



BOOK SYMPOSIUM

The materiality of indeterminacy
. . . on paper, at least

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I would like to thank the contributors and *HAU* for this symposium. It is a great privilege to receive such insightful and challenging comments, which have expanded and clarified my thinking on a range of fronts.

I'll begin with the comment of Stephen Lyon and David Henig because they raise fundamental methodological questions. I am very pleased that they see my book as having significance for discussion of methods for studying organizations in relation to broader social terrains, particularly in Pakistan. They make two criticisms.

The first is that I provided no methodological (as opposed to theoretical) justification for my approach. The book does attempt to develop a systematic method for the analysis of bureaucratic documentary practices in light of theories of semiotic anthropology and actor-network theory. But Lyon and Henig's focus is on the adequacy of my method for the study of the Pakistan bureaucracy and government politics. Here they are right that my justification was much less systematic. Nevertheless, I did articulate several reasons why a focus on documents is productive. First, the early Company documentary practices were extensive and evolved into what came to be known as the Document Raj, colonial precursor to the contemporary Pakistani bureaucracy. Second, Pakistan bureaucrats devote a large amount of effort to document production and circulation—and both bureaucrats and those who deal with the bureaucracy consider these practices consequential to the outcome of bureaucratic processes, in this case, the forms and social organization of the built environment. And third, it was my observation that virtually all activities involving bureaucrats and others are mediated by documents of some kind.

Their second methodological criticism is that my focus on documents led me to neglect the social relationships of kinship, friendship, and patronage that permeate the bureaucracy. Lyon and Henig contrast my method by comparing it to the kinship and patronage focused method of Lyon's (2013) study of land registration and ownership in rural Punjab. In a balanced assessment of the strengths of each method, they argue that each study would have been stronger if it had incorporated some of the other's method. While I can't dispute their call for more comprehensive accounts, I would defend the difference and adequacy of both our methods. Although they see the difference in our focus as originating in our choice of method, I would argue that it has more to do with the social processes and structures we studied. Lyon's (2004) excellent study of local rural politics shows that political processes are dominated by different forms of kinship, community, and patronage. In this setting, it seems likely that documents are rather insignificant mediators of local relationships, so greater attention to documentary practices would be misplaced. In Islamabad, the sort of kinship based conflicts over land prevalent in rural administration are much less common. (Though if I had included a chapter on private houses, kinship would have played a major role.) Lyon and Henig are right that more attention to kin and patronage relations would have strengthened my book, but I see no *prima facie* reason to assume these sorts of relations are as central to bureaucratic process in a new city of immigrants dominated by government service as they are in rural Punjab.

My main fieldwork was diffusely aimed at understanding the social processes that generated the built environment of the city so I was attentive to networks extending within and without the bureaucratic arena. As I later analyzed my ethnographic materials, I came to the realization that the mediations of documents produced social relations that often, even usually, did not correspond to either formal organizational structures or relationships of kinship, regional affiliation, friendship, or patronage. My subsequent decision to focus on documents for the book was more a reflection of this insight than an artifact of an ethnographic research method.

Finally, I think there is value to highly focused thematic ethnographies, though I don't see this as a model for all or even most ethnographic writing. Such works are not silos so long as they theoretically and methodologically open themselves to articulation with accounts focused on other dimensions of social life. It is my hope that mine does.

Béatrice Fraenkel poses many incisive questions on the authority and function of signatures. There are too many for me to address adequately so I will focus on the set of questions around validation and tracing. Drawing on her own research, she makes an important distinction between two kinds of marks: validating marks and tracing marks (those that track time, location, possession, etc.). In Europe through as late as the fourteenth century, the signatures used by subaltern scribes were tracing marks, while great persons used seals, whose main function was to validate documents with testimonial and executive power.

As I reflect now, I had probably not distinguished these functions clearly, because in Pakistani bureaucratic inscription practices each type of mark is so rarely found without the other. Date marks, for example, are always signed. The so-called mark directing a file to another officer is always minimally validated by the receiving officer with a single stroke through it. Furthermore, the powers of validation are not invested only in the head of an organizational division. The distribution of



validating power throughout the officers (grade 17 and above) of an organizational division is manifest in the way they all initial the text of a decision. Petty validation powers even extend to the lower levels of clerks whose work seems primarily oriented toward tracing: if a petition is not registered in an office logbook, it is not a valid petition that an officer must respond to. The diffused power of validation is salient to Fraenkel's question about the relation of participation and legal responsibility. I understand less well how legal investigations proceed in bureaucratic settings in Pakistan, but in the cases I have seen participation entails legal responsibility wherever an individual is in the organizational division.

What does the sociological and functional separation of marks in contrast to their virtual fusion in graphic arrays say about differences in organizations or political arrangements? My hunch is that this contrast points to the degree to which the person at the apex is accountable to other persons. That said, while there are tracing marks that do not validate, I wonder if there are validation marks that do not trace as well. Although the king was not accountable to others for his actions through documents, even the capacity to effectively validate when documents moved beyond the presence of the king would seem to depend on a crude tracing, an indexing of the source of the validation.

