because of different disease regimens and climate, Japanese and Mideastern demographic-cum-economic rhythms to 1500 seem to have correlated poorly with those in northern Europe, China, South Asia, and much of Southeast Asia.

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other cereals. In China, Southwest Asia, and South Asia between 1200 and 1800, post-crisis recovery and maritime trade intersected with a third grand coordinating dynamic, namely, Inner Asian interventions. Moving toward the coast from the interior but deriving much of their initial strength from generalized medieval prosperity and a concomitant increase in sedentary-steppe interactions, Inner Asian conquest elites became the chief agents of early modern integration throughout the exposed zone.

In her theoretically informed discussion of territorialization, Cartier is as concerned to analyze the epistemological and political context of contemporary historiography as she is to fix the content of historical change. She notes the close intellectual fit between economic globalization in the 1990s and Anthony Reid's 1993 emphasis on trade diasporas and economic mobility in early modern Southeast Asia. She analyzes recent shifts in sensibility that favor a more territorially centered approach such as I have developed, and she ponders the contemporary political implications of G. William Skinner's well known thesis that Chinese imperial administration, given its fixed structure and mushrooming population, was inherently devolutionary. Underlying Cartier's analysis is an insistence on the fluidity and indeterminacy of territorial concepts in both our own day and in the medieval and early modern periods. To consider the variable dynamism of domestic geographies, she writes, is "not to look at the interstitial zone between states, but at the different conditions and systems of territorialization within states… that have cohered over the *longue durée*." This seems an apt description of my project insofar as I make two overarching claims. On the one hand, in Europe, Japan, Southeast Asia, China, and South Asia, territorial structures were *sui generis*, political identities were labile, and the triumph of any particular configuration remained contingent. On the other hand, each region exhibited a long-term, essentially one-way movement toward some type of political integration, which had both cultural correlates and spurs. In other words, as I understand Cartier's densely argued essay, we both distinguish between near universal processes and highly singular outcomes.

Capitalizing on his background in European and Chinese history and the history of European-Asian diplomacy, Wills deftly summarizes some of the main themes of *Strange Parallels*, while identifying *en passant* what he regards as themes deserving fuller coverage. I sympathize with all of his suggestions, although they are partly a matter of taste, and in a book of 973 pages further expansion faced obvious limits. Wills thinks, for example, that English history, and Anglo-French military competition as a motor of French state formation, deserved more attention than they received. He also wishes that I had provided more substantive treatment of Chinese and French political culture, including popular attitudes during the French Revolution.

Among the four country-oriented responses, Hellyer's articulate essay most clearly justifies my hope that *Strange Parallels* may encourage specialists to reconsider familiar local features as idiosyncratic expressions of wider patterns.

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He not only certifies the factual accuracy of my Japan chapter, but also accepts the basic thesis that Japan was both distinct from, and quite similar to, other protected zone realms. Many of Japan's peculiarities can be attributed to its relative isolation, which was simultaneously climatic, epidemiological, and military. Japanese demographic and economic rhythms often ran counterphasic to those elsewhere in Eurasia. Whereas other protected zone realms saw two or three devolutionary cycles, Japan experienced but one. Those military spurs to administrative reform that were a more or less constant feature in other protected zone realms were chronologically constricted in Japan. The absence of an external enemy inhibited what was actually a remarkable degree of cultural homogeneity from becoming overtly political, and helps to explain why in Japan alone, the regime in 1830 was not substantially stronger than in the early 17th- century. Set against such features were overarching similarities to other protected zone realms including: a reasonably centripetal internal geography; a late first millennium charter state that looked to older Eurasian civilizations for inspiration; the gradual failure of charter institutions to accommodate sustained economic growth; a post-civil war, unprecedentedly effective "early modern" unification in the late 1500s/early 1600s; a deepening symbiosis between political consolidation, commercial intensification, and literacy-aided cultural integration, with the latter exhibiting both vertical and horizontal features; and – as befit all protected zone realms – freedom from sustained foreign occupation. In Hellyer's view, this bifocal concern with idiosyncrasies and parallels can restrain a regrettable tendency to stress Japanese cultural essentialism.

In one of his more intriguing asides, Hellyer notes that *Strange Parallels* failed to compare Japan to its most appropriate geographic match, namely, the Indonesian archipelago. Regrettably, I had not given the matter much thought, but my initial impulse is to suggest that between 800 and 1830 indigenous peoples in the archipelago never matched Japanese levels of integration because: a) The Indonesian islands are far more numerous and widely dispersed. b) None of the principal Indonesian islands, not to mention the archipelago at large, has a natural core comparable to the Inland Sea-Kanto axis. c) Agrarian productivity and commercial specialization remained lower than in Japan. d) A combination of valuable spices and greater nautical accessibility exposed island Southeast Asia to deforming European interventions over three hundred years earlier than Japan. (On barriers to integration in island Southeast Asia, see too Andaya's commentary.)

