Serendipity in Anthropological Research
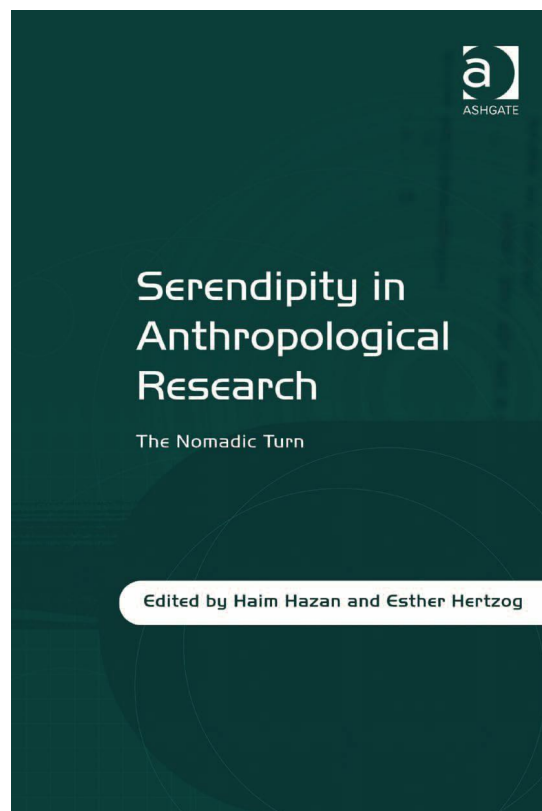
The Nomadic Turn

Haim Hazan & Esther Hertzog

May 22, 2017

Contents

[Front Matter]

[Dedication]

For Emanuel Marx, a mentor of nomadism with an anthropological home

[Title Page]

Serendipity in Anthropological Research
The Nomadic Turn

Edited by
Haim Hazan
Tel Aviv University, Israel and
Esther Hertzog
Beit Berl Academic College, Israel

[Copyright]

Contents

List of Figures and Table

Figures

Table

List of Contributors

Richard Werbner is an Emeritus Professor in African Anthropology and Honorary Research Professor in Visual Anthropology at the University of Manchester. Among his books are *Ritual Passage, Sacred Journey* (1989), *Reasonable Radicals and Citizenship in Botswana* (2004) and *Tears of the Dead* (1991). The latest of his four films is *Holy Hustlers* (2009).

Ugo Fabietti is a Professor of Anthropology and Director of the Ph.D. programme at the University of Milan-Bicocca, Italy. He taught in Turin, Pavia and Florence Universities as well as in Paris (EHESS) as a visiting professor. He carried out fieldwork in Saudi Arabia, Iran and Pakistan, and travelled in Africa and Madagascar. He is the author of books and articles on the Middle East and anthropological theory.

Emanuel Marx, a social anthropologist, is Professor Emeritus at Tel Aviv University. He has studied Bedouin in the Negev, Israel; a new town in Galilee, Israel; Bedouin in South Sinai, Egypt; and Palestinian refugee camps in the West Bank and Gaza. His main research interests are pastoral nomadism, bureaucracy, prehistoric man in the Levant, and power and violent behaviour.

Haim Hazan is a Professor of Social Anthropology at Tel Aviv University and editor of *Israeli Sociology*. His main areas of research cover the life course, time, cultural and interactional aspects of old age, total institutions, community and nationalism. Among his books are *The Limbo People* (1980), *A Paradoxical Community* (1990), *Managing Change in Old Age* (1992), *Old Age: Constructions and Deconstructions* (1994), *From First Principles* (1996), *Simulated Dreams* (2000).

Eyal Ben-Ari, is professor of anthropology at the Hebrew University of Jerusalem. Previous publications include *Body Projects in Japanese Childcare and Mastering Soldiers* (1997). Among recent edited books are (with Edna Lomsky-Feder,) *The Military and Militarism in Israeli Society* (2000) and (with Timothy Tsu and Jan van Bremen) *Perspectives on Social Memory in Japan* (2005).

Shifra Kisch is a doctoral candidate at the Amsterdam School for Social Science Research of the Universitet van Amsterdam and instructor at the University College Utrecht. She is currently completing her dissertation on gender and deafness among the Negev Bedouin. She has also studied the lay-encounters between Arab and Jewish women in an Israeli hospital.

Reuven Shapira is a senior lecturer of social anthropology and sociology in The Western Galilee Academic College, Israel. He experienced management in Kibbutz Gan Shmuel, received a Ph.D. from Tel Aviv University, and authored many books, booklets for managers, and articles in Hebrew and English on kibbutz and inter-kibbutz organizations.

Harvey E. Goldberg, is an Emeritus Professor in the Sarah Allen Shaine Chair in Sociology and Anthropology at the Hebrew University, Jerusalem, has authored *Jewish Life in Muslim Libya* (1990), and *Jewish Passages: Cycles of Jewish Life* (2003), and edited *Sephardi and Middle Eastern Jewries* (1996), and *The Life of Judaism* (2001).

Cédric Parizot (Ph.D.), is a researcher at the Institut d'Etudes et de Recherche du Monde Arabe et Musulman, Aix en Provence, Centre National de la Recherche Scientifique (CNRS). He is an anthropologist of politics. After studying electoral processes among Bedouins in the Negev (Israel), he now focuses on mobility and borders in the Israeli-Palestinian space.

Susan Rasmussen, is a Professor of Anthropology, University of Houston, has studied religion, healing, gender, aging, and verbal art in Tuareg communities of Niger and Mali. Her books include *Spirit Possession and Personhood among the Kel Ewey Tuareg* (1995), *The Poetics and Politics of Tuareg Aging* (1997), *Healing in Community*, and *Those Who Touch* (2006).

Raquel Romberg teaches anthropology at Temple University, and is the author of *Witchcraft and Welfare: Spiritual Capital and the Business of Magic in Modern Puerto Rico* (2003), *Healing Dramas: Divination and Magic in Puerto Rico* (2009), and several articles on Afro-Latin religions, Spiritism, and creolization.

Dina Siegel is Professor of Cultural Criminology at Utrecht University. She studied sociology and social anthropology at Tel-Aviv University, Israel and obtained her Ph.D. in cultural anthropology at the VU University of Amsterdam, the Netherlands. She

has studied and published several books and articles on post-Soviet immigration to Israel, organized crime, terrorism, drug- and women trafficking and criminality in the diamond sector.

Longina Jakubowska, currently an Anthropology Fellow at University College Utrecht in the Netherlands, received her M.A. in anthropology at Warsaw University and her Ph.D. at SUNY at Stony Brook. She has worked with a Bedouin community in the Negev Desert in Israel since 1981.

Nigel Rapport, M.A. (Cambridge), Ph.D. (Manchester), is a Professor of Anthropological and Philosophical Studies at the University of St. Andrews, where he directs the Centre for Cosmopolitan Studies. His most recent book is *Of Orderlies and Men: Hospital Porters Achieving Wellness at Work* (2008).

Dale F. Eickelman is the Ralph and Richard Lazarus Professor of Anthropology and Human Relations at Dartmouth College. His recent books include *The Middle East and Central Asia: An Anthropological Approach*, 4th ed. (2002) and *Muslim Politics*, new edition (2003), co-authored with James Piscatori.

Esther Hertzog is a Professor of Social Anthropology at Beit Berl Academic College. She studies bureaucracy, gender and welfare systems. Among her publications are *Immigrants and Bureaucrats* (1999), *Life, Death and Sacrifice: Women and Family in the Holocaust* (ed., 2008), *Perspectives on Israeli Anthropology* (co-editor, 2010) and *Patrons of Women* (2011). For the last twenty years she has been involved in feminist struggles and has published numerous op-ed columns.

Aref Abu-Rabi'a is a native anthropologist, at the Department of Middle East Studies at Ben-Gurion University. His main publications are on traditional medicine, family customs, holy saints, education, violence and abuse of children, cosmopolitanism, Islamic medicine and Sufism. His research focuses on the Middle East, North Africa and Islamic communities in the West.

Frank Stewart is a Professor Emeritus in the Department of Islamic and Middle Eastern Studies of the Hebrew University of Jerusalem. His publications include *Fundamentals of Age-Groups Systems* (1977), *Texts in Sinai Bedouin Law* (1988–1990) and *Honor* (1994).

Alex Weingrod is an Emeritus Professor of Social Anthropology at the Ben Gurion University of the Negev. His most recent book (with Andre Levy) is *Homelands and Diasporas: Holy Lands and Other Places* (2005). He is presently working on an anthropological analysis of present-day Israeli society.

Ofra Greenberg is a senior lecturer at Western Galilee Academic College. She studies medical Anthropology, Bio-Ethics, new towns in Israel, and total institutions. She authored *Women in Jail in Israel* (1982), *A Development Town Visited* (1989) and *Nes Ammim: Life Amid Contradiction* (1999).

Acknowledgements

Foreword[1]

The first anthropologist to win the Israel Prize, Emanuel Marx used the strengths of the Manchester School to introduce social anthropology to Israel. Much influenced by his mentor Emrys Peters, whose Bedouin essays he collected in a posthumous book (Peters 1990), Emanuel made his earliest major contribution, Bedouin of the Negev (1967), based on his doctoral thesis, by locating Bedouin and their relations with imposed military government squarely within Israeli society. I had the great pleasure of being a member of the Manchester graduate seminars in the early 1960s, when Emanuel first presented his thesis chapters. His scholarship was very impressive, I recall, and his interest in works on the Bedouin, limitless, perhaps fed by his father's own great passion, as an antiquarian bookseller who filled almost every room of his home with precious books. The depth of Emanuel's highly disciplined commitment to the study of the Bedouin is evident in his forthcoming book on Bedouin of Sinai, which he completed in his eighties and which addresses the Israeli occupation of Sinai with the same boldness as his youthful critique of Israeli military government in the Negev. In anticipation of the book itself, readers may turn to his recent article on hashish smuggling in South Sinai (2008).

Almost from the start, Emanuel pitted the strengths of his fieldwork evidence against a simplistic view of ecological adaptation. The everyday realities of social life among contemporary Bedouin were not to be reduced to a historic geographical determinism. Instead, the people's own ideas, their actor orientations, had to be weighed up, and heavily, though never exclusively, viewed over the longue durée. The problematizing of ideology is a lasting hallmark of Emanuel's intellectual project.

For example, Emanuel appreciated the widespread and long-term shift among most people in the Negev to a combination of farming and pastoralism. Then he went on to account for their differences in movement and settlement. These differences he showed to be directly dependent on the force of ideals, i.e. the people's understanding of how they thought they ought to live, given their sense of who they were and by contrast to others. He gave precedence to his observation that some people identified themselves as 'Bedouin' and espoused a pastoralist or 'nomad' way of life; that others, by contrast, made themselves out to be 'peasants' or farmers; that in accord with their orientation, and not as an ecological necessity, so did they move or settle. There were, he noted, 'limitations which their ideology sets to their freedom of choice' (1967: 98). Moreover, he stressed the importance of territoriality and the people's perception of the value of

---

[1] Some material from this foreword was originally published in Richard Werbner (2004: 340).

their distinctive attachments to land, 'The contrasting camping habits of Bedouin and peasants are neither due to ecological factors nor to a difference in their economies, but they have to be traced back to the unequal distribution of land ownership' (1967: 99). His first monograph's argument was deliberately holistic: he sought and found, in his own words, 'connections between the administrative order, land ownership, the composition of camps and movement cycles, political organisation and leadership, and patterns of marriage' (1967: xiv). But he also took great care to document and analyse long-term transformations from Ottoman times onwards. From that basis, he was able to raise questions about how far Bedouin ideology was more or less autonomous relative to change in the Negev economy.

Refugees, immigrants, and people on the move as nomads loom large in his nine books, all written with a deep sympathy from his boyhood experience, being a refugee from Hitler's Germany. Emanuel extended his interests widely from the maintenance of West Bank and Gaza Strip refugee camps to North African Jewish immigrants' encounters with bureaucrats, to advocacy in a Bedouin resettlement project.

Emanuel founded the department at Tel Aviv which grew so prominently from a Manchester–Tel Aviv research project. This he effectively ran, in the field, with Max Gluckman as its charismatic and peripatetic guru. The work of Shmuel Eisenstadt, Israeli sociology's founder, after the impact of that project, reveals how very considerable Emanuel's influence has been on the social sciences in Israel.

