Viewing the Holy City: an Anthropological Perspectivalism

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I started my investigation into the question of Jerusalem and disciplinary *foci* by looking at a couple of classic analyses of the anthropology of the Middle East, specifically Michael Gilsenan's "Very Like a Camel: the Appearance of the Anthropologist's Middle East" (Gilsenan 1990) and Lila Abu-Lughod's "Zones of Theory in the Anthropology of the Arab World" (Abu-Lughod 1989). Interestingly, but not surprisingly, I discovered that the models they sketched out, both historical and contemporary, did not really encompass the anthropology of 'the Holy City'[1]; whilst there might be occasionally appropriate characterisations, focussed on particular approaches to Jerusalem's populations and their habits, the city in general did not appear particularly to fit the *topoi* of the anthropologists' Middle East. This may, of course, be largely a matter of the anthropological study of the Middle East having been, until recently, largely an engagement with rural, rather than urban, communities. It will, of course, also have much to do with the fact that Jerusalem is not only a city on the ground but as well -- and more than most -- a city of multiple imaginings, not only national and nationalistic but also global and transcendental. Nonetheless, disciplinary training does provide its students with conceptual toolboxes, and it is interesting to observe what has been -- and can be -- drawn from past anthropological traditions to apply to contemporary Jerusalem.

While Gilsenan's observation that much of Middle Eastern anthropology focussed on the village or tribe might seem apt for characterising those studies which carve out for attention particular communities from the urban congeries, isolation from currents both of history and of the surrounding milieu does not generally characterise the study of particular Jerusalem communities. This, of course, is in part because urban anthropology generally problematises such isolation and in large part because Jerusalem, both in pre-modern and modern terms, has been and is profoundly compounded and transnational. Abu-Lughod's assertion that Islam, the harem, and segmentation have focussed the discourse of Middle East

[1] Gilsenan argues that the village and the tribe, conjugated around the issue of a disappearing traditionalism, has marked traditional anthropological studies of the Middle East, and notes that the historical -- as an active force in shaping identities rather than just a corrosive displacement -- has virtually been excluded. Abu-Lughod's contention is that segmentation, the harem, and Islam have been the dominant tropes in the (largely male dominated) anthropology of the Middle East, and that while Middle Eastern women have been the focus of many texts these women have been oddly 'haremized' and insulated from the wider currents of Middle Eastern life (rather in the same manner as the Middle East, except when the focus of policy studies, has been itself secluded in orientalist isolation). Lindholm's 'The New Middle Eastern Ethnography' (Lindholm 1995) reviews six recent anthropology texts relating to the region, but none are here more than incidentally relevant.

anthropology has resonance in Jerusalem although it does not quite work for a situation in which Islam, contested by the co-presence of both Judaism and Christianity, is generally presented -- at least by Western commentators -- as a 'problem' rather than as a socio-cultural determinant, in which the harem is generally seen as having disappeared with the death of Solomon (although some work on ultra-orthodox Jewish communities is tinged with an 'haremic' concern with the sexual, albeit tempered by psycho-medical discourse), and in which segmentation, while serving as a model for understanding inter-communal and intra-communal divisions (see, for instance, Webber 1985), has also been critiqued as an inadequate tool for encompassing the complexities of processes of political and cultural definition (see my retort to Webber [Bowman 1986]).

Furthermore, the 'absence' of history in Middle East societies -- or rather the 'stranding' of Middle Eastern societies in a atrophied traditional past -- is far from the case in perspectives on Jerusalem, although the role of history is there rather complex and problematic. Jerusalem has, if anything, *too much* history, and one result of this has been a problematic bifurcation of anthropological studies of the city. On the one hand, one finds an anthropology of an historical past; there is a wealth of material, both literary and archaeological, on the many Jerusalems of the Biblical and post-Biblical pasts, and this draws scholars to apply to it 'anthropological' methods. Thus a brief scan by 'Google Scholar' for references to texts treating Jerusalem anthropologically reveals that a significant proportion of those found are anthropologies of biblical or ancient history[2]. On the other hand, 'history' -- in the sense of politically significant developments -- constitutes another Jerusalem for anthropologists. Here we discover a political anthropology which seems dedicated less to the close observation and analysis of social and cultural configurations than to setting out political problems and proposing solutions to them, or at least suggesting means of accessing alternative futures to those dire futures conditions seem to predict. The products of such endeavours appear either as current event reports in more journalistic disciplinary outlets such as *Anthropology Today*[3] or as contributions, often not publicly evident, to 'think tank' work around the issues. Here anthropologists are at risk of becoming simply one cadre in a phalanx of experts engaging with the 'issue' of Jerusalem or the wider issue of 'the Middle

