

Communications to the Editor

Leslie Orr's response to Daud Ali's review of *Donors, Devotees, and Daughters of God: Temple Women in Medieval Tamilnadu* (JAS 60.3:906–8):

In his review of my book, *Donors, Devotees, and Daughters of God: Temple Women in Medieval Tamilnadu*, Daud Ali charges that my work is flawed by serious methodological and theoretical shortcomings. I am pleased to have the opportunity to take up some of the issues he raises, in order to rectify what I believe is an inaccurate and unjust characterization of the book.

"The book's most important drawback," in Dr. Ali's view, "is its overreliance on inscriptions to the exclusion of other sorts of evidence" (p. 907). Ali claims that I have entirely ignored textual sources, deeming them irrelevant to the subject matter of my book. This is, of course, nonsense. In addition to surveying in the first chapter all the references in Indic literature to temple women, I have, throughout the book, made frequent reference to textual sources as these bear on the subject under discussion—including such topics as women's property rights, slavery, forms of worship, temple service, initiation, devotion, and "marriage mysticism." I have taken particular care to connect my findings based on epigraphical sources with literary sources that would seem to have an immediate bearing on the temple context with which I am concerned. Consequently, citations of the Sanskrit Āgamas (medieval religious manuals) and of Tamil devotional and hagiographical literature are more abundant than those of other kinds of texts. Therefore, Ali's expression of astonishment that the book "so glibly" rejects the evidence of the Āgamas (p. 908) is perplexing and very misleading. It is not true, for example, that I have said that a discussion of the kinds of terms used for temple women in the Āgamas is "beyond the scope" of the book, as Ali claims; what I actually say (p. 50) is that it is beyond the scope of the book to examine in detail the terminology used in inscriptions from Karnataka and northern India, given that the focus of my study is Tamilnadu.

Ali finds it "curious" that I distinguish between temple women and prostitutes. He seems to believe that this distinction is found neither in the Āgamas nor in recent ethnographic studies of the *devadāsīs*. With reference to the Āgamas, a great deal of further research remains to be done, but it is significant, in my view, that in these texts "prostitute language" and "god language" are scarcely ever conjoined; for example, the term *rudraganikā* ("courtesan of Rudra [Śiva]") is not found in the Āgamas themselves, but only in later compendia and handbooks. I believe that an argument can be made that the "prostitutes" (as well as "virgins" and others) whom the Āgamas describe as taking part in temple festivals should be regarded as belonging to different social categories from that of the temple servants who are referred to as *devadāsīs*. While this may be open to debate, it is certain that Ali is mistaken in believing that contemporary ethnographic literature "suggests that sexual favors also formed a source of [temple women's] income" (p. 908). As I point out in my book, the studies of Frédérique Marglin, Saskia Kersenboom, and Amrit Srinivasan "emphasize the absence of exchange of money for sexual services and point out the opportunities that temple women had to acquire money through other means" (p. 199). So if, as I maintain, medieval temple women were not prostitutes, how did they acquire the wealth they used to become temple patrons? For some reason, Ali says

that among those temple women who were not slaves, most “received no remuneration” (p. 907), and that the inscriptions are silent about how they obtained property. In fact, the inscriptions provide a good deal of information about the ways in which temple women (*except* for women who were slaves) *did* receive support from the temple in the form of land, houses, clothing, gold, or daily or yearly allowances of food or grain (pp. 126–34). It is not at all obvious that the source of a woman’s wealth must be her sexual relationship with a man.

As another instance of my “selective and sometimes myopic use of the sources,” Ali objects to my argument that in the Chola period “there was little overlap between courtly and temple ritual” (p. 907). In this book, I do not suggest that the ceremonies of court and temple were unrelated, or that human and divine lords were depicted in dissimilar ways. The point I sought to highlight in this connection is the contrast between the more distinct definition of these two domains that one finds in the Chola period and the context emerging in the sixteenth and seventeenth centuries in which the two spheres come increasingly to merge. In this later period in South India, the temple woman is a figure clearly visible in both domains—serving two masters—whose ceremonial functions in the court parallel her ritual duties in the temple and whose temple repertoire is composed by court musicians. In the Chola period, however, we do not find any evidence, either inscriptional or literary, that points towards such forms of interchange and movement between the two domains, or the incorporation by the court of the realm of the temple.