Coming to social anthropology after having been an adviser on Arab Affairs to the Israeli Prime Minister, he served his country in a number of capacities, all in the quest for peace in the Middle East. Late in life, he became Director of the Israeli Academic Centre in Cairo, where he fostered Egyptian–Israeli academic exchange, hugely increased the mutual translation of the countries' literatures, and became the first Israeli social scientist to reach a very wide Egyptian audience through Al-Ahram.

As a prolific editor of collections in English and in Hebrew, Emanuel has set a very high standard in that his introductions are always synthetic. Each brings together an acute distillation of the articles with an overview of the studies in the field, be it contemporary Israel and the anthropological study of nations (1980) or economic change among pastoral nomads in the Middle East (1984). Each also draws the reader in lucidly, and never with trendy jargon or fanciful tilting at windmills. Not glitz in ironic conceits and certainly not edification by puzzlement or some other alchemic flirtation instead of anthropology, but coherence of argument is the aim and accomplishment in Emanuel's own collections. We can only hope that readers will judge this book to be worthy in his honour.

Richard Werbner

Emeritus Professor in African Anthropology and

Honorary Research Professor in Visual Anthropology,

University of Manchester

References

Marx, Emanuel. 2008. Hashish Smuggling by Bedouin in South Sinai, in *Organised Crime: Culture, Markets and Policy*, edited by D. Siegel and H. Nelen. New York: Springer, 29–37.

——. 1980 (ed.) *A Composite Portrait of Israel.* London: Academic Press.

——. 1967. *Bedouin of the Negev.* Manchester: Manchester University Press.

Marx, Emanuel and Shmueli, Avshalom. 1984. *The Changing Bedouin.* Transaction Books: New Brunswick and London.

Peters, Emrys L. 1990. *The Bedouin of Cyrenaica*: *Studies in Personal and Corporate Power*, edited by J. Goody and E. Marx. Cambridge: Cambridge University Press.

Werbner, Richard. 2004. Marx Emanuel, in V. Amit (ed.) *Biographical Dictionary of Social and Cultural Anthropology.* Routledge: London and New York.

Introduction: Towards a Nomadic Turn in Anthropology

Haim Hazan, Esther Hertzog

Besides being a major focus for research in the anthropological tradition, nomadism is a state of mind central to the understanding of the ethnographic enterprise. That is not to say that other disciplinary courses of inquiry do not subscribe to the nomadic trope. We merely suggest that the anthropologist's nomadic predisposition, being the constitutive gaze of this unleashed discipline of travelling adventurers, poachers and missionaries, deserves special consideration as the means of manufacturing its ethnographic end-product. Aware of the experience and culture of actual nomadic people, the critical consciousness of the nomadic anthropologist 'resists settling into socially coded modes of thought and behavior' (Braidotti 1994: 5). Emanuel Marx, whose work has inspired this volume and to whose special mark on the discipline it is a tribute, indeed observed that the chief characteristic of nomads is their 'continual adaptation to a changing world' (2006: 92). This is precisely the ethnographer's experience: she encounters incessant changes in the field which require her to be physically mobile, mentally alert, emotionally resilient and socially agile; she must be prepared to modify and revise her theoretical standpoint time and again; and she must cope with the frequent unpredictable mutations in the articles of faith as to the desirable management of anthropological knowledge. In consequence, anthropology cannot be bound by prescribed formulae of writing culture, and each researcher in her turn is challenged to reinvent fieldwork practices, research methods and theoretical orientations. This built-in elasticity renders the discipline patently undisciplined and sets it apart from other behavioural regimes of knowledge whose languages of representing experience are kept at bay from the categories held by those they study. Thus, vacillating between a commitment to an acknowledged scientific inquiry phrased in an apt lingo on the one hand, and aspiring for its articulation in literary and personal terms on the other, the ethnographer is often at a loss as to the just and appropriate manner of positioning her ethnographic products within the scope of a particular discourse.

Thus, the nomadic force drives anthropologists from one idea to another, transcends boundaries, shifts involvements and transforms commitments until it is finally arrested and shaped in the published text. We believe that the flirtatious interplay between the anthropologist's wandering mind and the transient field could strike a seductive chord for accounting for that process of turning lives into works. This quintessential

property of the making of anthropological rendition breaks through set paradigms, undermines research programs and liquidizes solid matters of interest and concern but, nevertheless, clings to the paradoxical ethos of authentic representation. This tension between seeking fact and conjuring fiction exposes the anthropologist as an alchemist whose sorcerer's stone converts the mundane into the sublime and the prose of the field into the poetics of the script, while at the same time being constantly aware of the presence of both performances. In this respect the nomadism ascribed to the anthropologist does not conform to most of the images assigned to describe this mode of discovery. It is not the detached pedestrian exploration of the modern urbanite experience by the blasé Benjaminian flaneur (Buck-Morss 1989; Harvey 2003) whose voyeuristic horizons do not exceed the range of her stroll. Nor is it the authoritative spectre of the 'professional stranger' (Agar 1980), who comes today and stays tomorrow (Simmel 1971 [1908]), and neither does it cast a non-committal, distant tourist gaze (Urry 1990). Furthermore, anthropological nomadism is neither an aimless wandering as in the postmodern condition (Bauman 1992: 164–7), nor is it the 'real' nomads' planned journey towards a desired destination. For nomads 'do not "wander", but rather select migratory routes and new residential locations after carefully sifting information' (Salzman 2001). Nor is it an escape attempt of a free ethnographic spirit from the iron cage of preset, disciplined knowledge formations. Rather, it could be viewed as a way of evidence-based form of Peirce's abductive reasoning of creating and applying novel explanations to new observations (Quieoz and Floyd 2005). We choose to invoke the mythological concept of serendipity to mark the intuitive logic that transcends both subjectivity and objectivity, by which fluid anthropological sense is articulated and constantly reformulated.

It is the description and understanding of this serendipitous journey towards the elusive vista of knowledge that constitutes our call for revisiting current trends of anthropological thought and practice. The dominant pervasiveness of post-colonial agendas superimposes a-priori master-narratives of power/knowledge that turns the translation of field experience into writing it as culture (Clifford and Marcus 1986) a self-fulfilling prophecy, an autonomous self-sustaining and self-referent textual hyper-reality. Thus, under the guise of a multivocal reflective authorship, supposedly allowing for dialogical engagement between the researcher and the researched 'other', the onus of ethnographic accountability is sanctimoniously shared by all narrators of the field, albeit as scriptured in the literary fashion crafted by the anthropologist (Geertz 1988). This storytelling of the ethnographic yarn dovetails with its predestined narrative structure and, therefore, misses the critical junctures of wonderment, indeterminacy and uncertainty engendered by the omnipresent effervescence of field reality in the ethnographer's lived experience. Hence, the prevalence of that self-indulgent literary turn in cultural anthropology tends to position the anthropologist as the ringmaster of an anthropological realityshow in which she plays herself as a proverbial 'other'. Rather, it is the unexpected adventures and escapades in the 'desert of the real', to

borrow a term coined by Zizek (2002), that the book wishes to address and invoke as the driving spirit of the discipline.

This sense of the unadulterated 'real' looms before, and at times beyond, symbolization and representation lies at the core of any field encounter. However, its attempted reproduction in the form of a programmed ethnographic account invariably submits its transmutations and permutations brought on by respective changes in experience and consciousness to the aesthetic codes of set textualization. The analytic tension between the two is often resolved in anthropological discourse by resorting to preset consciousness under the guise of untainted experience (Rabinow 1977). That is, an adherence to the ethos and ethics of descriptive activity, sometimes known as 'auto-anthropology' lauding the significance of field experience for eliciting cogent interpretation, while at the same time practicing a personal free-floating, uncritical poetics of 'imaginative horizons' (Crapanzano 2004) or 'dreams and myths' (Augé 1999 [1988]). A counter-movement towards restoring faith in the much maligned, yet obsessively pursued, 'authentic' could offer an alternative way of redressing the balance between presentation and representation in favour of the former. By 'authenticity' we by no means suggest a revisit to any temporally 'frozen' cultural product of the variety of 'primitivism' (Turgovnick 1990) or 'mentalities' (Lloyd 1990). Instead, we issue a challenging call for a rebirth of anthropology as first and foremost a territory of action and interaction rather than a map of language games, as so many of its contemporary exponents would have it, dubbing it 'cultural critique' (Marcus and Fischer 1986). This flurry of agenda-laden cultural commentaries veils the process through which ethnography is created and unravelled, thereby failing to touch base with the rudimentary elements of doing fieldwork. These elementary forms of anthropological life ought to be recovered if the first principles of doing ethnography as generating ever-increasing circles of abductively propelled meanings are to be reinvented. This epistemological call for revitalizing anthropological self-recognition and, arguably, self-respect, is rendered timely in view of recent awareness of the weakening of the intellectual thrust of disciplinarian discourse due to the disregard for the core concerns of context and classifications (Marcus 2008) and in the wake of the growing aporia in anthropological establishment as to the scientific standing of the discipline. Thus, deductive, politically generated, public texts have taken over inductive inference, with the former posturing in the image of the latter as a contrived confluence of actors' texts, a confidence trickster masquerading as a contextually staged literary artist. Today's anthropology is afflicted by the advent of the literary tendency to mistake texts for contexts (Strathern 1987). This trend is amply manifested in the flourishing growth of branches of cultural studies whose claim to anthropological fame is in the name of culture as a social text. This usurpation of ethnography is facilitated by the tenor of the postmodern coupled with the dissipation of disciplinarian boundaries. Hence, ethnography re-inherits the original split between ethnos and graph. The retrieved construction of context as an emergent property of abduction could salvage anthropology from its current state of

erosion and 'suspension' born and bred in the fold of 'the narrative turn' (Marcus 2008: 1).

Abductive weaving of context is a double-edged artistry, for its relativistic properties enable both the quiddity of the milieu under study (Scharfstein 1989) and open-ended levels of comparison (Kuper 2002). The anthropological conversation between the two epistemic contingencies is conducted in a peripatetic fashion that could take a turn for the worse as futile meandering, or for the better as dialectical nomadism driven by discursive oscillation between symbols of localized knowledge and signs of common humanity, hence drawing on a contemporary image of a 'hetero-logoi' anthropos (Rabinow 2007). The logic of such movement is tracked down in the present volume where each and every chapter is marked by the footprints of processing contextualization according to the domains of experienced reality relevant to a particular field of research, and in accordance with the serendipitous dialogue between the hetero-logoi of the field and the accommodating logic of the anthropologist.

The seeming polyphony of voices echoed in the texts can be reduced to five key themes, to each of which a section is devoted. The five sections composing the volume illuminate the anthropologist's spatial and intellectual erratic journey of revelations within and without a designated human society. They follow the ethnographer's search for undiscovered and unexpected destinations till she comes to roost in an unintended one.

The first section – 'navigation' – traces the multi-directional trial and error quest of transforming a socially grounded reality into conceptualized cultural categories through making choices between serendipitous moments and mutations. It addresses the core problem of making sense of the ethnographic encounter, namely the modes and stratagems of translation adopted to transmute the exploration of a vibrant context into the print set of the text.

The central role of discovery in anthropological practice is discussed by Ugo Fabietti. His chapter develops the subject of discovery in ethnography through his personal experience in some areas of the Middle East, Indian sub-continent and 'beyond'. The course of the argument juggles between ethnography, anthropological theory, history and human sciences, and shows how the ethnographer, through her errancy between 'field' and 'theory', may bring a substantial contribution in understanding the specificity and uniqueness of anthropology.

Emanuel Marx, whose groundbreaking work on nomads, immigrants and refugees (ex. Marx 1967; 1971; 1976; 1984; 1992; 2001) has stirred this volume, elaborates on the process of conceptualization, which stands at the core of anthropological work. He argues that only an incessant dialectic between our selective observations and tentative interpretations can get us somewhat closer to understanding a reality that is almost impervious to conceptualization. Marx's analysis emerged from a series of studies of Bedouin spanning half a century, first in the Negev, and then in Sinai (Marx 1967; 1977; 1980a; 1984; 1987a; 2005; 2006; 2008). His work demonstrates the fruitful process of 'obstinate drilling in the hard rock of our mind, in order to overcome our stereo-

types and mental blocks, and continuously to revise our interpretation of ever larger quantities of amorphous data'.

