

# Communications to the Editor

## TO THE EDITOR:

David Gilmartin's and Tony Stewart's thoughtful review of my *Rise of Islam and the Bengal Frontier, 1204–1760*, published in the current issue of *The Journal of Asian Studies*, raises two issues to which I would like to respond, not by way of petty quibbling, but in a spirit of constructive debate.

The first pertains to the proper level of political analysis at which historians should study religious change. The reviewers complain that whereas I provided a detailed political narrative for the earlier years of Muslim rule in Bengal, my discussion of politics for the eighteenth century—a critical period for Islamization in the delta—was so limited as to make it “difficult to link religious change clearly to shifting patterns of political and cultural patronage during this period.” The book's discussion of politics, however, does not diminish as one moves from the earlier to the later chapters; rather, the level of analysis shifts from a Bengal-wide to a micro-society framework. I eschewed a Bengal-wide narrative of eighteenth century politics not only because this is a very familiar story, but more importantly because I wanted to capture a worm's-eye view of the politics of local patronage systems so as to link such patterns with religious changes that were also occurring at microlevel. Thus, chapter 7 examines how the political culture carried into seventeenth-century Bengal by the imperial Mughals was articulated in various subregions of the province. Using local documents, chapters 8 and 9 then explore, at the village level, land tenure and patronage networks among district officers (*faujdār, āmil*) and landholders (*zamindār, chaudhuri*) that led, in the seventeenth and eighteenth centuries, first to the emergence of Muslim charismatic pioneers and finally to that of Muslim peasant communities. Perhaps, then, we do not disagree on the importance of political analysis for explaining religious change so much as on the most appropriate level for making such an analysis.

Second, the reviewers contend that my model of religious change, which focuses on the evolving cosmologies of societies undergoing such change, deflects attention from the “actual religious practices that shaped Bengali life.” But for one thing, as the reviewers themselves acknowledge, our source materials on “actual religious practices” are extremely limited; we simply do not know much about the religious practices of premodern Bengali Muslims. A more principled theoretical issue, however, is the extent to which scholars should, when analyzing religious change, focus on a society's praxis as opposed to its religious cosmology, that is, the superhuman agencies that it postulates and with which it interacts. The problem with the praxis strategy, I suggest, is that it so easily lends itself to reducing religious systems to fixed checklists. But who gets to draw up such checklists?—especially when religious practices are so notoriously variable across societies, as is attested by a rich anthropological and historical literature. Is the singing of hymns or the use of Christmas trees either normatively or descriptively “Christian”? What does one make of the bewildering welter of practices that have been identified, through time and space, with any given religious tradition? Thus my book's focus on the changing identity and function of superhuman agencies in Bengali cosmologies was prompted not just by a paucity of sources on religious practices. As indicators of a society's

religious identity, verifiable data on religious cosmologies are, I believe, both more important and more reliable than something which, to an outside observer, might be considered as proper practice. Indeed, the very silence on matters of practice in premodern Bengali and Persian literature, in contrast to the substantial literature on superhuman agencies, suggests that premodern Bengalis themselves would have agreed with this position.

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TO THE EDITOR:

Richard Eaton's comments on our review of his pivotal and appropriately award-winning work, *The Rise of Islam and the Bengal Frontier*, raise several important issues.

The strength of Eaton's book lies precisely in his ability to politically and socially contextualize the transformations leading to the emergence of a large Muslim population in eastern Bengal. It is in this context that we noted the relative imbalance and asymmetry in the discussions of political context in the earlier and later parts of the book. As Eaton rightly points out, there is considerable attention to political context even in his later chapters, with the focus of this attention shifting increasingly to the local level. This conscious choice in and of itself cannot be faulted, save for a developing expectation on the part of the reader. Our central point, however, was that his model of stages of Islamization, discussed in chapter 10, is not, by and large, a politically contextualized one. Though he provides by far the best discussion we have had of the local Mughal political and cultural context for the establishment of settled Muslim communities in rural Bengal, the subsequent process of gradual Islamization is discussed largely as a self-sustaining one, driven by the power of the written word, with little attention to the changing political and social contexts of late Mughal (and eighteenth century) Bengal which might have influenced it. It is this, we argued, that gives the model, in spite of his disclaimers, a somewhat teleological character.