“Equally vexing” to Ali is what he sees as my disinclination to consider medieval temple women as “subalterns”; this is especially problematic given “the lack of any discussion of the legal/theoretical boundaries of slavery, service, and bondage” (p. 907). Because of the nature of the evidence I am using, I am actually able to discuss, instead, the *practical* dimensions of such relations of obligation and I devote a good deal of space in this book to such discussions. The inscriptions make it clear that the connection with the temple of female slaves—those women who were sold or given as property to the temple—was on a continuum with that of the temple women who had access to property and privilege (pp. 120–21, 125–26). In part this is because they were all women, and because much of women’s work in the temple, whether menial kitchen labor or attendance on the deity, was unskilled and inessential. But it is also because *all* people who were engaged in temple service or who obtained support from the temple—from potters to priests—were bound to the temple in relationships that involved duties as well as rights (pp. 97, 127–30). Ali’s attribution to me of the view that temple women were exempt from such ties of obligation and servitude is entirely unfounded.

His critique here is tied to his assessment that my approach is “inadequate” “from the larger perspective of understanding women’s history in medieval India.” Apparently I have a rather simplistic and single-minded view of “agency,” which I pursue “at the cost of theorizing any precise location of women within patriarchal structures”—producing, unfortunately, an image of the temple woman “as monolithic as that produced by earlier scholarship.” I cannot help but think that what Ali really means by this is that he disagrees with my findings. His recent article “From *nāyikā* to *bhaktā*: A Genealogy of Female Subjectivity in early Medieval India” (in *Invented Identities: The Interplay of Gender, Religion and Politics in India*, edited by J. Leslie and M. McGee, New Delhi: Oxford University Press, 2000) makes it abundantly clear how different from mine are his understandings of women’s history in medieval India and of the locations of women within patriarchal structures. There are certainly grounds for debate here. Because of the continuing tendency, in scholarly work as

well as in popular stereotypes, to see Indian women's lives as being shaped by structures and a history that are not their own—as being defined and circumscribed, from ancient times to the present, by monolithic normative religious codes of behavior—I have perhaps gone too far in my efforts to demonstrate that women have actually been participants in this history. But it cannot in fairness be said that I have ignored women's exclusion, women's marginalization, or women's oppression. It has been precisely my purpose to show the *mixed* nature of women's circumstances, as these are (literally) inscribed within the institutional framework of the medieval South Indian temple—partly governed by restrictions and partly characterized by liberty of action.

I do not think that the evidence of inscriptions can ever provide us with a complete picture of society, religion or culture, nor tell us “what really happened” in India's history. Whether we rely on inscriptional or literary or art historical sources, there is a compelling need for us to share our findings, to collaborate, to discuss our differences, and to work toward a more comprehensive view. I welcome the opportunity to learn how the gaps in my knowledge might be filled in by the evidence provided by the literature that was produced in the royal courts of the Chola period—in which Ali is especially well-versed. But I do not believe that these, or other kinds of texts, are ultimately to be privileged. In my book, I did in fact refer to many literary and religious texts, but I resisted using textual sources—or a particular text or textual genre—as the interpretive key that would open the door to what the inscriptions mean. I will continue to resist doing this because I regard the epigraphical evidence as capable of giving voice to agents other than those who have composed the Dharmaśāstras, the Purāṇas, and the Āgamas. The stone inscriptions crowding the walls of South Indian temples also give voice to others than those who authored the copper-plate grants and eulogies of kings. These voices are muted—Noboru Karashima has invited us to listen to their “whisperings”—and those who speak, individuals who have gone to great lengths to leave us these records, to record their actions for posterity, were of course constrained in terms of what they could do and how they could talk about it. Nonetheless, *they* thought they were making history, and it behoves us to attend to that fact.

Daud Ali responds to Leslie Orr:

It is perhaps both natural and perilous for any reviewer, in formulating an overall estimation of a work, to try to relate strands of argument and method into a synthesis of critique. Natural because this is what criticism is, but perilous because any reading, after all, is merely an interpretation. I consider myself fortunate to have Dr. Orr's response to my review, as it may help refine my initial opinions, and will also bring further to light dimensions of an important book that certainly merits discussion. There are three points in Orr's response that I wish to take up below.