The culturally malleable concept of Community is at the core of Haim Hazan's essay and it is brought to bear the cutting edge of conceptualizing reality to the extent of abandoning its power of representation. As 'a catch word', 'community' could imply delineated geographical boundaries and face-to-face interaction. It can also abandon these parameters completely. It can denote a lifestyle, or it can be used to convey a social ideology. Using a case of an urban renewal project in an Israeli poor neighbourhood Hazan offers a reading of three different narratives that demonstrates how 'community' is an image, whose seduction allows it to metamorphose and ultimately simulate and substitute basic reality.

The second section – 'mirage' – reflects the anthropological urge to reconcile between the desire to transfix field reality into a system of disciplined representations on the one hand, and the awareness of its unbridled nature through constant experiential transience and ephemera on the other. Encountering this dilemma constitutes the main epistemological paradox upon which any ethnographic inquiry is built, that is the simultaneously dual position of the researcher as a subject-cumobject. This double bind is responsible for painting the spectrum of anthropological discursive practices as stretching between evidence-based conventional science and mere storytelling. The constant interplay between defining fact and refining fiction, while transgressing and questioning the boundaries of the discipline, gives it the drive of its vitality and endurance.

Discussing fieldwork and the dynamics of participant-observation, Eyal BenAri argues that the attempt at self-critique is both a precondition for, and a limit on, the validity *of* 'findings'. Using his experience as an Israeli soldier during the first Palestinian Uprising (1987–92), and as a fieldworker in Japanese day-nurseries (in 1988), he traces out how facets of his identity as Jewish, male, scholar, soldier, and committed citizen figure in his research and writing.

Following on Marx's work (1987b) on the Bedouin society in the Negev, in which he defied gender dichotomy, Shifra Kisch reflects on her own experience of negotiating her gendered position among the Negev Bedouin. In the process of manoeuvring creatively within seemingly rigid power relations she occasionally found herself abruptly shifting positions and status to the subtle signs of her interlocutors she had to learn to read. Thus, she concludes, the privileged nomad often travels roads paved by others. 'Seeking guidance from those we encounter as we linger and from the signs left along the roads, we attempt to draw maps of newly assembled routes.'

The long and hard journey to anthropological discoveries is elaborated on by Reuven Shapira, who describes his personal *via dolorosa*, as a non-conformist Kibbutz member and researcher. Using Bourdieu's concepts he investigates how his changing habitus and capitals caused his departure from conventional careers, both managerial and academic, and made him persevere in overcoming stumbling blocks and achieving major discoveries. He argues that blind alleys in which one progresses for a considerable

distance while seeking new directions may encourage a study of unexplored parts of the field and efforts at radical change. Shapira emphasizes the advantage of home or native ethnographers in overcoming what Emanuel Marx considers as the (1985: 147) 'hardest part of [ethnographic] research', that is 'discerning the context of phenomena'. They can advance social science way beyond adding authenticity up to reforming a whole field of study.

The anthropologist's contention with the romantic image of Nomadic life is discussed by Harvey Goldberg. Adopting Emanuel Marx's ability to see the logic of an 'exotic' way of life, as reflected in his work on Bedouin in Israel, Goldberg challenges romantic notions of society. While often accused of succumbing to and perpetuating such images, whether embedded in popular or academic discourse, anthropologists more often wrote against them. This complexity is explored on the basis of Goldberg's experiences among Jews from Libya living in Israel. He shows the range and variety of reactions of members of a group to the work of anthropologists, indicating that today it is the very discipline of anthropology that the practitioner must carefully convey in writing.

Elaborating on moving fieldwork and ethnographic experiences in conflicting contexts, Cédric Parizot suggests that nomadizing between conflicting affects can provide a unique tool of inquiry in the anthropology of conflicts. He describes his constant passages from Arab to Jewish environments and between Israel and the Occupied Palestinian territories, during his research and life in the IsraeliPalestinian space. Drawing on his hardships and being inspired by Emanuel Marx's recommendation, to look at people's actions and choices as being highly emotional, he stresses the power of politics of emotions in drawing power relations between people as well as in shaping their representations of the conflict.

The third section – 'the journey' – grapples with the role of memory, mimicry and risk-taking in constructing the thrust of the ethnographic momentum. It demonstrates how the process of ethnographic decision making hinges on succumbing to field realities that might pose an intellectual as well as an experiential challenge to received opinions and given cosmologies. The suspension of disbelief required for confronting such ruptures in consciousness calls for a relinquishment of self-identity in favour of a mutually bounded space shared by both ethnographer and 'native'. The anthropologist's journey, therefore, is always encoded by a double helix of self and other whose inextricable interdependence blazes the trail towards the legible end-product.

The ways anthropological thought and action are forged in active dialogues with local residents is discussed by Susan Rasmussen. She focuses on incidents in which the anthropologist is drawn into the 'discovery procedures' of spirit possession. Elaborating on field research in semi-nomadic, stratified Tuareg communities in northern Niger and Mali, West Africa, Rasmussen explores how the local concept of spiritual and 'wild' space, called *essuf*, both guides and disorients the wandering soul of a person in spiritual and social space. It is locally viewed as a kind of spiritual wandering.

Aspiring to bridge between the spatio-temporal journeys and the marks on the page, Raquel Romberg uses her account of the embodied memory which connected past

magic acts with embodied mimetic memory. She describes her conversations with Tonio Lacén, the Witch of Loíza, in Puerto Rico, whose mimetic wanderings mesmerized her and stirred her imagination. She found herself drifting off to the interstitial spaces between magical power and its terrors, which have successively conflated the work of healers with heresy, charlatanism, and heritage.

The role of the anthropologist as a nomad in a dangerous field and the emergence of Cultural Criminology are at the focus of Dina Siegel's chapter. She views the anthropologist as an inter-disciplinary nomad in the sense that s/ he wanders among various intellectual areas, questions stereotypes that are taken for granted and breaks taboos. In her research on Russian organized crime in the Netherlands Siegel was inspired by Emanuel Marx's immense contribution to criminological research, in his studies on violent behaviour in the context of an immigrant community (Marx 1976) and in his description of drug smuggling patterns among the Bedouin of South Sinai (Marx 2008). Following Marx, Siegel illustrates the impact of ethnographic research on the recent study of organized crime in general and on a case study of the Russian Mafia in particular. She argues that the best criminological research is based on ethnographic research.

Longina Jakubowska's chapter is interested in the epistemology of the discipline. She elaborates on her academic and personal anthropological journey and compares the different stratagems she followed in two settings, i.e. the Bedouin in southern Israel and the gentry in post-communist Poland. The different positioning of the anthropologist with regard to informants, historicity, and anthropological canon are perceived as formative to the processes of ethnographic writing.

The fourth section – 'wandering' – extends the anthropological enterprise to overt and covert social claims and agendas looming beyond the boundaries of the field that nevertheless mobilize and shape the disciplinary discourse. This extradisciplinarian ambition for a voice and say in the public sphere sets anthropological routes within landscapes of moral grounds and horizons of social accountability and cultural judgment. Anthropological discourse thus grapples not only with charges of being an accomplice to colonial exploitation of the subaltern, but also with its own self-justification as re-presenting or representing the 'other'. Involvement in the field often breeds identification with the suffering and worldview of those studied and that, in turn, might engender pro-active indebtedness to their version of humanity as well as to what seems to be its implications to promoting the general good. Thus, paying penance to anthropological power that produces knowledge behoves ethnographers to *gaze* conscientiously upon the human condition at large.

Openness is perceived by Nigel Rapport as a social value and an analytical value alike. Adopting Marx's (1980b) warning against premature conceptualization that tends towards the bounded, closed and monolithic, Rapport calls to convey social life as a series of partially overlapping open systems. The city is conceived as a site where an embodiment of 'global guesthood' might be secured and enshrined; movement and multiplicity as ideal ways to measure just procedures of state; justice as ensuring the

free movement that is fundamental to human being and becoming, to its potential for multiplicity.

In his chapter Dale Eickelman emphasizes social scientists' responsibility to engage in public issues, in basic research and in producing outcomes that are not tailored to anticipate what sponsors or policy makers want to hear. Thus he praises Emanuel Marx and the Bernstein Project as having made better sociology, for not distancing themselves from policy objectives and the territorial state as their object of study. Maintaining the balance among these competing factors is in particular a responsibility of those engaged in the study of the Middle East.

Some of the ethical dilemmas that the anthropologists face when studying recollections are discussed in Esther Hertzog's chapter. Using documented interviews with her mother Hertzog tries to combine between the anthropologist's 'instinctive' suspicions and her professional and ethical training, while exploring Holocaust survivors' stories. While ostensibly documenting her mother's story she became a companion to her mother's emotional, verbal and intellectual journey between places, periods and ethical positions. Inspired by Emanuel Marx's unique and subtle perceptiveness, which makes him so skilful in pinpointing hidden meanings of human expressions, Hertzog realized that as puzzling, contradicting and even absurd the informant's stories and interpretations may seem, they all are part of an intriguing conduct of a human being.

The fifth section – 'oases' – follows the quest for setting findings and analysis within a broader context of social change and cultural codes. This is the nomad's outpost from which the route hitherto taken could be observed, appraised and recalculated against the backdrop of the overall landscape of other possible tracks and landmarks. This is when the flaneur stops roaming the streets and enters a cafe, when the tourist checks in a hotel room, when the stranger sojourns for the night and when the nomadic anthropologist turns around to look for a publisher or to revisit already published work. This is a moment of temporary respite; a time for reflective reading of field notes as they are rewritten and reshaped as ethnographic scripts. The production of a predestined monograph is thus an encapsulated panorama of the anthropologist's musing and observing that converge into a critically exposed authorized publishable work. This textual resting-place is by no means final, since escaping, revisiting and reshaping are common practices in the ever-increasing circles of the serendipitous enterprise. Thus, any oasis ultimately reverts into yet another attempt at navigation, a different mirage, a new journey and a rediscovered feat in wandering and wondering. Like in the shifting kaleidoscope, the captured composite of anthropological shapes and forms changes as each beholder rotates the tube. The chapters of this section visit the oasis of camping anthropologists as they re-contextualise their fields and calibrate their gyroscopes on a never ending voyage of the hunting of an anthropological 'snark' (Carroll 2008 [1898]).

Abu Rabi'a's chapter demonstrates the importance of contextualization. Drawing on his study on Bedouin tribes in the Negev Desert, especially those that continue to raise livestock, Abu Rabi'a deciphers the range of meanings of colours and their use in medical therapy in pastoral Bedouin society. He suggests that the symbolic significance

of colours for the pastoral Bedouin reflects his environmental and social circumstances, economic situation, religious and folk beliefs, and the exigencies of situations.

In his chapter on the structure of Bedouin Society in the Negev Frank Stewart offers a re-analysis of Emanuel Marx's (1967) groundbreaking work on the subject. He suggests that the richness and accuracy of the information provided by this work make it part of the short list of indispensable anthropological studies on the Middle East. The chapter penetrates beyond superficialities, presents rich factual material, and insists on theorizing it. However, Stewart also offers critical perspectives with regard to terminological and conceptual difficulties of Marx's work.

Alex Weingrod's chapter examines how bones and burials are woven into the fabrics of Israeli society and culture. Different social contexts are briefly examined – among others, the burials and reburials of historic Zionist and Israeli leaders; repatriating and reburying the bones of Moroccan saints and elders; Palestinian funerals during the intifada. These events, both historical and contemporary, are perceived as forming a prism through which the changing features of society can be interpreted.

Ofra Greenberg's chapter is interested in reconsidering the boundaries of the research field, the anthropologist's position within it, as well as in previous interpretations. She revisits her own pioneering study of a women's prison in Israel, in 1972–73. The events that have accumulated over the years have thrown new light on two central figures and provided fresh insights into their behaviour during and after their imprisonment.

The trope of the interchange between being imprisoned and being set free is an apt accord to conclude these introductory remarks. The totality of commitment to ethnographic work and life is broken down by the free spirit of the anthropologist whose serendipitous moments could trigger a surge of creativity and inspiration so as to transcend the canonized boundaries of the discipline and by the power of abduction to move into other realms of consciousness altogether. Serendipity therefore is an engine for moving anthropology forward, but at the same time it holds *in* store the promise and the prospect of derailing elsewhere towards other destinations. It is our conviction that each and every one of the book's chapters could, with a little twist of language, ethos and purpose, transform into text of different genres. The anthropological containment of the chapters in spite of that vulnerability to metamorphic potential proves the strength of their disciplinary conviction.