[2] for instance "The Anthropology of Clothing in the Joseph Narrative" (V. H. Matthews, *Journal for the Study of the Old Testament* 1995) or "Anthropological Evidence of Interpersonal Violence in First-Century A.D. Jerusalem (J. Zias, *Current Anthropology* 1983).

[3] I note here *Anthropology Today* articles from the past ten years on Jerusalem from the same 'Google Scholar' search: Efrat Ben-Ze'ev and Eyal Ben-Ari writing on 'Imposing Politics: Failed Attempts at Creating a Museum of "Co-Existence" in Jerusalem' (1996), Ulf Hannerz, 'Reporting from Jerusalem' (1998) not to mention articles by Jonathan Benthall, Emmanuel Marx, and others which refer to the 'problem' of Jerusalem.

East', abandoning wider perspectives to focus on specific problems. As Gilsenan warns, discussing an earlier emergence of 'Area Studies' (one seemingly being revisited with current US and UK strategies on developing Middle East 'security expertise'),

> the emergence of 'the expert' is central here. Knowledge of contemporary events and the structures or systems held to underlie them became critical...Such an evolution tended to direct students away from anthropological theory and more towards the idea of expertise....Debates tended to become internalized to Middle Eastern studies, and it was all too easy to find oneself in a ghetto" (Gilsenan 1990: 229)[4].

The question then becomes one of whether or not one is doing 'anthropology' in the sense of close ethnographic observation and analysis brought into productive conjunction with comparative insights or whether one is engaged in journalism, policy studies, or a combination of the two. The question also surfaces of what of Jerusalem itself is visible if it is approached as simply the setting of contestation or the plum to be fought over in a vicious war of civilisations.

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In writing the above I have found myself troubled by a failure to bring to mind many notable examples of what I would consider contemporary anthropologies of Jerusalem[5], whether these be in article or book form. Earlier work like Azarya's study of the Armenian Quarter is good, and there are anthropological elements in other interdisciplinary projects (for instance Romann & Weingrod 1991 and Spolsky & Cooper 1991) which are useful despite being essentially surveys rather than ethnographies, but material is sparse. When I was a Lady Davis Fellow at the Hebrew University in 1983-1984, I would hear anthropologists in the Sociology and Anthropology Department telling me that, apart from Azarya, no anthropologist worked in Jerusalem, either the Old or the New City. Possibly this was

[4] I would stress that I am not in any way questioning the vital necessity of political analysis of the situation, especially insofar as anthropological work -- which usually entails a more sustained face-to-face relation between researcher and researched -- is likely to make visible conundrums and brutalities more positivist research effaces. I am, however, implying that the focus of such studies excludes informations which may, in the less immediate term, be vital to understanding fundamental characteristics and underlying structures of the site.

[5] Because of my own field research I tend to think of Jerusalem's Old City when I speak of Jerusalem, but as Laurie King-Irani pointed out in comments on a draft of this paper such a delineation is somewhat artificial: 'Why just the old city? Does it not articulate economically and politically with the rest of the city, and now, thanks to Israeli expansionism, a huge swath of territory never considered to be part of “Greater Jerusalem” until about two decades ago? And if we want to take the Greater Jerusalem idea to its maximal expression, then it encompasses the entire Muslim, Jewish and Christian worlds, in terms of collective representations." Perhaps the remainder of the paper will explain why my vision is so delimited, and why such delimitation may be linked to processes of carrying out field research.