First, Orr's method. Her focus on inscriptions to reconstruct women's history in precolonial India, is both significant and pathbreaking. Any medieval historian cannot help but appreciate this. In this sense, Orr's book was the first of its kind and will remain a benchmark work. Partly by the nature of her sources and partly from her more comprehensive analyses, her study remains more complete than Kumkum Roy's discussion of women in Sanchi inscriptions (“Women and Men Donors at Sanchi: A Study of Inscriptional Evidence,” in *Position and Status of Women in Ancient India*, edited by L. K. Tripathi, Varanasi: Department of Ancient Indian History, Culture, and

Archaeology, Banaras Hindu University, 1988) and more focused than the more recently published overview of women in inscriptions by Kirit Shah (*The Problem of Identity: Women in Early Indian Inscriptions*, Delhi: Oxford University Press, 2001). The point that I made in the review was that any reliance on inscriptions alone to understand the role and function of women in temple ritual, even more so to reconstruct their experience, without taking into account other types of evidence, remains limited. The Āgamas, among other genres of literature, like *pirapantam* literature in Tamil, bhakti hymns, and secular texts in Tamil and Sanskrit, remain important sources which Orr's book on the whole ignores. Her exclusive focus on temple inscriptions leads her to develop sometimes highly untenable assertions, like her claim that temple and royal ritual remained unconnected spheres until the seventeenth century. Were she to step back from her sources and compare them more widely with available evidence, it would be clear that the connection between courtly and temple ritual was indeed very intimate. Puja itself, it can be argued, from its very inception included various services (Sanskrit *upacāra*) which had their origin in the royal court, and the terminology used to speak of palaces and temples (Tamil *koyil*) and kings and gods in both literary texts and inscriptions is largely identical (the Tamil word *arul* or grace, for example, appears as an auxiliary verb for the actions of both gods and kings). The Āgamas, or manuals on temple ritual, are particularly significant for the "brahmanical" and orthodox perception of temple women, and it was my feeling that she did not take these into account fully enough, if even to offset their perceptions against those of the Tamil language sources. The author's rebuttal of my assertion is also quite selective. I provide the full quote which she refers to selectively above: "It is beyond the scope of this study to interpret the significance of the use of 'prostitute' terms for temple women in inscriptions of Karnataka and northern India or to relate this use to what appear to be parallel references in the Āgamas" (my emphasis, p. 50). It may be argued that it is unfair to fault a book on what it does not set out to do. A book on temple women through the evidence of Tamil language inscriptions can hardly be faulted for its eschewal of other sources. Or can it?

I suggested in my review that the book raised a number of important points that may have benefited from consideration of other types of sources. In particular I raised the issue of prostitution, and this is the second point which I would like to take up. I do not disagree with Orr's fundamental argument, that the *devadāsī* is better conceived of as a temple servant than "sacred prostitute." The language of her sources points to a world of lords and devotees—rather than one of customers, bawds (*kuttaṇṭis*) and intermediaries (*viṭas*) (which we know from courtly and urban literatures)—a world in which women shared with other agents attached to the temple a set of general characteristics, often honorifically eulogized, as "servants of god." The strength of her argument cannot be denied here, and at this level the book remains convincing. But when we turn to other inscriptional traditions and the Āgamas, the evidence becomes more complicated, which seems to beg the question whether the picture is so simple. There seems to be an overlapping of terminology between temple women and prostitutes. In her response, Orr has attempted to explain this problem by distinguishing certain traditions of temple women *within* the Āgama and *paddhati* literature. This explanation is fascinating, but still remains inconclusive to my mind. As for the ethnographic evidence taken from Marglin's study of Orissa, the picture is once again less straightforward than Orr suggests. It is true that Marglin says sexual relationships between *devadāsīs* and the upper water-giving castes of Puri "were not at all like those of common prostitutes not attached to temples." Since *devadāsīs* were supported by kings and temples, according to Marglin, and resided in their own

households, they did not depend on their lovers for maintenance and thus did “not sell their services” (Frédérique Apffel Marglin, *Wives of the God-King: The Rituals of the Devadāsīs of Puri*, Delhi: Oxford University Press, 1985, p. 90). Marglin also tells us, however, that such women did receive “gifts, sometimes very substantial ones” from their lovers. She suggests that such relations might be more accurately described as “concubinage” rather than prostitution based on commercial contract. Yet, significantly, the *devadāsīs* refer to themselves, as do local townsfolk, with words that generally refer to prostitutes (*beśyā, dāri*), which Marglin rather deceptively translates as “courtesan” (pp. 83, 91–92).