References

Agar, Michael. 1980. *The Professional Stranger*. New York: Academic Press.

Augé, Marc. 1999 (1988). *The War of Dreams: Studies in Ethno Fiction*. London: Pluto Press.

Bauman, Zygmunt. 1992. *Mortality, Immortality and Other Life Strategies*. Cambridge: Polity Press.

Braidotti, Rosi. 1994. *Nomadic Subjects.* New York: Columbia University Press.

Buck-Morss, Susan. 1989. *The Dialectics of Seeing: Walter-Benjamin and the Arcades Project.* Cambridge MA: MIT Press.

Carroll, Lewis. 2008 (1898). *The Hunting of the Snark, an Agony in Eight Fit*s. London, McMillan

Clifford, James and Marcus, George (eds). 1986. *Writing Culture: The Poetics and Politics of Ethnography.* Berkeley: University of California Press.

Crapanzano, Vincent. 2004. *Imaginative Horizons: An Essay in LiteraryPhilosophical Anthropology.* Chicago: University of Chicago Press.

Geertz, Clifford. 1988. *Works and Lives.* Stanford CA: Stanford University Press.

Harvey, David. 2003. *Paris, Capital of Modernity.* London: Routledge.

Kuper, Adam. 2002. Comparison and Contextualization: Reflections on South Africa, in *Anthropology by Comparison*, edited by A. Gingrich and R.G. Fox. London: Routledge, 143–66.

Lloyd, Geoffrey Ernest Richard. 1990. *Demystifying Mentalities.* Cambridge: Cambridge University Press.

Marcus, George and Fischer, Michael. 1986. *Anthropology as Cultural Critique: An Experimental Moment in the Human Sciences.* Chicago: University of Chicago Press.

Marcus, George. 2008. The End(s) of Ethnography: Social/Cultural Anthropology's Signature Form of Producing Knowledge in Transition. *Cultural Anthropology*, 23: 1–14.

Marx, Emanuel. 1967. *Bedouin of the Negev.* Manchester: Manchester University Press; New York: Praeger.

——. 1976. *The Social Context of Violent Behaviour: A Social Anthropological Study in an Israeli Immigrant Town.* London: Routledge and Kegan Paul.

——. 1977. Communal and Individual Pilgrimage: The Region of Saints' Tombs in South Sinai', in *Regional Cults* (ASA Monograph 16), edited by R.P. Werbner. London: Academic Press, 29–51.

——. 1980a. Wage Labor and Tribal Economy of the Bedouin in South Sinai, in *When Nomads Settle: Processes of Sedentarization as Adaptation and Response*, edited by P.C. Salzman. New York: Bergin, 111–23

——. 1980b. On the Anthropological Study of Nations, in *A Composite Portrait of Israel*, edited by E. Marx. London: Academic Press, 15–28.

——. 1984. Changing Employment Patterns of Bedouin in South Sinai, in *The Changing Bedouin*, edited by E. Marx and A. Shmueli. New Brunswick, New Jersey: Transaction, 173–86.

——. 1985. Hamehkar ha'antropologi-hevrati ve'hakarat ha'hevra ha'aravit' (The Social Anthropological Study of Arab Society), in *Lehakir 'amim qerovim* (To Become Acquainted with Neighboring Nations), by J. Bashi et al. Jerusalem: Van Leer Jerusalem Foundation, 137–52.

——. 1987a. Labour Migrants with a Secure Base: Bedouin of South Sinai, in *Migrants, Workers and the Social Order* (ASA Monograph 26), edited by J.S.

Eades. London: Tavistock, 148–64.

———. 1987b. Relations between Spouses among the Negev Bedouin. *Ethnos*, 52(I–II): 156–79.

———. 1992. Palestinian Refugee Camps in the West Bank and the Gaza Strip. *Middle Eastern Studies*, 28(2): 281–94.

———. 2001. (Guest Editor). Employment and Unemployment among Bedouin. *Nomadic Peoples*, 4(2).

———. 2005. The Bedouin's Lifeline: Roving Traders in South Sinai, in *Social Critique and Commitment: Essays in Honor of Henry Rosenfeld*, edited by M. Al-Haj et al. Lanham, MD: University Press of America, 193–206.

———. 2006. Tribal Pilgrimages to Saints' Tombs in South Sinai, in *Archaeology, Anthropology and Cult: The Sanctuary at Gilat, Israel*, edited by T.E. Levy. London: Equinox, 54–74.

———. 2008. Hashish Smuggling by Bedouin in South Sinai, in *Organized Crime: Culture, Markets and Policies*, edited by D. Siegel and H. Nelen. New York: Springer, 29–40.

Marx, Emanuel and Ben-Porath, Yoram. 1971. *Some Sociological and Economic Aspects of Refugee Camps on the West Bank*. Santa Monica, CA: Rand.

Marx, Emanuel and Shmueli, Avshalom (eds). 1984. *The Changing Bedouin*. New Brunswick, New Jersey: Transaction.

Quieoz, Joao and Floyd, Merrell. 2005. Abduction between Subjectivity and Objectivity. *Semiotica*, 153: 1–7.

Rabinow, Paul. 1977. *Reflections on Fieldwork in Morocco*. Berkeley: University of California Press.

———. 2007. *Marking Time: On the Anthropology of the Contemporary*. Princeton, Princeton University Press.

Salzman, Carl Philip. 2001. *Understanding Culture: An Introduction to Anthropological Theory*. Prospect Heights IL: Waveland.

Scharfstein, Ben-Ami. 1989. *The Dilemma of Context*. New York: New York University Press.

Simmel, Georg. 1971. The Stranger, in *Georg Simmel on Individuality and Social Forms*, edited by D.N. Levine. Chicago: University of Chicago Press, 143–9 (originally published in 1908).

Strathern, Marilyn. 1987. Out of Context: The Persuasive Fictions of Anthropology. *Current Anthropology*, 28: 251–81.

Turgovnick, Mariana. 1990. *Gone Primitive: Savage Intellects, Modern Lives*. Chicago: University of Chicago Press.

Urry, John. 1990. *The Tourist Gaze*. London: Sage.

Zizek, Slavoj. 2002. *Welcome to the Desert of the Real: Five Essays on September Eleven and Related Dates*. London: Verso.

Part I: Navigation

Chapter 1: Errancy in Ethnography and Theory: On the Meaning and Role of 'Discovery' in Anthropological Research

Ugo Fabietti

Prologue

When I was invited to contribute to a book on the 'nomadism of the anthropologist', I immediately shared the intentions of its editors. Quite apart from having conducted my own early research among the nomads, I also have a nomadic story linked to my professional background. First, to my university training. Having come from philosophical studies, I didn't attend any courses in Italy on anthropology. I was therefore relatively late in turning to the discipline, and was something of an anomaly compared to standards prevalent in the 1970s in many European countries (and today including Italy). Second, to my theoretical leanings. By training, I am not firmly committed to any one approach over another. I waver, sometimes perhaps unconsciously, between different paradigms: not in homage to a form of eclecticism, but rather as a consequence of a realization that not everything can be treated in the same way. If I have convictions (or idiosyncrasies) about theory, they have never been of any 'school'. And lastly, my nomadism is linked to my ethnographic experiences. I have transited, if I may so put it, with a certain conviction through three principal 'terrains': the Arabian Peninsula, the Iranian coast of the Persian Gulf, and south-west Pakistan. In Arabia I was with the Bedouin nomads of the Great Nefud desert; in Iran with the fishermen of the Hormuzgan region; in Pakistan with the sedentary farmers of southern Baluchistan. Between the beginning of the first and the end of the third, just under twenty years passed, during which my perspective (and above all my 'anthropological awareness') was enriched and changed direction. It was perhaps also sharpened by the diversity of contexts and objects on which I came to reflect.

I believe that certain aspects of my 'nomadism' show some of the limitations, as well as some of the strong points, of anthropology. The lack of absolute paradigmatic references, the transitivity from one disciplinary context to another, and the frequenta-

tion of different ethnographic fields can, in effect, impose limits for those who maintain that scientific work is the pursuit of a research programme geared to a hyper-specialist paradigm. But for others, and especially for anthropologists, empirical and theoretic mobility can prove to be a force and to constitute the truly vital elements of the discipline. This is because, by erring (personally or not) from one cultural context to another, and having incessantly to confront otherness (which is, in a sense, its bread), anthropology seems to be in a condition best to reflect, in its underlying inspiration, practice and epistemology, the reality of a 'world in movement' like that of today. This mobility in approaching its objects makes it possible to 'see how', and to 'put into perspective', as no other humanistic discipline can. The anthropological style of reasoning has by now 'erred' into bordering fields of knowledge, and is unlikely to be rejected by these in the future. Whatever may happen, the style of reasoning adopted by anthropology will remain a specific and necessary one for describing the 'human condition'.[1]

Stating the Problem

I confess that these, and other more strictly epistemological matters, were not exactly the main focus of my attention when, in July 1978, I made my debut as a field researcher. Nor is it immediately possible to 'stand aloof' from one's ethnographic research (or even to take such a possibility for granted). This possibility is, so to speak, 'remodulated' subsequently, on the basis of further experiences of field (and other) research, trial writings, and always the play of memory which of course opens up boundless and often unsuspected spaces for self-reflection, but also entails restrictions and imposes censorship.

If distance in time from the 'terrain' enriches 'field memory', that same distance also produces an estrangement from the initial results of our research, exposing them to a sensation of thematic and methodological obsolescence. However that same distance has the advantage of enabling us to rethink certain aspects and moments of the work, which at times appear suddenly, and unexpectedly, crucial.

On rethinking a number of things about my work in the field, both the earliest and the most recent, I realized that, however much in our writings we may strive to recount the reflexiveness, positionings and power relationships in which we have been involved; the misunderstandings, the stratagems and the snares into which we have fallen or more or less luckily managed to avoid, we hardly ever recount the way we

---

[1] As George E. Marcus wrote 'what happened in 1980s is that many other disciplines, especially humanistic ones, like history and literary studies, in an effort to renew themselves with a social relevance became passionately interested in many of anthropology's established framing concepts and postures. In literature, history and art, interest in the nature of cultural difference and the expressions and exercise of power through cultural innovation became the fashion in a far less parochial manner' (Marcus 2005: 678).

came into possession of certain apparently insignificant information which nevertheless helped us to make progress in our fieldwork. Or, if we do, we tend to present the matter in an incidental manner, without problematizing and discussing the exact role of such events in the process of our understanding. This non-problematization does not help to contrast the idea of anthropology as being a rather loose discipline largely founded on chance. And it ties up with another, fairly widespread idea in many academic circles, whereby our discipline would be the product of a 'nonscientific' practice, and therefore worthy of scant attention inasmuch as it is deemed incapable, unlike others, of evincing generalizations, quantitative data and predictive models.[2] I believe there is not much to be done in the face of prejudices of this kind, which are based on a reductive idea of knowledge. I think we ought instead to explore – I might say in a Kantian way – the avenues along which anthropology proceeds, and to highlight the points in common between sciences and knowledge which, though remaining separate by virtue of the different nature of their object and 'style of discourse', are often founded on similar reasonings.

One of the elements that joins all types of research and learning together is what may generically be defined as 'discovery'. As regards anthropology, the explanation of how we came into possession of certain information that enabled us to move forward in our research is methodologically crucial. It is connected with what I call 'ethnographic discovery', a topic that has in my opinion long been neglected in anthropological literature.

Before going into the details of the question through an example drawn from my own research, a few empirical as well as theoretical explanations are required.

On the empirical level, I must point out that by discovery I do not certainly mean having for the first time witnessed a rite, gathered a terminological system of kinship previously unknown, or been present at an encounter with a population of which no news had ever previously been received.[3] Still less do I mean, by discovery, the innumerable things that come to be known in ethnographic routine, in the daily 'fatigues', as Lévi-Strauss observed, which reduce the ethnographer's profession to 'une imitation du service militaire' (Lévi-Strauss 1984: 9). It is true that the anthropologist in the field keeps on 'discovering' things. But few of these will allow him (if ever) to reverse a perspective, to problematize in an absolutely new way a theme, or to hit on a factual confirmation of a characteristic aspect of a certain group or society.