Several comments raise comparative questions. Akhil Gupta's ground of comparison is what he calls the "gap" between documents and their referents, which he characterizes as a mismatch between bureaucratic classifications and the recalcitrant classified entities. While exploring this issue through the imposition of European categories in colonial settings, he gives us two factors to explain why this gap is greater or lesser in a particular setting. One factor is the degree to which a government has remade its society according to its own categories, a process Gupta sees as far advanced in European states. The other factor is the degree to which the sociopolitical process generating bureaucratic categories is articulated with the social terrains on which they are deployed. This insight allows us to approach this issue in general sociological terms rather than as only a relation between metropolitan categories and colonial societies. As Gupta notes, US bureaucratic categories are developed through sociopolitical processes largely disconnected from the social worlds of different kinds of marginal populations. Within the colonial world, a differentiated social analysis of articulations *between* colonial settings explains the local dissonance of bureaucratic categories. European bureaucratic categories evolved quickly in their new colonial environments, but they were just as quickly transplanted elsewhere. English property forms poorly suited to Bengal were adapted, only to find new kinds of mismatches when deployed within the land arrangements of Punjab. As Gupta points out, the Indian Penal Code became the model for the Nigerian Penal Code.

I wonder what Gupta would make of the conundrum of Singapore that Michael Gilson poses. The foundations of Singapore were established in 1819 by the English East India Company that ruled it for another forty years before its administration was, as in India, absorbed into a Crown government. I don't know the administrative history of the city-state, but it is, like India, Bangladesh, and Pakistan, probably the inheritor of the Company administrative practices developed through the eighteenth century. And yet as Gilson observes, it seems to be at the "opposite pole" of Pakistan—its bureaucracy reputed to be so efficient and free of corruption that the gap Gupta highlights seems virtually absent. Aside from a

shared colonial administrative history, the differences between the countries of the subcontinent and Singapore are too many to enumerate. Singapore, like Lyon's rural Punjab administration, provides a caution against giving bureaucratic practices too much weight in the explanation of how government works. The explanation for the different character of Singapore's bureaucracy might lie in what Gilsenan calls the close relation between "the political economy and the political economy of paper" or, more specifically, what Gupta characterizes as the degree to which a government formats society in accordance with its bureaucratic categories, especially those of capitalist development. And yet, for all Singapore's apparent differences, Gilsenan's fascinating closing anecdote about documents on display suggests the presence of an orientation to documents that many people of the subcontinent would find very familiar.

Gupta's and Gilsenan's implicit ground of comparison is the degree to which bureaucracies live up to a Weberian picture of an organization of "domination through knowledge" (1978: 225). In her account of the Romanian secret police, Katherine Verdery picks up a more muted strain of Weber's account of documents: their role in constituting an organization. Here the relation of documents to the entities they are mainly about (class enemies, traitors, spies—and even anthropologists) is less important than how they coordinate utterances, perspectives, and activities. In an organization in which secrecy precludes knowledge of even who is working alongside one, the circulation of files provides the channels of communication and the sinews of cohesion. The secrecy and compartmentalization makes me wonder about the issues of tracing and validation raised by Fraenkel, both of which usually depend on some level of organizational recognition of who people are. What stands in place of the signature in such an organization? I look forward to reading Verdery's book.

Justin Richland raises another kind of comparative question. To what extent do buildings and other elements of the built environment have the same capacity as documents to sediment the past in ways that make it semiotically accessible? In other words, to what extent are the material qualities of buildings, like those of some documents, indexical of the histories that generated them? One of my reservations about using the linguistic anthropological term "text-artifact" instead of "graphic artifact" as a term for documents is that it could mistakenly imply that only some artifacts, because of their qualities, serve semiotic functions. This is not the case, as those archive theorists who have tried to define a "document" as something different from all other existing material entities have discovered to their frustration (Buckland 1997). Everything is some sort of trace of something else—a practical problem for archivists with their small boxes!

My book project originally began as an attempt to integrate studies of materiality and signs through looking at built forms in Islamabad and I have been struck by how much of what I learned about buildings has proved valuable for thinking through documents. But what about buildings and the past? Here I would perhaps refuse a general answer about buildings in the same ways I would about documents. Some documents, especially for example, files in Pakistan, are designed precisely to represent their histories. Ideally it should be impossible for them to have a history without it being recorded, that is, for them to be altered in way that would render such alterations untraceable. Other documents, like lists, by design or accident bear almost no traces of their production and circulation. Buildings can



prove to be similarly transparent or opaque to their histories—some more like the pockmarked Pergamonmuseum, others more like an asphalt parking lot.