because it was too close (one looked down on the Old City from the Senior Common Room at Mt. Scopus) and the 'anthropology at home' paradigm had not yet caught on, and possibly it may have been because any 'anthropological' study of Israelis would be seen to demean them (see Dominguez 1989). Research on Palestinians was politically problematic either because leftists didn't want to denigrate them by separating them out as objects of study or, more likely, because Israeli academics -- with the exception of an earlier generation's such as Abner Cohen, Emmanuel Marx, Henry Rosenfeld -- were simply nervous about working with the enemy. Generally, the anthropology of Palestinians by Israelis is a deeply murky area (see Rabinowitz 2002) and it may be that many simply wanted to stay clear -- or to appear to be staying clear. In the meantime excellent work was being done, and continues to be done, by Palestinian academics I would describe as anthropological, such as Salim Tamari (Tamari 1983; 1991; 1992a; 1992b; 2004a; 2004b), but this work tends to be either archive-centred or social demographic. Frankly, problems of access (far worse now than in 1983-85 when I first worked out there) have made Jerusalem an impossible object of research for Palestinian academics and, then as now, the demographic and social work of charting the impact of occupation is more important than seemingly more arcane ethnographic research. Here the issue of 'too much history' is very much too the fore.

Curiously, however, Jerusalem has also not seen the kind of interest from foreign anthropologists that its salience would lead one to expect. I was involved a few years back, at the Van Leer Institute and later at the American Anthropological Association meeting in New Orleans, in a for the most part anthropological series of seminars on Palestinian-Israeli mixed towns organised by Daniel Monterescu and Dan Rabinowitz (this is the context in which I first met Madelaine Adelman). The seminars heard excellent papers on Haifa (2), Jaffa (2), Acre, Nazareth, and Lydda, but the one paper on Jerusalem (presented by Tamari) was archival and demographic. This gap -- this empty centre -- seems somehow indicative, and, although I am aware of two anthropologically trained scholars of Jerusalem currently producing valuable work[6], I would like to see more work -- and more diverse work -- being carried out.

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As readers here are undoubtedly beginning to note, I have a quite specific criterion for judging whether or not work is suitably anthropological. Unsurprisingly this is one very central to the tradition of British Social Anthropology -- that initiated by Branislaw

[6] Tom Abowd, who finished his PhD at Columbia in 2002 (Abowd 2002) and is publishing in *Middle East Report* and the *Journal of the Institute of Jerusalem Studies* and Rochelle Davis, who also completed in 2002 (Davis 2002), and who has published in several books and journals on Jerusalem history.

Malinowski -- and has at its core an insistence that good anthropology depends on an extended period of intensive participant observation research amongst the people or peoples one studies. Such work, which usually entails learning a local language and living amongst (and in the same conditions as) your subject population, is intended to ensure that the anthropologist becomes aware of not only the quotidian habits and assumptions of the people he or she works with but as well as of the central concerns and conditions of their life. Anthropologists, or at least those of this particular 'school' of anthropology, contend that other methods of social research -- such as those based on questionnaires and surveys -- predetermine the researcher's findings insofar not only as all directed questions delineate a particular range of reply but also because informants -- be they nervous, accommodating, or desirous of reward -- are very often likely to tell the researcher what they think he or she wants to hear.

Essential to the process of anthropological fieldwork is, strangely, the necessity of getting lost; lost not so much in place (although we too are good at that, and learn from finding our ways back out) but lost in terms of not knowing precisely what one is doing. I can do little better here than to relay a story that Godfrey Lienhardt, one of the central figures at the Institute of Social Anthropology in Oxford, where I did my postgraduate work, used to tell about fieldwork. He would tell aspiring anthropologists that they would, after a year or so of lecture, seminar, and library preparation, head to their respective fields (his had been the Sudan) with their brains full of theories, of hypotheses, and of scholarly descriptions of their peoples and their contexts. For the first six months of fieldwork, he would say, everything would seem tremendously exciting and satisfying; what informants would relay would tie in beautifully with expectations, offering strong support for the theses you were intent on demonstrating. Then, one day, someone would say or do something odd and -- Lienhardt would say -- 'if you were good' you would take note of it. Once sensitised to such inconsistencies, you would become aware of them popping up more and more often, and as you noted them down -- and tried to think them through -- they would begin to erode your assumptions until, somewhere in the second six months of field research, you would find yourself sitting in your tent profoundly depressed that you knew nothing, that you understood nothing, that all of the theories you'd carefully packed along with your luggage seemed unworkable, and that you just wanted to go home. This, Godfrey would say, was the moment real anthropological work could start as, over the next year or so, you reconfigured your assumptions more in line with what you were seeing and hearing around you, reconnected (and rethought) the theories you'd brought out with you with the materials you were now trying to explain, and put together something which provided real insight into the