By 'discovery' in ethnography I mean the identification of something that will allow the person identifying it to alter their perspective on a given theme or problem and, naturally, enable them go forward in the knowledge of their object.

---

[2] This opinion has undoubtedly been reinforced since the 'interpretive turn' established itself, which nevertheless established a timely break from the earlier 'objectivist' approaches, after these had proved to be widely untenable.

[3] 'Discoveries' of this type certainly occurred in the past (at least from the point of view of western observers), but from the period of Jean de Léry and his Tupinambas, to that of James Clifford and his postmodern museums, the space for discoveries of this kind dwindled to virtually none at all.

The first social scientist to concern himself with what I call 'discovery' here was probably Robert K. Merton. In 1948, he published an article in which he asserted that 'under certain conditions, a research finding gives rise to social theory' (Merton 1948: 506). The three key points of Merton's reasoning were: 1) 'The fairly common experience of observing an unanticipated, anomalous and strategic datum which becomes the occasion for developing a new theory or for extending an existing theory'; 2) 'The observation must be anomalous, surprising, either because it seems inconsistent with prevailing theory or with other established facts'; 3) 'The unexpected fact must be "strategic", i.e. it must permit [...] referring to what the discovery brings to the datum than to that datum itself' (Merton 1948: 506–7).

Abduction, Evidence and Serendipity

Although Merton makes no mention of it, his issue is linked to the notion of *abduction* which, according to Charles S. Peirce (1958: 7.218), indicates the logical process (considered by him to be 'the first step of scientific reasoning') by which, by inference, we obtain information about certain things that have a high probability of being true. An example of reasoning by abduction might be the following: I bring home a basket of red apples. After a while I happen to see a red apple on the kitchen floor. I have excellent reasons to maintain that the apple comes from the basket, even though this might not be 'true'.

Abduction is the logical process underlying what the historian Carlo Ginzburg, in an extensive and celebrated recognition of the processes of enquiry typical of the human sciences, from the history of art to psychology, from medicine to criminology (anthropology however was missing from this roll-call), defined as an 'evidential' process (*indiziario*, in the original). This was discussed by Ginzburg especially in relation to the problem of attributing works of art to a specific artist, to physiognomics and to the search for the causes of diseases in medical diagnostics. What is signified by reasoning according to clues is summed up by Ginzburg himself in the procedure adopted by a hunter:

> Man has been a hunter for thousands of years. In the course of countless chases he learned to reconstruct the shapes and movements of his invisible prey from tracks on the ground, broken branches, excrement, tufts of hair, entangled feathers, stagnating odours. He learned to sniff out, record, interpret, and classify such infinitesimal traces as trails of spittle. He learned how to execute complex mental operations with lightning speed, in the depth of a forest or in a prairie with its hidden dangers (Ginzburg 1990: 102).

According to Ginzburg the impact of this paradigm lies precisely in its being 'not very rigorous', at least inasmuch as it cannot be quantified – a price which the human

sciences seem obliged to pay if they are to achieve results of any importance and, most of all, of any perspicuity. The evidential approach to our knowledge of the world is found in everyday human behaviour, in the sphere of practical and technical expertise. It is closely connected with all types of knowledge or skills founded on *coup d'oeil*, instinct and intuition. Although it also belongs also to the physical and natural sciences, this approach is in particular characteristic of the human sciences – at least in the measure in which the latter almost always lack the possibility of elaborating an experimental method.

Merton, on the other hand, described the process qualified as *evidential* by Ginzburg, and *abductive* by Peirce, with the adjective *serendipitous*. Merton in fact takes up an English neologism dating from the mid-eighteenth century (serendipity) by means of which in time a series of events, behaviours and situations had come (almost always improperly) to be qualified (Merton and Barber 2004).

Serendipity is a word coined in 1754 by Horace Walpole, after he had come to hear of a 'silly fairy tale' (as he himself described it) published for the first time in Italian, in Venice, a couple of centuries earlier and in which is told the story of *The*

Three Princes of Serendip.[4]

Walpole wrote to a friend,

> As their Highnesses travelled, they were always making discoveries by accident and sagacity, of things which they were not in quest of: for instance, one of them discovered that a mule blind of the right eye had travelled the same road lately, because the grass was eaten only on the left side, where it was worse than on the right – now do you understand *Serendipity*? (Merton and Barber 2004: 108).

The term serendipity was used for two centuries with various shades of meaning, including that of 'accidental discovery'. But this is not the signification Walpole wanted to give to the word he had coined. He in fact says that the three princes continually discovered, *by accident and sagacity*, things they had not been seeking. By accident and sagacity, hence by chance and on the strength of an intuitive reasoning ascribable to what Ginzburg calls precisely the *evidential paradigm* founded on abduction. One must insist on the conjunction 'and' because at times it has been maintained, as I was saying, that *serendipity* might stand exclusively for a chance discovery, such as that of a treasure underneath the floor of an old house, or that certain particular circumstances can enable us to act randomly in one way rather than in another. The examples of this latter way of understanding the notion of serendipity (and thus of ignoring its evidential-abductive component) can be summed up in the definition of

---

[4] The name of ancient Ceylon, now Sri Lanka.

the term given in the *Concise Oxford Dictionary*: 'the faculty of making happy and unexpected discoveries by accident'.

The difficulty of shaking off this generic (and not perspicuous) idea of serendipity and of tracing it back to an intuitive process based on inference, remains also among those who recognize this human mindset as having an important role in research. In an article based on his fieldwork in China, Frank Pieke, for example, recognizes that serendipity is central in

> discarding the notion that reality is law-governed and knowledge is finite ... [and that] serendipity in this enterprise is less random and more proactive than suggested by the standard gloss of the term' [the definition of the *Oxford Dictionary*] ... [as it] describes the creative tension between structuration and event, and that balance between control and creativity which defines science as a vocation within a discursive community (Pieke 2000: 129–30).

Nevertheless in his detailed and interesting description of the research done by him in Beijing and in Raoyang, Pieke seems precisely to neglect the essential characteristic of serendipity: that of denoting random unintentional discoveries based on (abductive) intuition, and to confine himself instead to underling the 'fortuitous' character of certain encounters or situations that turned out to be important to the pursuit of his research.

On the other hand, according to Walpole's idea, which at the empirical level anticipated the formal definition of the abductive process given by Peirce, are excluded from the qualification of serendipitous all 'discoveries' by chance, whereas in that definition may be included discoveries made 'by sagacity'. These, albeit starting from a datum stumbled on by chance, make it possible to attain to more general conclusions about someone or something. To this specification must be added another, namely that Walpole's definition also includes the idea that what is discovered is not being sought at all: in the 'silly fairy tale' the sons of King Serendip continually discovered 'things they had not been in quest of'.

I now propose to illustrate, through a concrete case deduced from my personal experience in the field, this manner of proceeding which, if not exclusive to anthropological reasoning, does however play an essential role in ethnographic practice.

Prepare yourselves therefore to read 'a silly fairy tale'. But not without first being informed of my personal 'nomadic' context into which it fits.

A Silly Fairy Tale (Part 1)

After reading philosophy I had turned my attention to Americanistics, also publishing a work on violence in Amazon societies based solely on ethnographic literature.

However my closeness at that time to some French Africanists induced me to consider Africa, rather than South America, as a possible field of research.

At the beginning I had in mind an extremely vague project on the nomads of the Horn of Africa. But, thanks to a contact procured for me by my then thesis tutor Marc Augé at the École des Hautes Études en Sciences Sociales, I was enrolled (and here serendipity did not come into it at all!) by a development research company and was aggregated to a fairly numerous and highly heterogeneous team of *experts*, with the task of conducting a two-year survey on aspects of 'social change' in Saudi Arabia. After an exhausting wait of various weeks, I left France (and an Italy troubled by obscure terrorist plots), and arrived in Riyadh on 15 July 1978.

In that period 'my anthropology' was inspired by a 'critical', para-Althusserian Marxism, complete with the 'dynamist' approach of Georges Balandier and with the 'Manchester School' perspective, both of which took on the dimensions of conflict and change as landmarks central to research. I had in fact for a number of years been mixing with some French anthropologists who were particularly sensitive to the use of categories and analytical tools originating from nonorthodox Marxism.

When I reached Arabia I felt reasonably well seasoned theoretically, but I was ill-trained in the techniques of field research. My background was philosophical and the lectures in anthropology attended at the EHESS in Paris during the two previous years were not, in this respect, of much help.

If I consider my beginnings I must admit that I treated my research from a prevalently theoretic, and probably too abstract, angle. I did however activate the tools at my disposal: my studies of domestic groups in East African agropastoral societies; travel literature on the Middle East, and particularly Arabia; the segmentary lineage theory, a spot of history, geography and linguistics. Modern works, and notably monographs, on the Bedouin of the Middle East and especially of Arabia, were rare. After the masterpieces by Musil on the Rwala (1928) and the book by Dickson on the Bedouin of Kuwait (1949), in practice nothing more at all had appeared until the classic study by Emanuel Marx on the Negev Bedouin (1967), followed by the concise, but very good, one by Donad P. Cole on the Ahl Murra of Rub' al-Khali (1975); by a very interesting sociological study by Paul Bonnenfant on the urbanization of the nomads in Saudi Arabia (1977); and by a number of works on the Bedouin of Syria and Arabia by Fidelity and William Lancaster (whose extremely interesting book on the Rwala came out in 1981, when unfortunately my field research had already ended).

My almost total initial ignorance of the language forced me to rely on local or Maghreb interpreters, and only after several months was I able to follow some of the relevant aspects of conversations hinging on hitherto circumscribed topics. My initial linguistic handicap prompted me to focus my attention on the economic and social side of life in the Bedouin communities. In any case these subjects came under the fields of enquiry contemplated by the project for which I was working (transformations of productive processes, social change, urbanization). Also, they could call for the application of the theoretic tools at my disposal to objects of interest to me: power

relationships and forms of control of the nomadic communities by the state; unequal distribution of resources; formation of new elites, etc.

Arabia was not an 'easy' field for me. True, you could find everything there (from cassette players to under-the-counter alcoholic drinks, from Australian fillet to Cuban cigars and Bulgarian cheese). But the foreigners in that country lived out on a somewhat 'distorted' social limb. The authorities did nothing (on the contrary!) to attenuate this state of chronic separation from local society, and police control (though 'discreet', as I was able to try out on a couple of 'critical' occasions) was far-reaching. In some centres, where *wahhabi* orthodoxy is more deeply rooted, than elsewhere in Saudi Arabia, the 'mistrust' of non-Muslims and foreigners in general could be cut with a knife.

I arrived in Arabia during the reign of Khaled ibn Sa'ud (1975–82), just when the first serious contradictions were beginning to show between the 'Islamic' face of the country, pervicaciously proclaimed by its political and religious hierarchies on the one hand, and the reality of an urban society increasingly overwhelmed by modernization and consumerism on the other. The 1970s were a period of sudden accelerations, some of which upset important balances within the Muslim world itself. If the seminal event of this period was the Iranian Revolution of 1979, in Arabia the shock-moment was the occupation, precisely at the end of that same year, of the *haram* in Mecca by a few hundred rebels led by Juhayman ibn Muhammad al-'Utaybi and by Muhammad ibn 'Abdullah al-Qhatani, this lastnamed having been proclaimed *mahdi* ('he who leads') by his supporters. They had been hoping for a popular uprising leading to the removal from power of the Sa'ud, deemed politically corrupt and betrayers of the faith. Many of these rebels were students of the Islamic University of Medina and disciples of Egyptian *'ulamas* affiliated with the *Ikhwan* (Muslim Brotherhood) who had been expelled by Nasser and welcomed to Arabia by Faysal, Khaled's predecessor, in the 1960s. The episode, quelled with bloodshed after much hesitation by the Saudi authorities (in the *haram* at Mecca it has been forbidden to carry arms and to spill blood since pre-Islamic times), was only a widely acknowledged signal of the deep unrest felt throughout Saudi society in those years and a prelude to today's conflicts.[5]

In short, the atmosphere in Arabia was tense and, unlike what I later had occasion to experience in Baluchistan (where things were also extremely explosive, but entirely different), I was myself viewed with suspicion by all. On the whole the Bedouin looked kindly upon me. But owing to their not always easy relationship with government bodies, I was continually at risk of being viewed by them as a possible channel of information for the authorities. Meanwhile the authorities appeared not to take much notice of me, though actually they were very well informed about what I was doing. They were worried, as I later heard, that my work might touch on politically 'sensitive' topics (which was fairly obvious when talking of the distribution of resources). Even the

---

[5] For a general picture of the recent political developments and of the internal opposition to the Sa'ud see Al-Rasheed 2002.

officials from the company by whom I was employed had their suspicions, sometimes of the same kind as the Saudi officials.