This is closely related to the question of changing religious practices. In noting that Eaton's book does not examine the religious life of practicing Muslims in Bengal, we in no way intend to suggest that changing cosmologies should not also be examined; this should not be an either/or proposition. Rather, our intention was to suggest that greater attention to changing forms of conflict over religious practice might have helped to soften the mechanistic character of his model of Islamization. There is a considerable body of texts in Bengali dealing with ideals and problems of religious practice during the period in question, texts that would identify issues of practical import to Bengalis (not to outsiders). While some of these are translations of Persian, Arabic, and Urdu sources—that in itself is significant—many are original works. Prominent among this mix would be Āfzal Āli's *Nasiyat* or *Nasihāt Nāmā* (sixteenth century), Nasrullah Khan's *Śarīyat Nāmā* (late sixteenth or early seventeenth century), and no fewer than twenty extant seventeenth and eighteenth century works, including Śekh Parān's *Nasihāt Nāmā*, Muhammad Khan's *Nāmāz Māhātmya*, Hāyāt Māhmad's *Hitājñān Vāñī*, and perhaps the most important work on practice written in the premodern period and still referred to today, Ālāol's *Tohfā* (late seventeenth century). Similarly, the works of numerous Sufi masters of the period could be employed for some of the more esoteric reaches of Islamic praxis (and cosmology), including those of Śekh Cānd, Hājī Muhammad, Śekh Mansur, and Ālī Rojā. While the value of normative works in reconstructing actual practice is certainly limited,

they do portray ideal types that can be used to establish the parameters of contemporary discourse on the issues in question. Importantly, differences in these ideals point to (potential) issues of real contention and can be used to identify key areas of changing practice and belief among Muslims of the day. Greater discussion of these issues would have substantially augmented the attempt to provide a more politically historicized vision of the process of change within Bengal's diverse and rich Islamic tradition.

Eaton's book is a landmark, precisely because it does contextualize the emergence of Bengali Islam in a highly sophisticated and nuanced way. And it is for this very reason that we raise the question about the model of Islamization that seems to describe a religious landscape without explaining how those different combinations were made theologically and politically possible.

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#### TO THE EDITOR:

Any work that presumes to offer a new approach to the study of an old problem courts controversy. Unfortunately, Mina Roces's review of my edited volume, *Anarchy of Families: State and Family in the Philippines* (*JAS* 53.4 [November 1994]: 1322–24), simplifies and ultimately distorts the book's arguments. In her crude caricature, the original work disappears. Since she is attacking a book of her own invention, it is difficult to reply. Yet in the midst of her rhetoric and prosecutorial questioning, Roces does, however, hit upon one substantive issue that merits discussion: the role of violence in Philippine politics. Indeed, Roces ignores a number of major essays in the volume to concentrate her attack solely on my work, focusing particularly on my discussion of violence.

In dismissing my emphasis on violence, Roces states (*JAS*, 1323): "I would argue that violence and warlordism are atypical of kinship politics. While the Duranos, the Moncados [sic], and the Dimaporos represent one extreme of families who use violence as a dominant dynamic, a more typical family would be the Osmeñas analyzed in the excellent essay in this book by Resil Mojares. . . ." Instead of depicting them as reliant on "military or economic coercion," Mojares, in her view, "insightfully shows the Osmeña family . . . using . . . ideology/issues." Through this "more sophisticated analysis," Mojares "pointedly challenges and even subverts McCoy's theme."

If Roces were correct and a contributor to the collection had shown that violence was irrelevant in the postwar rise of a major political family, then my analysis might require some modification. But we must ask: is Mojares, in fact, denying that the Osmeñas used violence? His essay, first drafted in 1988, does indeed focus on the family's projection of an ideology of rational management and thus does not engage material issues such as violence. Nonetheless, Mojares's earlier work provides undeniable evidence that the Osmeñas made extensive use of violence in postwar Cebu.