My book aims to bring together the approaches of Peircean semiotics and actor-network theory, but Constantine Nakassis' discussion of materiality highlights an important difference between these two approaches on the question of the material. Nakassis has produced a thicket of arguments too dense to engage in detail here. So let me concentrate on what I take to be the main issue. Nakassis develops a concept of materiality within a Peircean semiotics. Peirce has been immensely helpful in showing how signs are linked to the world (Keane 2003), but his semiotics is broadly speaking a species of Kantian epistemology, an account of how we know the world. In one of the earliest formulations of his semiotics Peirce elaborated a "new list of categories," whose function is to "reduce the manifold of sensuous impressions to a unity" (1991: 24). Although he considered the notion of a thing in itself self-contradictory, signs only represent reality, which is fully known only at the theoretically unspecifiable end of a process of inquiry by a scientific community. Understanding this can help us see the broad problem with trying to define materiality within a Peircean framework.

In Nakassis' account, bureaucrats, villagers, and others play the role of Peirce's community of scientific inquirers, discovering new qualities of what he calls the "experientable or intelligible" materiality of graphic artifacts through novel semiotic engagements with them. Although Nakassis concludes from this, "the ontology of materiality . . . is epistemological," his account, like Peirce's, can only characterize an experienced materiality. It must leave open the possibility of an unexperienced (if not unexperientable) materiality, matter waiting for the imposition of form. This unexperienced materiality is a substance lying beneath the experienced qualities, for example, theoretically inexhaustible unknown material qualities of documents that await encounters that will reveal them.

In contrast, my own view of materiality is in line with actor-network theory (Latour 1999 and Whitehead 1978), which does not leave a Kantian place for qualities of things that are outside history. In actor-network theory, as in Nakassis' view, events are central. But they are not occasions for the discovery of the properties of a material substance standing beyond events. Rather, an artifact gains or loses material qualities and stability as events remake its relations with people, ideas, times, processes, other things, institutions, God, et cetera. Just as it would be strange to say a file says something that is not yet written on it, it makes little sense to say, for example, a list has material qualities that enable fraud before someone has actually pulled it off—or related (Mol [2002] would say "coordinated") this list to other lists that have been so abused.

Finally, Naveeda Khan raises the question of "the political." I am skeptical about the notion of "the political" as a "domain," whether defined in relation to states, "the city," corporations, things, or infrastructure. It seems to me that if we leave politics somewhat ill-defined in general we can better see it when it crops up in particular, unexpected forms. That said, I certainly agree with Khan that however politics is characterized, it should include roles for equivocation and indeterminacy. However, I am puzzled as to why she finds discussion of them absent from my book. While Khan seems to conflate equivocation and indeterminacy, let me point out where I see the book addressing each respectively.

First equivocation. Since the concept of equivocation is anchored in the linguistic performance of an individual, “contention” might be a better term to characterize the fields of debate Khan finds in the work of Agrama and Robinson. There is more in my book about contention (or Bakhtinian “dialogue”) than equivocation: contentions over whether the new capital was built to serve the Pakistan people or simply the wealthy and well-positioned; whether illegal mosques are the product of piety or greed; whether individuals or groups should be compensated for expropriated land; whether residence or participation should be the basis of mosque membership; whether government housing should be allotted on the basis of kinship or bureaucratic position. Most of the utterances of participants in those disputes seemed to me to be rather unequivocal, but in other cases I argued that equivocation was at the heart of political processes. The chapter on files is about the techniques of equivocal writing that contribute to the indeterminacy of responsibility. In my treatment of petitions, I distinguished political subjects as analytic types only to show the ambiguity generated in practice when people combine them using genres of writing, language, graphic layouts, and interaction styles that index different kinds of subjects. This was in fact my point in the example Khan cites of the man tearfully begging for a promotion as he presents a petition to his officer. In my account of land expropriations, I showed how villagers bemoaned the fact that the government forced them to engineer fraudulent schemes, which they see as at once debasing, just, and economically necessary. On the other side of this conflict, I portrayed an official who righteously fought these fraud schemes even as he wearily admitted that these schemes ameliorate grossly unjust compensation rules.

Second, one of the larger arguments of the book is that the prevailing view of writing as fixing relations among people, things, places, et cetera should be revised to accommodate the indeterminacy that the mediation of documents often generates. And I show that this indeterminacy is central to political contestation within the bureaucratic arena. For example, the document chains used to qualify villagers for compensation for expropriated homes and land rendered the determination of residence within expropriated areas virtually impossible, which created the opening for a particular kind of intervention. Similarly, the indeterminate relations between the methods used to measure land for expropriation and those used to record ownership opened up the expropriation process to villagers who could resist government commensuration efforts. Beyond document-based disputes, I described how officials and others speculated about who is really behind a proposal, who might be supporting a particular person, if the head of an organizational division is really demanding the bribe his underling says he is, and whether a decision was made to favor some party or according to the rules. In short, I tried to show that uncertainty is a pervasive component of actions and their interpretation within the bureaucratic arena.



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