At the time of my own research (I don't know what exactly happened afterwards) there were a number of 'tribes' (*qabayel*) of high 'reputation' (*sharaf*) who seemed not yet to have 'settled their scores' with the Sa'ud. To one of these tribes belonged the Bedouin among whom I spent most of my research time. They were a sub-fraction of the Shammar, the main supporters of the Ahl Rashid of Hail, and had been sworn enemies of the Sa'ud in the struggle for the control of the Arabian peninsula between the mid-nineteenth century and the 1920s. Having favoured the study of this group caused me quite a few difficulties in organizing my enquiries into the relations between the Shammar and the Saudi authorities, and all the more so because some 'sections' (*ashair*, *fukhud*) of this important tribe had for decades settled in Iraqi territory while keeping in touch with those still in Saudi Arabia. About these relations I was simply asked by the Bedouin themselves not to ask questions.

What had driven me to focus my work on this group is hard to say exactly. Certainly the historical background contributed. My approach tended to privilege socio-political change and transformation, and to play down more proficient approaches to the symbolic aspects of cultures, including the structuralist one then still in vogue in Paris. Chance, too, however played its part: the courteous (and unexpected) collaboration of a functionary from the local emirate. And perhaps also a certain aesthetic influence that fitted my predilection for the historical dimension. When I visited Hail for the first time, many of the constructions, though abandoned and in ruins, dated from the days of the Rashid. Some of these crumbling buildings (which the bulldozers were clearing away) still revealed their interiors decorated with red and yellow geometrical motifs, as in the watercolours of Anne Blunt. Seen from the top of the *kasr*, Hail was surrounded on all sides by dense tresses of palms, whose intense green contrasted subtly with the pinkish sands of the Great Nefud desert. On rereading, once back in Europe, the description of these places left by Doughty in that masterpiece, *Travels in Arabia Deserta* (1888), I 'retroactively' reinforced these impressions, traces of which will certainly be found in my early work (Fabietti 1984).[6]

Much of my research was devoted to outlining the processes of structural change induced in Bedouin communities by the introduction, in the reproductive cycle, of new kinds of resources: wage-paid work, government subsidies, and above all land, granted by the state first for rent, and later as property. Perhaps the most significant result of the research carried out by me in Arabia consists in the outlining of some aspects of a newly emerging social scenario.[7]

---

[6] Once again what T.H. Lawrence wrote in his introduction to the 1921 edition of Doughty's book was confirmed: 'The more you learn of Arabia the more you find in Arabia Deserta'. The subject of the 'construction' of one's ethnography on the basis of ex-post suggestions is certainly an epistemologically relevant one.

[7] I realize today that the work I devoted myself to, although I did also arouse some appreciation, was not backed by an outstanding amount of 'empirical data'. On the subject of 'data', it is true, it

A Silly Fairy Tale (Part 2)

Since these were Bedouin it seemed obligatory to deal with questions regarding the segmentary-lineage theory and hence with their genealogies. At the time of my research on this subject in Arabia, things had, in some ways, come to a dead end.

Obviously it was known (and long had been) that genealogies did not 'tell the truth'. It was also known that they were liable to processes of compression, fusion, fission, selection, forgetfulness and invention. It was, in short, known that they were the fruit of a manipulation both unconscious and intentional. There was consequently no point whatever in trying to trace the history of the Bedouin tribes on the basis of genealogies. The great book by Max von Oppenheim, *Die Beduinen* (1939–68) was for me only a terrifying list of names.

Genealogies were known to change continually. More stable at levels farthest from our time, they grew increasingly fluid as they drew closer to the present. Bedouin genealogies in fact explain not so much the past as the present. They justify the state of political relations among groups. Briefly, the more politically united they are, the closer their respective ancestors become; the fewer relations they have, the farther away from each other are their respective ancestors (and not vice versa!).[8]

Precisely during my stay in Arabia I happened to read an article by Robert Montagne dated 1932. Montagne, who in the 1920s was director of the Institut Français de Damas, had been able to make contact with a number of Bedouin of the frontier which today separates Syria from Saudi Arabia, and among these were some Shammar. To my great surprise, Montagne had included in that article the genealogical structure precisely of the sub-fraction on which I had focused my own work. Between the 'data' in my possession and those supplied by Montagne there were however some notable discrepancies. In Montagne's work the group in question (ar-Rmal) presented itself as comprising the following nine sub-fractions: Hazim, Gared, Khanshar, Mneikher, Kelab, Seloug, Racham, Omur and Dweilé. On the basis of the data that I had personally gathered, this group was formed by as many sub-fractions. But their names were almost

---

is not possible to decide 'how many' these must be, and often one hears it said that it is mainly the plausibility of the picture we produce that constitutes the guarantee of the validity of our ethnography. Nevertheless it is certainly true that my own work remained fairly abstract, possibly due to my philosophical background which fostered in me an excessively speculative attitude (at least so it seems to me today).

[8] Although the nomads have known writing since time immemorial (obviously I am not talking about their literacy rate!) their genealogies have always been strictly oral. The oral character of genealogical memory in fact allows that flexibility in the 'use' of genealogies which is extremely functional to the practical life and to political relations among the groups. It has to be said that as a consequence of the steady imposition of central administration upon the lives of the nomadic populations, genealogies can become stable in that they are transcribed to political purposes, that is to say as a means of affirmation of, and demand for status or particular rights from the central authorities. This is a theme dealt with in more recent years by Shryock (1997) with reference to the Bedouin in Jordan, a subject which at the time of my own research I knew nothing about except for a couple of feeble clues which however I did not take into consideration.

all different: Msellam, Khasciaràn, Khanshar, Khatlàn, Hadlàn, Muhammed, Racham, Amra and Ali. Only two names recurred in both cases: Khanshar and Racham. In just over fifty years (the time lapse between Montagne's survey and my own), no less than seven of the nine sub-fractions making up the ar-Rmal group had changed!

Who was telling the truth? Of course, it is possible that Montagne may have received second-hand information, or that I myself had been misinformed ... . Having checked the exactness of my information, and having no evidence to maintain that Montagne's information was bogus, I resigned myself.

I felt there was nothing else to do but try to explain, with convincing formulas and words comprehensible to an (Italian) anthropological public at that time unfamiliar with the subject, what had been the 'genealogical dynamic' of the Bedouin tribes of Arabia. To render better the idea of what this 'genealogical dynamic' was, I felt (apropos of 'errancy' between areas of knowledge) that a term taken from the vocabulary of the sciences of the Earth might suit my case: *actualism.* In geology, actualism (or *uniformism*) is the theory whereby the agents currently active in changing the Earth's landscape are the same as those that altered it in the past. Translated into genealogical terms, actualism to me meant something very simple: knowing that genealogies justify only the present and not the past, the processes (and causes) on the basis of which the Bedouin groups tighten or slacken their alliances must in the present be the same as those which determined the arrangement of genealogies in the past and from which future transformations stem. Per se, the idea was certainly not new. But it seemed to me that the notion of actualism, which I kept turning over in my mind during the months of my stay in Arabia, might serve to represent better than any other the image of Bedouin genealogies as fluid, changing entities, whose transformation was however guided by always identical factors.[9] Rather than change as a result of someone's decision or due to the occurrence of sudden events, genealogies had to change, or so I thought, as a consequence of slow, uniform processes, imperceptible not only to the eye of the ethnographer (which might be excused) but also to that of the Bedouin themselves. What 'intrigued' me most, in the case in point, was that although I had two 'snapshots' of the genealogy of a group, it was not possible to capture the passage from one snapshot to another.

But it was not these themes, I repeat, on which I had focused my research. However, one day something unexpected, and anomalous, happened. On my way with a group of Bedouin from the sub-fraction with which I had my main contacts, to an oasis on the edge of the southern Nefud, we came across the home of a Bedouin belonging to the same sub-fraction, who was introduced to me as a profound connoisseur of Shammar genealogies. When the conversation got going I couldn't help asking him to list the names of the segments (*fukhud*) that were part of ar-Rmal. This man confirmed

---

[9] I had found the concept of actualism, or uniformism, in Charles Lyell's *Principles of Geology* (first published in 1831) when I had devoted myself, as a philosophy student and as a dabbler in anthropology, to the birth of this discipline and to its relations with the sciences of the Earth and with archaeology in the Victorian age.

what the other Bedouin had told me until then, namely that ar-Rmal was made up of the nine groups named earlier: Msellam, Khascharàn, Khanshar, Khatlàn, Hadlàn, Muhammed, Racham, Amra and Ali. When the conversation turned to other topics he suddenly added: 'Msellam, Khatlàn, Hadlàn, Racham and Amra, together with Seluj and Farha, are *Umkhalaf* (allies)'.

This last statement seemed to leave some of my fellow-travellers flabbergasted and, though puzzled, they were unable to contest it in any way.

When I tried to understand better what the genealogist pundit meant by introducing a distinction between ar-Rmal and Umkhalaf, the Bedouin himself answered that the sub-groups called Umkhalaf were so denominated because 'they were very closely connected with one another'; that they 'were very close'.

I was not sharp enough to understand whether in that critical situation I had been the witness to a feat of mnemonic skill by the expert genealogist, or whether a minor 'political drama' had just been enacted. Who was the genealogist speaking to? To the anthropologist or, as it occurred to me later, to the members of his own lineage group? Was that Bedouin telling the others that 'manoeuvres' were in progress, or was he exploiting the situation for purposes known to himself only? One last possibility: had I been escorted on purpose to him so that I might 'spread' the news of a change, capable of reaching the ears of some important personage? At that time a number of sub-fractions were receiving lands from the government, whilst others were on the waiting list. These questions will clearly never be answered. Nevertheless something had happened with that genealogical 'manifestation'.

If we compare my snapshot of the ar-Rmal genealogy with that of the expert genealogist, it is clear that missing from the Umkhalaf group were the sub-groups Khanshar, Khasciaràn, Muhammed and Ali (which are however part of ar-Rmal), whereas within the Umkhalaf appear the names of two new sub-groups: Farha and Seluj. Whilst Farha does not feature in the genealogy collected by Montagne, Seluj 'returns' (Seloug, in Montagne's transcription 'à la française').

How was it then that Seluj (Seloug) had first been present (Montagne) and subsequently vanished (Fabietti) and now reappeared among the Umkhalaf groups?

Now there is something that really might be called a 'serendipitous situation'! A new, unexpected datum, *something I had not been in quest of*, was, once it had been correlated with other data, not only confirmation of a dynamic which no one had ever been able to observe in its 'factual' reality: but it also helped to reconfigure the picture of supposed political relations present in that moment on the ground.

The situation can in fact be interpreted as follows.

The sub-groups called Umkhalaf had intensified their relations by interacting with one another to a greater extent than they themselves had been doing at that moment with the Khanshar, Khasciaràn, Muhammed and Ali groups.[10] Very probably

---

[10] By interactions I mean the complex of relations which two or more groups can maintain at various levels: economic, logistic, matrimonial, and 'political' in the strict and in the broad sense of the term.

the Khanshar group, from which the *shuyukh* of ar-Rmal have by now long originated, had set up, with the advent of land distribution, strategies aimed at reinforcing their own already politically pre-eminent position.[11] For that purpose it had favoured the alliances with a few groups (Khasciaràn, Muhammed and Ali) to the detriment of others whose only answer was to intensify their relations to the point of defining themselves, by contrast, as Umkhalaf.

The fact that in the Umkhalaf group the names of two sub-groups appear which were not included in the present formal ar-Rmal genealogy, can be traced to a process of the same identical nature, and is an essential part of the evidential procedure sparked by the serendipitous encounter with the Bedouin genealogist. Farha and Seluj might in fact have been two groups with which the other Umkhalaf had just recently started to interact in a continuous manner. The fact that one of these two groups, the Seluj, had in the past been part of the formal genealogy of the ar-Rmal (Montagne), whilst at the end of the 1970s it was excluded from it, might mean that for a certain period it had left the field of interaction with the other segments, but was currently 'getting closer'. The idea that the principle of actualism was a metaphor also valid in anthropology could have been considered by no means odd.