Indeed, only two years before, Mojares published a definitive biography, *The Man Who Would Be President: Serging Osmeña and Philippine Politics* (1986), detailing the role of violence, among other elements, in the career of the Osmeña family's postwar leader Sergio, Jr., known popularly as "Serging." Summarizing Serging's rise as the dominant politician in Cebu Province, Mojares (1986, 79) focuses on violence: "For Serging and many of his contemporaries, violence, image-manipulation, and fraud were instruments in a range of available resources . . . Serging did not only typify an emerging style of Philippine electoral politics, he was one of its exemplars."

From the time he entered Cebu provincial politics in the 1951 elections, Serging Osmeña organized an “ ‘Internal Security Group’ of armed men” and used them to achieve what he later called a “balance of terror” (Mojares 1986, 47). During the 1953 elections, Serging campaigned “escorted by carloads of constabulary soldiers and an armored car with a mounted machine gun” (Mojares 1986, 61). Backed by “a retinue of armed bodyguards,” Serging, at one point in this bitterly fought contest, “sought out Mayor Gantuangco . . . and challenged him to a duel.” Another opposition leader was “gunned down by pro-Osmeña men” (Mojares, 1986, 59–60). By 1955, violence was so entrenched in Cebu politics that political “kingpins,” Serging among them, “maintained what came to be called ‘private armies.’ These were irregular collections of policeman, licensed security guards, constabulary soldiers on special detail, and hired criminals. Outside of legitimate work . . . , they were used for harassment and mayhem” (Mojares 1986, 77).

If Mojares is so resoundingly clear about the Osmeñas’ reliance on violence, why does Rocés try to distort the historical record by portraying them as her “typical family” that supposedly avoided violence? Her doctoral dissertation on “Kinship Politics in the Post War Philippines: The Lopez Family 1945–1989,” which she cited at length in this review, indicates that Rocés is writing in defense of her family, her family’s allies, and their class. In making sense of this debate, it is important to understand that Rocés belongs to one of Manila’s most prominent families. Under the Philippine Republic (1946–72), there were two leading media conglomerates—the Lopezes of *The Manila Chronicle* and the Roceses of *The Manila Times*. Indeed, Rocés and I have had this argument about violence before, not over the Osmeñas but over the Lopezes—the subject of my essay in the *Anarchy* volume and Rocés’s dissertation.

After 250 pages on the postwar rise of two Lopez brothers, Rocés (1990, 249) concludes her dissertation with an appendix on three similar families to illustrate “slight variations” in the “typical” patterns of elite politics. In one of these three case studies, Rocés (1990, 270–71) writes a short history of her own family and reveals their close alliance with the subjects of her dissertation: “Martial law was a trying period for the Rocés family . . . Congressman Rocés [her uncle] and his two journalist brothers Alejandro [her uncle] and Alfredo [her father] lost their jobs and were not permitted to leave the country. The family also developed close ties with the Lopezes who like themselves were also victims of the martial law era.” In the 1986 uprising against Marcos, the Rocés and Lopez families “stood side by side at the barricades.” When the dictator fled into exile, several members of the Rocés family “joined the Lopez family in the revival of *The Manila Chronicle*.”

In her naturally sympathetic account of the Lopezes, Rocés is determined to absolve them of any use of violence and attacks me for research to the contrary. In both my dissertation (1977, 124–31) and my later essay in the *Anarchy* volume (1994, 461–68), I described—from local press accounts and surviving participants—how the Lopez brothers used criminals in the late 1930s to force a rival family out of the provincial bus business through violence, assault, and intimidation. Without any contrary evidence, Rocés (1990, 92) insisted, in the section of her dissertation on this incident, that “the blatant use of violence was not characteristic of Lopez business or political panache.”

Since Rocés is writing about her own family and their closest allies, her determination to reinvent the past is understandable. Her logic may be muddled but her intentions are clear. Dismissing corrupt, violent warlords like Ramon Durano and Justiniano Montano as ‘atypical’ of real political families like her own, she offers

instead a sanitized account of Philippine politics as the work of a principled elite. Warlords who use violence, no matter how many provinces or towns they might rule, are not “typical” political families. Real political families of the Manila elite are patriots who abhor violence.