Victor Mair is no less interested than Hellyer or I in globalizing particularist historiographies. Indeed, although we have never met and although I discovered his work rather late, I consider Mair an intellectual guide and mentor. A scholar of Inner Asia and East Asia, he has been an early and persuasive advocate of the view that Inner Asians, far from being relentlessly destructive, often were a creative force. By helping to transmit military and non-military technologies, flora, fauna, and cultural concepts across Eurasia's interior routes of trade and

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migration, nomads and pastoralists contributed to the rise of civilization in North China and elsewhere. What is more, starting in the second millennium BCE and climaxing between c. 1450 and 1800 CE, cavalry superiority accorded Inner Asians a seminal role in state formation in lands accessible from the steppe. In China, in particular, the most extensive and long-lived empires almost invariably depended on non-Sinitic leadership. Without the northwestern peoples, Mair concluded, there would have been no "China." Thus he brought into much sharper focus, and supported with the weight of his enormous expertise what I had begun to sense were two of the defining, interrelated characteristics of the exposed zone, namely, precocious civilization and openness to Inner Asian political domination. A contrast with the protected zone was at least implicit in Mair's earlier writings and, is of course, explicit in the current essay (Mair, 2005).[2] No less welcome is Mair's strong endorsement of climate change as a driver of coordinated Eurasian development.

Sreenivasan adopts a more internal, synchronic, anthropologically inflected perspective than do other contributors. Her commentary – essentially an effort to deconstruct the categories that I used to analyze South Asian political and social organization between c. 1550 and 1800 – rejects reified, abstract notions of state, caste, family, and religion to focus on the malleable self-images and interests of concrete local actors. The Mughal state, she warns, must be seen not as a stable organization, but rather as a fluid system of negotiated relationships. Likewise, caste designations, far from being fixed, were constantly manipulated.

This distinction between normative categories and actual practices is certainly apposite, and Sreenivasan's sensitive, sophisticated review of recent scholarship is it itself a welcome addition to the literature. Yet, as she herself emphasizes, her thematic and chronological encapsulation and my comparative approach conflict. The problem, as I see it, is three fold.

First, her lack of interest in macro history leads her to misconstrue my basic thesis. Her claim that I see "state formation" as "the central motor both driving and manifesting" long-term development is simply mistaken. As a primarily local phenomenon, state formation cannot explain Eurasian synchronicity. If we must identify a prime mover in India and Eurasia at large, it was the secular-cum-cyclic expansion of population and material resources, which created the preconditions for commercial, cultural, and political integration.[3]

Second and not unrelated to the previous difference, Sreenivasan deems my analysis suspect because I "reify" the state, exaggerate its power, and privilege integration over "disorder." But she fails to explain how exaggerating state

[2]See especially Mair's seminal 2005 essay, 46–84, 205–217. I am grateful to Peter Perdue for bringing Mair's work to my attention.

[3]A view summarized in the introduction and conclusion of Lieberman, 2003 and 2009; in the overview diagram of forces promoting integration in Lieberman, 2009, 47 Figure 1.3, and in my comments on Tagliacozzo's work *supra*.

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power would in any way strengthen my interpretation, or conversely, why acknowledging the conditional nature of Indian political authority would jeopardize my analysis. My view of imperial power derives not from some abstract notion of "absolutism," much less from self-aggrandizing imperial rhetoric. Rather, my perspective is entirely relative and developmental: my central claim is simply that, judged in terms of resource control and military capacity, the Delhi Sultanate was substantially more effective than its pre-Muslim predecessors, but that the Sultanate was weaker than the Mughal empire, which in turn was weaker than the British Raj. This schema can easily accommodate all manner of contingency, local idiosyncrasy, and spatial irregularity. Indeed, if I point to Mughal political and fiscal achievements, I also characterize the Mughal empire, like its early modern Southeast Asian, French, Russian, and Japanese counterparts, as a collection of patronage networks subject to constant bargaining, accommodation, and entropy. Such features are key to understanding Mughal, as well as Delhi Sultanate, decline. Likewise, I portray caste as a set of local categories frequently renegotiated, and the boundary between "Islam" and "Hinduism" as unstable and permeable. In short, in arguing for fluid, contextualized understandings, it seems to me that Sreenivasan pushes against an open door.