Had the phenomena that I have described evolved in line with the dynamics of that time, the genealogical relations between the sub-groups included in the formal ar-Rmal genealogy and those that were part of Umkhalaf could have led to what is known as genealogical segmentation. However the outcome of such a process never appears concluded once and for all. Indeed the phenomena of coalescence and fission were such as to involve, at later points in time, other possible groups which in the period of my research did not feature within either the ar-Rmal or the Umkhalaf. Some of the sub-groups which in the late 1970s appeared in ar-Rmal and/or Umkhalaf could, with time, have ended up interacting with groups different to those of that time, and thus have disappeared from the genealogy of both, in the same way in which other groups might have appeared both within ar-Rmal and within Umkhalaf (as for example in the case of Farha).

This reasoning cannot therefore but give rise to a partial predictive model, in the sense that it affects only the outline of a dynamic form. The reason for this 'partiality' of the model lies precisely in the fact that whilst it may be maintained that the dynamic of genealogical changes is due to identical processes, it is *not possible to know what relations the groups will develop in future and with whom.*

I hope that the silly fairy tale told by me may have served to illustrate with some degree of truthfulness the procedure that may happen to be adopted in field research, and the type of generalizations (among the various) which it is possible to produce in anthropology. Other approaches in ethnography certainly exist. But I believe that in

---

[11] A process, that of the distribution of land to the Bedouin by the Saudi Crown (to which the land belongs by decree), begun in 1968 and which however only subsequently concerned the Shammar of the Hail province.

our (human) 'sciences' the evidential, or if we care to call it serendipitous, or abductive method is one that has too often been sacrificed, in the written and oral tradition of 'how to do research', first to models inspired by an 'objectivist' tendency, and later to models that have perhaps granted too much to the dialogical and reflexive dimension of fieldwork. Can the awareness of such a method be an anchor, if I may so put it, for our anthropological nomadism?

References

Al-Rasheed, Madawi. 1991. *Politics in an Arabian Oasis: The Rashidi Tribal Dynasty.* London: I.B. Tauris.

Asad, Talal. 1986. *The Concept of Cultural Translation in British Social Anthropology, in Writing Culture, The Poetics and Politics of Ethnography*, edited by J. Clifford and G.E. Marcus. Berkeley: University of California Press, 141–64.

Bonnenfant, Paul. 1977. L'évolution de la vie bédouine en Arabie centrale. *Revue de l'Occident musulman et de la Méditerranée*, 21(1): 111–77.

Cole Donald. P. 1975. *Nomads of the Nomads, the Ahl Murra Bedouin of the Empty Quarter.* Chicago: Aldine.

Dickson, H.R.P. 1949. *The Arabs of the Desert.* London: Allen and Unwin.

Doughty, Charles M. 1979 (1888). *Travels in Arabia Deserta.* New York: Dover.

Fabietti, Ugo. 1984. *Il popolo del deserto. Gli Shammar del Gran Nefud (Arabia Saudita).* Bari: Laterza.

Ginzburg, Carlo. 1990 (1979). Clues: Roots of an Evidential Paradigm, in *Myths, Emblems, Clues* (translated by J. and A.C. Tedeschi). London: Hutchinson Radius, 96–125.

Lancaster, William. 1981. *The Rwala Bedouins Today.* Cambridge: Cambridge University Press.

Latour, Bruno. 1993. *We Have Never Been Modern.* Boston MA: Harvard University Press.

Lévi-Strauss, Claude. 1984 (1955). *Tristes tropiques.* Paris: Plon.

Lyell, Charles. 1997 (1830–33). *Principles of Geology.* London: Penguin.

Marcus, George E. 2005. The Passion of Anthropology in the U.S., Circa 2004, *Anthropological Quarterly*, 78(3): 673–95.

Marx, Emanuel. 1967. *Bedouins of the Negev.* Manchester: Manchester University Press.

Meillassoux, Claude. 1975. *Femmes, greniers et capitaux.* Paris: Maspero.

Merton, Robert K. 1948. The Bearing of the Empirical Research upon the Development of Social Theory, *American Sociological Review*, 13(5): 505–15.

Merton, Robert K. and Barber, Elinor G. 2004. *The Travels and Adventures of Serendipity: a Study in Sociological Semantics and the Sociology of Science.* Princeton and Oxford: Princeton University Press.

Montagne, Robert. 1932. Notes sur la vie sociale et politique des Chammar du Nejd, *Revue des Etudes Islamiques*, 6: 61–79.

Musil, Alois. 1928. *The Manners and Customs of the Rwala Bedouin.* New York: American Geographical Society.

Oppenheim, Max F. von 1939–68. *Die Beduinen.* Leipzig: Otto Harrassovitz.

Peirce, Charles S. 1958. *The Collected Papers of Charles Sanders Peirce*, (vol. VII), edited by A.W. Burks. Cambridge, MA: Harvard University Press.

Pieke, Frank. 2000. Serendipity: Reflections on Fieldwork in China, in *Anthropologists in a Wider World*, edited by P. Dresch et al. New York and London: Berghahn Books, 129–50.

Sahlins, Marshall. 1972. *Stone Age Economics.* Chicago, IL: Aldine.

Shryock, Andrew. 1997. *Nationalism and the Genealogical Imagination, Oral History and Textual Authority in Tribal Jordan.* Berkeley: University of California Press.

Chapter 2: The Growth of a Conception: Nomads and Cities

Emanuel Marx

My understanding of the Bedouin of Mount Sinai and, by inference, of pastoral nomads in general, grew by painfully slow stages. It has taken me the best part of thirty years to evolve a conception of these Bedouin that comprehends the fullest range of data I observed and wrote down in the field. Perhaps I should apologize for being so slow-witted, but I naively believe that I am no different from other anthropologists involved in the extended process of conceptualization. As our conceptions determine which aspects of reality we see and which we ignore, conceptualization stands at the core of anthropological work. It requires us to engage in obstinate drilling in the hard rock of our mind, in order to overcome our stereotypes and mental blocks, and continuously to revise our interpretation of ever-larger quantities of amorphous data. Without this extended work and toil we would get stuck in the fragmented commonplace knowledge created jointly by the people we study and by us, their students. Only an incessant dialectic between our selective observations and tentative interpretations can get us somewhat closer to understanding a reality that is almost impervious to conceptualization.

The worst thing an anthropologist can do is to rely on her or his stored up research experience. For then he is bound to go wrong. He will easily recognize familiar landmarks, but fail to see the many intriguing new sights and insights concealed in his data. In order to make sense of his or her observations in a new field, the anthropologist must make a radical fresh start. First, she must develop an initial conception of the social aggregate studied, being well aware that it is simplified, even stereotyped, but will inevitably change and grow as work progresses; second, she must focus on a sociological question that touches the lives of the people she lives with; and third, she must devise a methodology that precisely fits the problem and the people studied. Only such a three-pronged approach gives the anthropologist a chance to achieve his or her ultimate aim: to get the ethnography right, in the sense that a large number of 'facts' become meaningful, hang together and explain one another, thus enabling him or her to understand at least a limited aspect of an elusive reality. As we all know, this is easier said than done.

In my earlier work on Bedouin cultivators and pastoralists in the Negev (Marx 1967) I had argued that, in as far as they specialize in pastoralism, Bedouin are full

participants in a complex city-based market economy. While cereal farming provides them with some economic autarky, their labour-intensive pastoral activities produce expensive commodities, such as meat, milk, wool and hides. They consume only small quantities of these products, as they can sell them for a good price in cities. With the proceeds they buy their staple foods, such as cereals, oil and sugar, and the numerous goods they require in daily life. On the face of it, the nomads can transact their business during one or two annual trips to the city. They may even dispense with personal visits and, instead, trade with itinerant merchants. Yet they depend on the city for sheer existence, and can remain pastoralists only as long as the city-dwellers are wealthy enough to buy their products.

When Bedouin become labour migrants in cities and in industrial enterprises they become even more involved in urban society. Ironically, while they are only sojourners in the city and work in the lowliest occupations, they are more exposed to the city's economic and political vicissitudes than most other city-dwellers. For in a recession, they are the first to lose their jobs. Then they may return home to their flocks and fields and sell the produce on the market, thus re-entering the city economy by another door.

Thus, when I began fieldwork in Sinai I already knew that Bedouin depend on cities. It took me some time to realize that these particular Bedouin not just need the city, but fully participate in urban civilization. This insight helped me in formulating a first conceptualization of their society. It took me much longer to work out a more advanced second conception. That happened when I finally grasped the significance of a reality I had seen for many years: that these Bedouin had developed an urban civilization in their midst. Only then did I realize that this urbanism was evident in every aspect of their lives, as in their complex division of labour, their total dependence on foods and other commodities produced in cities and villages, their heavy reliance on income from migrant labour and drug smuggling, and their submission to the indirect rule of the state.

The dependence of the Mount Sinai Bedouin on the city was even greater than that of the Negev Bedouin. They never achieved any economic autarky, and could not survive in their mountain reaches without a steady supply of imported grain and other basic commodities. Even their small flocks and orchards could sustain them only for a few weeks. They were 'pastoral nomads' only in a very limited sense, and had developed a quite complex multi-faceted monetary economy, whose further growth was not so much limited by scarce capital, as by a lack of schooling and technological training. Therefore, I argued, they were a specialized segment of urban civilization.

These understandings led me to concentrate more and more on the complex economy of the Sinai Bedouin. I soon discovered numerous groups, which engaged in a wide range of social activities. While most of these groups were corporate, the descent groups, which are so prominent in ethnographic studies of pastoral societies, were in this case neither corporate nor very active. This induced me to look more closely at the workings of the corporate groups, which often included clusters of kinsmen, and

48

to pay less attention to descent groups and kinship networks. I was aware that I was parting ways with colleagues who considered kinship and descent as basic elements of 'social structure', as I had still done in Bedouin of the Negev (Marx 1967).

The First Conception

Between 1972 and 1982 I spent altogether about twelve months in South Sinai, mostly among the Jabaliya Bedouin in the Mount Sinai region. It did not take me long to realize that they were quite different from the Negev Bedouin whom I had studied earlier. In the Negev my theoretical starting point had been that the Israeli military administration had a critical impact on Bedouin society. Its officers believed in the notion, so common among administrators of pastoralist populations, that the Bedouin produced animals and grains mainly for their own subsistence and contributed little to the national economy. Therefore they did not hesitate to remove most of the Bedouin from their land and to confine them to a native reservation. They did not allow the Bedouin to enter the labour market, which was to be reserved for the masses of destitute new Jewish immigrants. Therefore they issued only few short-term labour permits to the Bedouin, and permitted them to visit the market town of Beersheva just once a week. Thus they reduced people who had previously participated in a complex economy to nearly becoming pastoralists and dry-land farmers.

In Sinai such a conception did not work for either the Egyptian authorities or the Israeli occupying forces which replaced them and put much effort into administrating the Bedouin population. While the Jabaliya did raise flocks and tended orchards, in contrast to the Negev Bedouin they did not derive an income from them. Theirs was a token pastoralism which, they were at pains to tell me, only caused them losses, and a horticulture that only produced fruit and vegetables for home consumption. They treated the fruit especially as 'sweets', and not as a staple food. They earned their cash income mostly as labour migrants, as they had done, so the elders insisted, for many generations. This claim is fully corroborated by ancient documents stored in Santa Katarina monastery and by travellers' accounts (Marx 1987). They maintained the flocks and orchards as resources to fall back on in bad times, a kind of economic reserve or insurance policy. Experience had taught them that in the recurrent economic and political crises that befell the region, the labour migrants were always the first to lose their jobs. Whenever there was a wave of unemployment, they would return home and reactivate their flocks and orchards, thus rejoining the market economy by an alternate route. During my fieldwork there were two occasions when the reserve economy came alive; one was the October 1973 war between Egypt and Israel, and the other, ironically, US Secretary of State Kissinger's shuttle diplomacy between Cairo and Jerusalem in 1976 which was to lead to a peace treaty between the two states and, incidentally, to end the Israeli occupation of Sinai. On both occasions the labour

migrants returned home and worked hard to increase their flocks and raise the yields of their orchards and vegetable patches.