lives of the people you were living with. I've tried to analyse in some detail the implications of this anecdotal rendition of fieldwork in a couple of essays (see Bowman 1997 and Bowman 1998) but actually the story itself is hard to fault as a guide to issues of anthropological observation.

Let me briefly expand upon it with reference to my own experience doing research in the Old City in the early to mid-eighties. I, like many anthropologists of my and previous generations, had come to anthropology from literature; in my particular case I had been drawn to the study of Jerusalem by an interest in two coexistent and ideologically incompatible forms of literary travel -- pilgrimage and mercantile -- popular in the late Middle Ages. Invited to move from literary to literal Jerusalem pilgrimage by Erik Cohen, an anthropologist at Hebrew University, I came out in 1983 and, after three months of living in West Jerusalem, managed to move into the heart of the Old City, taking up residence in a housing compound populated by Christian Palestinians who had been driven from West Jerusalem in the 1948 war. Certainly I worked on pilgrimage practices during the sixteen months I lived over the 8th Station of the Via Dolorosa -- I travelled across the country with a number of different pilgrim groups from different nations and different Christian denominations, attended the many feasts of the plethora of Christian sub-communities who celebrated the place, and spent countless hours sitting, watching, and interviewing in the *Anastasis* and numerous other churches and institutions -- but I also lived with my neighbours, perched in houses and shops talking and drinking coffee or *arak,* hearing them speak of their pasts, their family problems, their relations with tourists and soldiers, and their dreams and nightmares while they drew me out about who I was and what I thought. I also spent hours walking the streets of the Old City, accompanied or alone, day and night, coming to see it as a very different place than I'd read about or even perceived on my first visits. Although I certainly ended up writing about Jerusalem pilgrimage (Bowman 1991), I also found myself drawn -- through identification with informants and neighbours and psychological investment in the terrain we shared -- into an involvement with the dilemma of Palestinian tourist guides (Bowman 1992) as well as, amongst many other things of which it has proved the most notorious, a bemused fascination with why street merchants were obsessed with stories about sex with foreign tourists (Bowman 1989 and Bowman 1996). Fundamentally, long term engagement with a population in place brought me close to being able to see the place as they did without occluding the 'other view' with which my scholarship and my origins in a different place and culture provided me.

I stress here the importance of integrating the two perspectives the anthropologist builds up on a place -- that of local vision based on engaging, and engaging with, as wide

portion of the local community as one can, and the analytical knowledge of a 'stranger' who compares and contrasts what he or she is learning to see with a broad repertoire of theoretical and comparative materials pertaining not only to the social setting studied but to societies more generally. I say this in part because I am not trying to promote a naïve and romantic view of the anthropologist as someone who, shedding the baggage of his or her cultural past, 'goes native' and is thus offered access to a magical alterity undiscerned by the rest of the world. Anthropological knowledge builds both on ethnographic awareness of the particularities of the specific cultures studied and a learned ability to see those particularities in terms of structures, functions, and relations which are analogous to, or variations on, those operating in the other cultures of the world. The 'conceptual toolbox' I referred to in the opening paragraph of this paper empowers the anthropologist, through measuring and evaluating a 'play' of difference and similarity, to make sense of what he or she sees or is told in terms of theories and models other anthropologists know, share, and use to communicate. The tools in that toolbox are sufficiently stable to allow common usage but simultaneously manipulable enough to allow them to be put to uses not precisely those for which their fabricators imagined they would be used; observers of the histories of anthropological theories note a perpetual process of adaptation and transformation of those tools as anthropologists -- who Levi-Strauss would refer to as *bricoleurs* -- work with and on them to make them serve to make sense of new encounters or perspectives (see Moore 1999). Disciplinary involvement (which entails common access -- and the occasional contribution -- to its conceptual toolbox) means that anthropologist, 'lost' in the field, needs to work on the one hand on translating what he or she sees into something similar to what he or she has been trained to recognise and on the other to adapt the models and theorems learned so as to be able to incorporate the novel situation without doing it violence. This labour results in a dialectical relation between the known and the novel; one sees the everyday illuminated against a backdrop of theory and theory evidenced in the setting of the everyday.