Roces’s invented past runs counter to the historical record, but it is still revealing—though not in ways that she may have intended. Under his martial law dictatorship of the 1970s, Marcos (1971, 61–73; 1973, 4, 150–51; 1976, 209) attacked established families as a corrupt “oligarchy of the old society.” In the name of national progress, he confiscated their assets, including the Roces and Lopez newspapers. Since the dictator’s fall in 1986, established elite families have scampered back into power—reclaiming public office, recovering assets, and restoring the political status quo ante Marcos. In this process of restoration, it has become important to reestablish the legitimacy of the old elite of families like the Lopezes. Marcos has become a convenient scapegoat for the failings of the old order, particularly its violence. While Roces’s review is bad history, it is interesting as a reflection of the processes by which the country’s dominant class is reconstituting its ideological hegemony.

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### TO THE EDITOR:

In his response to my book review, Professor Alfred McCoy points out that it is difficult to reply to my criticisms because I was attacking a book of my “own invention.” Allow me to repeat three pointed criticisms which McCoy has conveniently ignored in his rejoinder: (1) In the book’s introduction McCoy states that the book’s aim was to show the interaction between family and state in the Philippines but almost all the essays in the book were about individuals and not families. Furthermore these individuals were not even successful in staying in power long much less establishing a family dynasty; (2) McCoy contradicts himself: although

McCoy argued that violence was necessary for an elite family's rise to political and national prominence he also asserted that "the process of legitimation . . . discourages the continuing use of political violence" (p. 15) and concluded of the Marcos regime "In the end, it was his use of violence, along with economic mismanagement, that forced the national elite to turn against him" and "Over the long term . . . reliance on violence discredited the regime, forcing Marcos into exile" (pp. 16, 439); and (3) By arguing that individual politicians had close ties with presidents (always interpreted as the ultimate patron) McCoy's approach endorses the factional model without presenting any new insights. Such a model provides a simplistic analysis of Philippine postwar politics where every single incumbent president (with the exception of Marcos) was voted out of office. How does patronage politics explain the EDSA revolution of 1986? Should we insist that those who risked their lives by standing before armored personnel carriers were—being Filipino—motivated by personal or family politics? How then does 1992 presidential candidate Miriam Defensor-Santiago almost make it to the presidency with no family backing and little patronage resources?

Instead of addressing these three major points, McCoy flees from criticism and attacks the critic, imputing to me the ridiculous motive of hatching a conspiracy to bring back family oligarchy. Placing me with the elite he makes the farfetched charge that I am involved in a conspiracy to restore elite power in the Philippines claiming that I write history in order to 'sanitize' elite behavior in politics and to present elite families in the Philippines as 'patriots who abhor violence'. The work he is criticizing is my Ph.D. dissertation submitted to the University of Michigan in 1990. Is McCoy saying that the members of the dissertation committee Victor Lieberman, Karl Hutterer, Bradford Perkins, and Rhoads Murphey, missed seeing this incredible design to restore the oligarchy back in power? Does he imply they are part of the "conspiracy"? McCoy's conspiracy theory completely misrepresents my dissertation.

To shore up his theory of political violence as the dynamic of Philippine politics, McCoy now cites another work, the Mojares book on Serging Osmeña to prove that the Osmeñas used violence to gain power, hoping to dispel my observation that the Osmeñas in the Mojares essay (in the McCoy volume which was under review) was a more typical family which did not use violence to gain power. The title of the Mojares book McCoy refers to is: *The Man Who Would be President*. Mojares was writing about Serging Osmeña as an *individual* and not the Osmeña family as a political family. It is generally known that Serging's father former Philippine president Sergio Osmeña III disapproved of his son's political style. The Osmeñas have yet to show cohesion as a family in politics. Disunity plagues the Osmeña family even today, as when in the May 1995 elections Sergio Osmeña (Serge) became embroiled in a feud with Senator John Osmeña. Even Senator John Osmeña was in conflict with his brother Governor Emilio Osmeña (Capco 1995: 1&5). In the McCoy volume *Anarchy* on the other hand, Mojares discusses the Osmeña *family* as opposed to individual Osmeñas, and in this essay Mojares does not associate violence with their political behavior. McCoy continually confuses the individual with the family which is probably why he confuses my comments as a lecturer at Central Queensland University in a book review with some bizarre Roces family plot to restore the oligarchy!