I would suggest that Marglin’s conclusions on this point are somewhat hasty, as are Orr’s. While it certainly plausible to make distinctions between women of the temple and those of the prostitute’s quarters, the considerable overlapping of terminology needs to be explained. Marglin’s suggestion that *devadāsīs* were concubines seems inadequate as concubinage in Roman law and later usage generally referred to the permanent *cohabitation* of a man and woman outside the bond of marriage, usually by the introduction of the woman into the man’s household. *Devadāsīs*, however, entertained their liaisons within their own households and maintained separate domestic arrangements. Marglin’s distinction between the *devadāsīs* as concubine/courtesan and the “common prostitute” should have been particularly scrutinized by Orr as a medieval historian. After all, understanding prostitution in precolonial Indian society should take into account the fact that the buying and selling of services, sexual or otherwise, was refracted through feudal notions of property, right and privilege. The texts which speak on the subject like the *Kāmasūtra* mention a variety of types or subcategories of prostitutes—which were in part determined by the various relations that such women could have with their clients. Clearly included within the larger category are precisely the sort of women whom Marglin is reluctant to call prostitutes—women who perform “favors” for single, long term lovers or “patrons” in return for “gifts,” but who maintain their own domestic arrangements (see particularly book 6 of the *Kāmasūtra*). In none of these cases were money transactions with numerous customers necessarily the norm. I cite these contexts for the definition of the prostitutes in the secular literatures not to suggest that medieval temple women in Tamilnadu necessarily fell into any of these categories, but to suggest the variety of roles which were open to prostitute terminology. My larger point was that given the fact that contemporary inscriptions from neighboring regions like Karnataka as well Āgamic literature often mix the terminology of prostitute and temple woman, and that a number of modern ethnographic studies indicate that in some cases temple women developed various types of liaisons with men in return for gifts, can Orr’s conclusion about the absence of such relations be so certain?

In a sense, it comes back to reading the sources. Few inscriptions speak openly of the lives of temple women beyond their role within the ritual economy of the temple. We must rely, critically, on other sorts of evidence, like literary texts, contemporary manuals and inscriptions, and modern ethnographies, to help fill out the picture. Given such a diverse evidentiary landscape, can we read any single type of evidence, in isolation, as a straightforward representation? This is exactly what Orr tends to do in her reading of the inscriptions. The distinctions in the evidentiary terrain for Orr typically represent different traditions, rather than different positions and representations of a single social reality. In some instances, to be sure, such a method is wise, but in others, it robs her analysis of any depth. Marglin, to her credit, is at least willing to posit ideals against realities. Uncovering the ideological character

of the representations of temple women would entail reading against the grain not simply of the Āgamas *but inscriptions as well*. What I find curious about Orr's approach to the issue of prostitution is that it seems as invested in producing a *definitive* position on the temple woman in Tamilnadu as some of the monolithic images she is seeking to complicate. This unnecessarily moors the book to earlier reformist and revisionist debates about the morality of the *devadāsī* institution.

The final point I would like to raise is the question of gender. Orr's book suggests that by studying the inscriptions dealing with temple women that we can recover alternative "whispers" from the past which can give us a new perspective on the history of precolonial India. Here she is surely correct. She uses inscriptions effectively to challenge textualist readings of key issues relating to women in early India. This is a tremendous strength of the book and its approach. Orr demonstrates, for example, that though the dharmasāstric norms generally offer little scope for the autonomous possession of property by women (with *strīdhana* being transferred to the male head of household), the inscriptions present a more complex picture, as temple women did in fact seem able to inherit and alienate wealth. Just as powerfully, her careful study of the role of women in temple ritual clearly undermines the simplistic models associating women and/or dance with sacrality and auspiciousness which have gained a sort of unquestioned status in the study of gender and Hinduism and particularly in Marglin's and Kersenboom-Story's accounts of *devadāsīs*. The weakness of the book, if I may state it clearly once again, relate to Orr's failure to take her findings back to the literature in order to develop more convincing contexts for the study of women in early India. In part, her isolation of inscriptions as a form of evidence relies on the idea that inscriptions represent the voices or agency of women. Her approach to women eschews any reference to patriarchy as a structure, as does her model of Chola society any notion of politico-economic hierarchy. While she suggests in her response that her approach is a corrective to scholarship which has largely ignored the agency of women in favour of monolithic models, she errs, I feel, in the other direction. Orr is able to demonstrate changing roles of women within the ritual programs of temples and to make certain observations about their function, like the fact that their position was optional rather than indispensable for ritual activities. Orr portrays the presence and activity of women at temples as signs of their agency, but admits, I think rather importantly, that their activities at temples seemed to be independent of and irrelevant to their social status. Without using non-epigraphic sources to reconstruct either the subjective worlds of temple women or their place in institutions like the temple and household *as social structures*, Orr's approach seems to lose the larger picture. Hypothetically, it might be compared with a study of women in modern offices which focused on whether women typed, filed, copied, or swept the floors, whether they took paychecks and re-invested in company stocks—all as descriptions of their agency. While such observations are undoubtedly important, without any connection to determining social, economic, and discursive *structures* both within and beyond the temple, such observations reveal a superficial and almost banal notion of agency and by extension, a rather flat impression of how patriarchies as institutions have functioned through time.