In my case I came to Jerusalem looking to investigate foreign pilgrims intent on finding on the ground a palpable rendering of a city they knew in song, text, and prayer. I found them -- many of them -- but I also found not only the complex social, economic and psychological structures of close-packed communities variously shaped by living in a city held sacred by others but as well structures of acquiescence and resistance thrown up in response to barely masked intercommunal warfare. The 'scene' was far more complex than anything I had imagined while reading about Jerusalem in Oxford (or in my first few months in the city), and that complexity was one of the inseparable interaction of a number of elements rather than simply one of multitudinousness. Thus the work I ended up doing on,

for instance, Palestinian street merchants and foreign women involved not only observing the disparate workings of tourism and family life in the Old City, seeing the ways merchants worked and competed in the everyday tension of a market economy dominated by buyers rather than sellers, coping with and gossiping about male competition in situations where local women were rendered inaccessible, noting the not always subtle signs marking the differential power of Israelis and Palestinians (and the gender symbolism of that differential), and listening to the vagaries of language use in conversations between merchants, pilgrims, tourists, and Israeli shoppers. This was not research I carried out by delineating a problem, determining to study it, setting out my methods and parameters, and collecting data; it was work I came to find myself driven to engage after several months of sitting with close friends in their shops, listening to their competitive banter, lusting with them after women who we could envisage as available to us, observing -- and hearing stories of -- interactions with tourists as well as Israeli guides and shoppers, and watching the way sexual politics were both imbued by and overturned by a barely spoken national politics. The material -- descriptions of bargaining sessions, sordid stories of small conquests and angry failures, descriptions of mythic Israeli seductresses and boys shamed for 'taking what was not offered' -- collected in my notebooks but also in my memories; this, via my connection with my friends, my growing anger at the occupation, my dismay at the sublimation of politics in the discourse of the market traders, and my own sexuality became a situation I needed to understand, and my anthropological knowledge enabled me -- in time -- to do so[7].

In another melding of moments and concerns, in this case giving rise to my 'Nationalizing the Sacred: Shrines and Shifting Identities in the Israeli-occupied territories' (Bowman 1993), my original project of working on Christian pilgrimages morphed into a study of local Muslim-Christian interaction around holy sites mutually revered in the period leading up to the first *intifada*. Again, my own life in a place, living over an extended period of time in close communion with a people whose practices and concerns I came to share, drew my attention to, and emotively involved me in, activities I had not planned to look into. These called on the scholarship I had prepared in anticipation of my planned work on Jerusalem pilgrimage, but drew that scholarship into new relations it at the same time was able to throw light on structures of interrelation which were not visible to the local eye. My current work, on 'shared' or 'mixed' shrines in Macedonia (FYROM) and West Bank Palestine has grown out of this work, and the resonances I could perceive of it in my subsequent

[7] Interestingly, and I suspect somewhat analogously, a close friend raised in the Christian Quarter by poor Catholic parents and eventually sent off by the Franciscans to study psychoanalysis in Paris also found himself working on and through the issue of 'fucking tourists' when unemployment stranded him back in the *suq* of the Old City.