On the issue of the Lopez family's use of violence, McCoy only cites one example dating from the late 1930s to substantiate his claim. It is not incumbent upon me to provide contrary evidence but for McCoy to offer supporting evidence for such a serious charge which he makes against the family. Incidentally, contrary to McCoy's assertion, until my review I have never had an argument with McCoy about violence, or over

the Lopezes, though my analysis differs from his because I focused on kinship politics and was critical of the Lopez family from this perspective. The Lopez family rose to national prominence from 1945–72, fell in 1972–86, and then rose again in 1986–95. If blatant use of violence is a major instrument for political power and is a critical dynamic of Filipino political families, then where is the reference to violence in the last, most important sixty years, repeat sixty years, the period when the Lopez family gained national political prominence? Furthermore, McCoy contends that if a contributor to the collection in his book showed that violence was not crucial then his analysis “might require some modification.” Apart from the Osmeña *family* discussed by Mojares, what about Ruby Paredes’ account of the Pardo de Taveras (also about individuals and not families)? Surely McCoy is not reading into Paredes’ account that the Pardo de Taveras used an iota of violence, in fact violence was done to them by Governor General de la Torre in 1872 and the painter Juan Luna in 1892.

I am of course concerned with the larger dynamics of kinship politics which may or may not involve violence. But unlike McCoy I do not see violence as an essential characteristic of that political dynamic. There is also a feminine side to kinship politics which the McCoy perspective ignores. In fact women, though not holding the symbols of power themselves, have an important nonviolent role. McCoy’s thesis which sees violence as critical in the rise of a prominent family is also gender blind, ignoring women as powerful political agents.

Finally, to support his claim of my review being ‘invented past’, he is reduced to quoting a man infamous for inventing history, President Ferdinand Marcos. He cites three books of Marcos, lamenting that the late unlamented dictator is “a convenient scapegoat.” Just for the record, I certainly do not claim and I have never claimed that Philippine politics is “the work of a principled elite” nor do I subscribe to the incredible notion that “real political families of the Manila elite are patriots who abhor violence.” Nowhere in my thesis or in my subsequent publications and forthcoming publications do I even remotely posit such silly invented premises. By the way, having lived outside the Philippines for the past eighteen years I have no urge to work towards Roces or Lopez plans to gain power, real or imaginary.

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TO THE EDITOR:

Jeffrey Mass charges me with “hectoring” him (*JAS* 54.1 [February 1995]:162). The events are as follows: my review of Mass’s *Antiquity and Anachronism* appears (52.1

[February 1993]); Gordon Berger, Mass's friend, rebuts it and I answer him (52.3 [August 1993]); Neil McMullin, another friend, chides (53.2 [May 1994]) and I respond (53.4 [November 1994]); now Mass himself retorts. If this presents a history of hectoring, who's the guilty one?

Neither Mass nor his cohorts evidently understand the substance of my review (naming and anachronism, for instance). The way they protest, it is as if they—including Mass—had never read the book itself! A review, and its rebuttals, ought to be addressed to the ideas, not *ad hominem*. My review discusses several intellectual failures of the book and suggests topics that might fill the “Black Holes [sic] in Japanese History” as Mass himself proposes in *Antiquity and Anachronism*. Mass then complains that I invade “a part of the profession that is not [mine].” Wrong again. Aside from the fact that I did not volunteer but was solicited, all intellectual matters ought to, and do, belong to everyone. In fact, the whole problem with this sad exchange derives from a fearful territorialism and self-defensive ghettoism.

I do not claim, on the other hand, to have the authority to “exhort” anyone to “persevere”—as does Mass now, and as did Togo and Tojo for the preservation of their own divine empire—but only hope that we can all express our different views on substantive issues without any reference to our personal roles and stations in the profession.

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