Nor do I treat integration as inherently preferable to fragmentation. To be sure, by lowering transaction costs, quelling disorder, and guaranteeing hierarchy, state strengthening could aid wide sectors of the population. Inevitably, these same processes exacted a heavy price – in wealth, state-sanctioned violence, and self-respect – from those on the social margins. *En passant* I describe both benefits and costs. But I am less interested in devising an ethical balance sheet (or a Eurasian political score card) than in solving a basic conundrum: Why in far-flung sectors of Eurasia over several centuries did interlocking trends toward political, cultural, and commercial integration grow ever more insistent and synchronized? This is the intellectual axis on which my volumes pivot, besides which cost-benefit calculations are irrelevant.[4]

Third and most basic, Sreenivasan's commitment to micro histories that at some future date might support a new macro history means that for the time being, she is unwilling to engage with any of *Strange Parallels'* central arguments. By definition, historical revision is never ending. But surely we already know enough to comment on the following claims/hypotheses: a) South Asia

[4]Moreover, the suggestion that my analytic categories are Eurocentric ignores the fact that if we accept the goal of comparing Eurasian societies, we need uniform criteria. Categories like market integration, political centralization, cultural diffusion, and so forth are now part of a universal discourse on which Japanese, Chinese, and Southeast Asian historiography depend no less than European. What is more, other basic concepts in my interpretation of European and Indian experience – charter polities, administrative cycles, progressively shorter interregna – derive not from European studies at all, but from my reading of Burmese and Chinese history. These issues are discussed at length in Lieberman, 2009, 117–21.

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experienced partial, but persistent long-term trends toward territorial, administrative, and cultural integration. b) South Asia's developmental chronology reflected the intersection of the Medieval Climatic Anomaly with Inner Asian incursions, followed by maritime-derived stimuli. c) Despite its prestige and homogenizing thrust, Perso-Islamic culture's limited incorporation of regional solidarities rendered it a flawed instrument of imperial integration. d) The stability of transregional formations was further imperiled by alphabetic scripts, low levels of literacy, caste, segmented sovereignty, and rural militarization. e) But if empires remained vulnerable, Inner Asian military power and the subcontinent's relatively centripetal geography repeatedly undermined post-imperial regional states. In a word, geography and culture accorded South Asia a trajectory intermediate between those of China and the protected zone: its regional kingdoms were less long-lived than in Europe, but more secure than in China. Are any of these ideas valid? At the end of Sreenivasan's essay, the curious reader is no better able to judge than after reading *Strange Parallels.*

In his commentary on island Southeast Asia, Andaya shows greater interest in large-scale comparisons than Sreenivasan. A widely respected historian of the Malay world and eastern Indonesia, Andaya is primarily concerned to identify those features that produced persistently lower levels of political and cultural unity in island than in mainland Southeast Asia. These elements (the first two of which I also sought to highlight) included the islands' far more fragmented geography, consistently lower demographic densities, and a greater emphasis on voluntary, reciprocal engagements between the paramount ruler and territorial subordinates.

As Andaya readily acknowledges, however, mainland-island differences often were merely a matter of degree. For example, although imposed acculturation may have been most common in Burma and Vietnam, the island pattern Andaya describes in which peripheral courts emulated the prestigious norms of the capital was also critical to consolidation on the mainland. Nor, I would add, was this peculiar to Southeast Asia. In 16th-century France, for example, the spread of French language was less the work of the monarchy, content to rule over a polyglot realm, than of provincial notables eager to ape the prestigious norms of Paris and the royal court.

I would also emphasize certain broad developmental rhythms shared by both sectors of Southeast Asia to which my earlier essay in this issue referred briefly. Those charter states that flourished on the mainland between c. 900 and 1350 had functional and chronological equivalents in Srivijaya, centered in southeast Sumatra, and in classical Java. Both of these island polities disintegrated during the 14th or 15th centuries, that is to say, in the same period as mainland charter realms faced severe crises. But from c. 1450 to 1650, during Anthony Reid's celebrated Age of Commerce, both sectors of Southeast Asia saw stunning political and economic revivals. If it lacked the same level of official patronage as its mainland counterparts, as a textually-based, universal set of practices that

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strengthened emergent polities and that spread rapidly after 1450/1500, Malayo-Islamic culture also resembled Burmese-Buddhist, Siamese-Buddhist, and Vietnamese-Confucian culture. Finally, between c. 1600 and 1840, as each mainland realm achieved novel levels of territorial influence and resource control, the islands too produced unprecedentedly powerful territorial-cum-commercial formations. In contrast to the mainland and other protected zone realms, however, in the islands these early modern formations were led not by indigenes – but by alien conquest elites, Dutch and Spanish, who in effect functioned much like Mughals and Manchus in continental Asia. Should one conclude from all this that post-1600 history on the mainland and in the islands diverged or continued to run in tandem? Clearly it did both at the same time.

All in all, I have found these exchanges quite valuable, both as a confirmation that my approach has some validity, and as a spur to redefine and defend my assumptions and categories. If, as seems true in several cases, my respondents also have been tempted to see the past in slightly new ways, this forum must be judged a success.

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