experience of driving through war-torn Yugoslavia

This 'binocular' (as distinct from schizophrenic) vision is, I believe, particularly anthropological, but failures to bring its two lenses into alignment will produce particularly symptomatic faults in anthropological commentators. One fault is that of 'over identification', a fault which is, particularly in war zones such as that which has engulfed Jerusalem over the past few decades, easily succumbed to. Here one commits completely to the position taken by the community one has adopted through one's research, to the extent of uncritically accepting its assumptions about and positions on its dilemmas[8]. I want to stress that I am not criticizing positioning oneself politically in activist terms in alignment with a community one has 'adopted' in the course of research; knowing, better than most outside that community, the situations it faces may in many cases call on a scholar to struggle against those depredations. I have certainly, in relation to the position of the Israeli state towards the Palestinian people, taken sides. I am instead criticizing an unwillingness to be critical of the strategies and assumptions of the community one works with when those appear counterproductive or simply wrong-headed to the academic[9]. As an outside scholar one brings into a particular situation knowledges drawn from other sites and situations which may be of value in working through conflicts or contradictions in the 'native' position, and a self-abasing celebration of the rightness of the other which forbids one from contributing those insights to internal debates damages not only the anthropologist's integrity but also the possibilities for self-assessment of the community.

The other fault, probably less overt (and more masked) in these days of reflexive anthropology than it appeared in its traditional form, is the insistence on seeing the subjects with whom one works as the objects of a detached and 'objective' knowledge. This is a failure of identification and, whereas in other locales of anthropological study it has often been evident in the 'modern' anthropologist's disdainful distance from the 'primitives' he or she studies, it can easily be found in less scientific guises in work around Jerusalem where scholars take an orientalist, or even racist, 'distance' from communities of 'Arabs', 'Jews', *mizrahim*, 'Orthodox', 'Bedouin' or the like. Said's comments on the politics of orientalist academia (Said 1978) need no elaboration before this audience, but their application to 'scientific' objectivity when it serves to mask ethno-nationalist or communitarian disdain is

[8] See here the debate between Moshe Shokeid (Shokeid 1992) and Ted Swedenburg (Swedenburg 1992) sparked by the latter's earlier "Occupational Hazards: Palestine Ethnography" (Swedenburg 1989).

[9] 'Fucking Tourists', given to my mates on the street, was 'therapeutically' critical (and taken as such) while my 'Exilic Imagination (Bowman 1999), presented at Birzeit University in 1998, attacks the nationalist mythification of Palestinian history as well as the condescension of 'returnee' Palestinians in their relations with those who'd remained under occupation.

not always so obvious.

You will note that I have not at any point cited 'partiality' as a fault. Fieldwork entails working closely in a particular setting, and the description I have provided in the preceding pages makes it clear that identification with the inhabitants of that setting in the specificities of their situation is an essential part of the work. I will allow that settings can be occupied by a number of different groupings and would note that, in my own work, I was brought into frequent, and often close, contact with diverse groups of people -- pilgrims, priests, tour groups, and locals of various faiths and political allegiances. Many of these groups shared little if any overlap, and I moved variously amongst all of them. I would also note that situational identifications are characteristic of our social lives, and that some ethnographic work may depend precisely on limning out and closely describing the experiential world of a group one shares for a relatively brief period of time; the vignettes in my study of three denominationally distinct pilgrim groups in the *Contesting the Sacred* volume (Bowman 1991) methodologically do precisely that.

In this paper, however, I have been describing longer immersions, and stressing the benefits of being engaged with a community for a sufficient time to enable one to begin seeing differently, and more complexly. In that immersion one is likely to identify predominantly with a circle of persons with which one is most closely integrated on a daily basis in one's relations, in sharing their surroundings -- public and private, and in engaging with and imbuing their perspectives and concerns. That group might be diffused and might conjoin persons or groups which would be considered, in academic terms, categorically distinguished (for instance Christian and Muslim Jerusalemites). Categorical borders are not the salient issue however; ethnographic life consists in large part of following local networks -- often across what seem to be boundaries, and observing the ways in which those networks stitch together individuals, groups and small communities. Such networks however are unlikely, except when necessity intervenes (in the form of events such as breakdowns, demands for the payment of taxes or calls to the cells for interrogation), to carry their inhabitants into the domains either of strangers or of those I would call 'antagonists' (see Laclau & Mouffe 1985: 92-148 and Bowman 2006: 33-34 and passim). As a consequence the ethnographer, while *knowing of* those on the 'outside' of the community he or she cohabits (whether they be enemies or just 'others'), is not likely to engage with their everyday lives and concerns. Close fieldwork with a community will teach an ethnographer much about the ways the members of his or her community characterize those beyond the border of the community, but it will not -- unless the anthropologist breaks away from his or her primary group to live with the others -- give any sort of access -- intimate or otherwise -- to the life-world of the outside

community.

Ethnographic knowledge is by its nature perspectival, and profoundly partial; the wider theoretical and anthropological knowledge the anthropologist carries in the aforementioned tool kit may allow that partial knowledge to be contextualised, and even juxtaposed with the similarly partial life worlds of other groups cohabiting the place studied[10], but it cannot produce an image of the 'real' city all of these groups share. This is because, at least in the experiential sense, there is no 'real' Jerusalem which can be caught in any single representation; Jerusalem is the compound setting of the life worlds of the peoples that cohabit in it whilst engaging it, and each other, differently.

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One cannot, before the interdisciplinary conference this paper is meant to address convenes, reach strong conclusions about the cross-disciplinary importance of an anthropological approach to understanding the city and its peoples, certainly not without having heard the representations of those of other disciplines. I have, however, criticised some of my own colleagues for pre-determining their findings by restricting the queries they direct towards Jerusalem and Jerusalemites. This is a danger all disciplines, by their nature as disciplines, are prone to, and I have tried to suggest that extended participant observation may suggest ways other scholars in other fields might escape the deductivism which lurks at the heart of most if not all of our methodologies. Some close forms of archival work, in history in particular, have begun to reconstitute what appears to be the life worlds of previous inhabitants of the city, although that work is of course held to account by questions of who (which groups?) had access to language and how much language can carry of the implicit shapes of the times and lives it claims to body forth. I have also regretted that so few anthropologists have worked on Jerusalem and its environs; there is a feeling, I suspect, that certain field sites belong to those scholars who have laid claim to it and that others should not seek to work there, unless to seek to prove the precursor wrong.

Perhaps I can close with a lesson taken from the struggle between Margaret Mead and Derek Freeman over the real character of Samoan sexuality and society (see Shankman 1996 and Côté 2000). Freeman dedicated his life to proving that Mead was an opportunist and a liar in what she said she had learned about Samoan life from a circle of young girls in the 1920s; he worked on Samoa, largely in the 1950s and 1960s, amongst a circle of male tribal elders, and collected from them very different versions of sex and life on the island. Freeman's error, and

[10] Friedland and Hecht's *To Rule Jerusalem* (Friedland & Hecht 1996) comes as close as anything I know to doing this convincingly and, while it is not an anthropological text in the formal sense, its authors' methods, involving intensive interviews, approximate one.

to a lesser degree that of Mead, was to assume that there was *a* Samoa on which all persons accorded their ways to *a* moral code. He was not only in error because three decades of rapid modernisation had changed Samoa inexorably, but also more centrally because he circulated with, and imbued the ethos of, a group of individuals profoundly different in age, gender and status from those Mead had earlier chatted with in the course of women's work and play. As we can see in retrospect, both Mead and Freeman were right, and wrong. Both saw aspects of Samoan society which were truths in their own context and partial truths in the larger context of an island culture in history. Had Freeman not -- rather unprofessionally -- decided to destroy Mead's reputation, he -- like later anthropologists have -- would have been able to work out how the two apparently incommensurate worlds could inhabit the same small place within a small span of time. Samoa, like Jerusalem, is a place 'compacted together' and the more partial views of the sites we gather the better an understanding we can derive of how the lives lived there fit, and sometimes clash, with each other. Only through a gathering of multiple views, carefully and painstakingly gathered and collated, can we begin to get a sense of the singular multiplicity that is Jerusalem.

I can do no better than to close with a wonderful phrase by the master of perspectivalism:

> There is only a perspectival seeing, only a perspectival "knowing"; and the more affects we allow to speak about one thing, the more eyes, different eyes, we can use to observe one thing, the more complete will be our "concept" of this thing, our "objectivity." (Nietzsche 1994 [1887]: 92)

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