Most Bedouin considered labour migration to be a fundamentally unattractive type of work, as it forced men to spend long periods away from family and home and also because it generally offered insecure and low-paid employment. While almost every Bedouin man spent many years of his working life as migrant labourer, he never intended to remain one for long. He eagerly awaited the time when he could stop working, namely when his sons would be old enough to take over. Then he would return home to South Sinai, to tend his gardens and raise a small flock, and be subsidized by the income of his sons. In practice, these longterm projects hardly ever worked out as planned. For when work opportunities were abundant the older men too would again go out to work as labour migrants, and when work was scarce the flocks and orchards could not rapidly enough be converted into profitable enterprises.

For a long time I thought that labour migration was the key to understanding this society. It certainly was, and had been for generations, the Bedouin's major source of cash income. As labour migration typically offers only insecure employment, the Bedouin invested great efforts and resources in building up a system of mutual assurance at home. This was an economy in reserve, in the widest sense of the word. It consisted of the gardens and flocks which were maintained in a state of readiness for rapid development. Further, it included carefully fostered ties with kin and friends, who would aid them in time of need. Lastly, it included the maintenance of tribal membership through which they acquired important territorial privileges, in particular the right to construct gardens and homes in their tribal territory, as well as free passage through, and access to pastures in, South Sinai, the region of the Tawara tribal confederation.

While the men were away at work, the women and children operated the reserve economy. The women kept the gardens and flocks just ticking over, so that when the men returned home they would put greater efforts in them and turn them into profitable ventures. Women also took care of relationships with kin and friends, the people who would support them in time of need. The men too would periodically take time off from work, in order to visit their kin and friends and bring them small gifts. Towards the date harvest in autumn many men would return home, in order to accompany their families on the annual tribal pilgrimage to the tombs of the tribe's ancestor and other saints. That was the only time when the members of the tribe, who had all year been scattered among many distant workplaces, were reunited. Together they re-embodied the tribe and reaffirmed its territorial rights, while each individual consolidated his or her relations with the saints, the intercessors with a provident God.

The Second Conception

This was the conception I used in my first articles on the Mount Sinai Bedouin (Marx 1977; 1980; 1984; 1987), and it seemed to explain most of the data. But soon I was writing several papers that did not fit the conception. The change was precipitated by a comparative study of pastoralists in the Arab Middle East that I had begun at the time (Marx 1992; 1995; 2006a). A major finding of that study was that pastoralists are so deeply involved in the market economy that they should be seen as a specialized sector of a complex city-based economy. In the city they market their produce, such as meat, hides and wool, and buy all the necessities of life, including most of the grains that are their staple food. They depend so heavily on cities, that any economic or political turbulence there may change their entire way of life, to the extent that they may temporarily or permanently cease to be pastoralists. This new viewpoint allowed me to account for the two dramatic political events that occurred during my fieldwork: the 1973 war between Egypt and Israel, and the negotiations between the two countries conducted by Henry Kissinger, then the American Secretary of State, in 1976. Both events had caused political upheavals, which sent home the Bedouin labour migrants. Such traumatic events, I realized, had occurred quite regularly in the course of the peninsula's history. For Sinai has for millennia served as land bridge between Africa and Asia, and was the scene of many battles between invaders from the North and Egyptian defenders, neither of whom cared much for the native population (Eckenstein 1921; Mouton 2000: 27–36).

The desire to understand how Bedouin coped with these recurring crises made me examine the available historical material. The Santa Katarina monastery in the Sinai mountains preserves a unique collection of ancient documents, many of which concern the Bedouin. The documents cover, very unevenly, the last 1,000 years. Most of the documents have been copied on microfilm (Stewart 1991: 98), but only some of the Arabic and Turkish documents have been deciphered and translated (Humbsch 1976; Schwarz 1970). An interesting history of Sinai (Shuqair 1916) used these and other documents to good purpose, but left room for further study. We also possess a long series of reports by travellers and pilgrims to South Sinai, going back to the fourth and fifth century (Hobbs 1995, Chapter 8; Deluz 2001). The earliest surviving accounts are those of Egeria (or Etheria), a Frenchwoman who visited Mount Sinai in 385 (Éthérie 1957; see Eckenstein 1921: 114) and the hermit Nilus who lived in Sinai around 410 (Henninger 1955). Some of the learned and observant travellers of the eighteenth and nineteenth century, such as Niebuhr (1774), Burckhardt (1822), Robinson (1867), Wilson and H.S. Palmer (1869–71) and E.H. Palmer (1871), have left valuable accounts of the region and its people. I could not afford to neglect such rich material, which could add a historical dimension to the study.

The new conception led me to re-examine my earlier work. I realized that the Mount Sinai Bedouin were even more profoundly involved in world events than I had thought, and my writings had not portrayed all the complexity and adaptability of

their society. In particular, their economy was more diversified than I had allowed for. The re-examination of the ethnographic material resulted in several articles. The first one dealt with oases (Marx 1999). It showed that there were various kinds of oases, all of them man-made. As some oases had been established as hideouts for bands of smugglers, the paper touched marginally on the importance of smuggling of narcotic drugs to the Bedouin economy before the Israeli occupation. Smuggling had driven economic development in South Sinai and smugglers had even created several new oases in the mountains. While smuggling was at low ebb during my fieldwork, it remained an important though submerged aspect of Bedouin society which demanded a fuller treatment (Marx 2008).

Another set of articles (Marx 2004a; 2005b) dealt with the critical contribution of roving traders to the economy of the South Sinai Bedouin. The traders supply the Bedouin with grain and almost all the other necessities of life. While grain is the Bedouin's staple food, it is not grown in the region, and had it not been for these traders the Bedouin simply would not have survived in arid Sinai. Under Egyptian rule, this trade had been in the hands of South Sinai Bedouin, at first cameleers and later truckers, who worked closely with Egyptian shopkeepers in Suez. Only during the Israeli occupation was the trade taken over by the urban merchants of al-Arish in North Sinai, and after Sinai returned to Egyptian sovereignty in 1982 – Bedouin truckers became once more involved in it.

All these articles prepared me for writing this essay, the most explicit theoretical statement about the Bedouin of Mount Sinai (for an earlier version see Marx 2005a). Once I saw the complex occupational specialization of the Bedouin, the secure base of Bedouin society, the tribe and kinship links, the flocks and orchards, and the tribal pilgrimage, could no longer be treated as an economic stand-by, a response to the exigencies of labour migration. They were just another way of life, one of many possible ones. Philip Salzman had tried to convert me to this viewpoint (Salzman 1980: 7–10), but at the time I did not heed his good advice.

It took me a long time to realize that the Bedouin not only participated in the great society and its market economy, but were an integral part of it. This was especially evident in a diversified occupational structure. While practically every household raised flocks and tended orchards, most men were labour migrants, and many others were involved in smuggling, quite a few men and women also engaged in a specialized trade. They were well-diggers, builders, caravaneers, truck drivers and vegetable farmers, including several who produced the local variety of tobacco and thus infringed a state monopoly. They were pollinators of date palms, fishermen, boat builders, shopkeepers (shops were called *kantin*) and owners of coffeehouses. Some were government employees, such as tribal chiefs, teachers and government informers. Others were tribal judges, healers, jewellers, tailors, basket makers and tourist guides. There were also up to thirty permanent employees of the Santa Katarina monastery, whom the monks called *subyan al-deir*, lads of the monastery. They included an electrician, driver, carpenter, cook and a number of menial workers. Nearly all the above mentioned Bedouin specialists

were men, but there were also women specialists, among them healers, fortune-tellers and midwives. The services of all these men and women were remunerated in cash.

Specialization was even reflected in such matters as the services of local healers and in the powers attributed to various saints. Each healer treated a specific set of complaints: one was an expert on diabetes, another specialized in circumcisions and extractions of the uvula, operations to which every boy and girl was subjected, and another again – in mental problems. Saints too were thought to specialize in dealing with certain problems. Bedouin often approached one saint when their children fell ill, another was visited by barren women, a third saint could bring back a lost camel, and yet another – assure the welfare of the flocks.

This does not imply that the economies of the city and of the Bedouin were almost identical. The low population density, the relative poverty and illiteracy of most of the inhabitants, the peculiar combination of labour migration, pastoralism and horticulture, all these produced a special brand of complex economy. It required the inhabitants to cover great distances to perform the simplest economic transaction; it induced them to use locally available materials, and to exercise great ingenuity in operating modern technologies. Thus a visit to a healer, often combined with a personal pilgrimage to a saint's tomb, might take them several days of travel by foot or by car. The basket-maker al-Rifi of Wadi Firan used only locally produced materials, the fronds and fibres of palm trees. Even where industrially produced cords were available, he preferred to twist his own ropes. His products were quite different from those marketed in the cities and villages of Egypt: he made containers for dates, baskets for coffee-making utensils or for clothing (to be hung from the rafters of the house or the roof of the tent), chickencoops and a kind of incubator for newly hatched chicks. While Egyptian villagers too used the material from local palm trees for the manufacture of numerous household goods, the only item made in both regions was cords made of the fibres (Henein 1988: 179–94). Similarly, local car-owners who had no access to spareparts, carried out ingenious improvised repairs with odd pieces of wood and ends of barbed wire left behind by the armies passing through Sinai.

There were other persons whose presence made Bedouin society possible. The roving merchants from al-Arish were obviously part of the society. So were the tourists, who brought cash to the Bedouin who stayed at home, the government officials who provided essential services, the soldiers and security men who interfered in the Bedouin's way of life and drove up the price of narcotics, and the Greek monks who had inspired the Bedouin's horticulture during centuries of co-existence and, after having themselves claimed to own all the land in the vicinity of the monastery, now supported the Bedouin's claims to the ownership of orchards and building sites.

Therefore it is essential to treat Bedouin society as a complex and economically differentiated social agglomerate, whose members participate in various ways in an urban civilization. A study of the Bedouin must take into account the impact of rapid and often turbulent changes in the wider political arena on the Bedouin economy. It must show how various economic pursuits produce numerous sectors and lifestyles within

this Bedouin society, existing side-by-side and yet linked up with one another, each sector adapting in its particular manner to changing political and economic circumstances. The study is therefore quite unlike the standard anthropological monograph. It works with an open system that emphasizes wider social contexts. The ethnography thus becomes exceedingly complex, wideranging and disjointed, and therefore lacks the dramatic unity of persons, time and plot that makes a good read. The tribal pilgrimages (Marx 1977; 2006b), however, reflect and fuse many of these disparate strands of society.

The Bedouin of South Sinai illustrate one solution to the profound connection between nomads and cities: their society is an attenuated version of the city's complex economic specialization. That does not prevent the Bedouin from making numerous excursions to the city in the course of the year, in order to benefit from the full range of its services. The Syrian Rwala Bedouin (Lancaster 1997: 166–7) exemplify a very different type of dependence on cities: their major specializations in sheep-raising, commerce and trucking require them to constantly move between the cities and markets of four countries (Saudi Arabia, Iraq, Jordan and Syria). But they too have for generations had numerous specialized artisans and traders living in their midst. Therefore I tend to argue that all social aggregates in which pastoral nomadism is an important part of the economy share the city's diversified economy.

The basic understanding that urban civilization pervades every aspect of Bedouin society made me question some of my own theoretical positions. I shall briefly discuss just two issues. First, the corporate groups of the Bedouin, because they illustrate a major theoretical reorientation. And second, the great geographic mobility among the Bedouin because, paradoxically, it helps us understand why Bedouin can, for the time being, remain in the Mount Sinai region. For the prevailing myth of demographic stability of the Bedouin population may make us oblivious to the threatened extinction of the Bedouin as pastoralists and horticulturalists.

From Corporate Descent Groups to an Abundance of Corporate Groups

My ideas about corporate groups changed twice. First, I found that corporate descent groups played an important role in Negev Bedouin society. On examination I realized that these groups had nothing to do with kinship, although many members were also kinsmen. While the descent groups were corporate and dealt with clearly defined issues, kinship relations were dyadic and people could call on kinsmen for any kind of assistance. And second, in Sinai I found that Bedouin descent groups were neither important nor corporate; instead, in the manner of city-dwellers, people associated in numerous and varied corporate groups. I began to wonder whether earlier anthro-

pologists had not given too much weight to descent groups and failed to appreciate the significance of the many other corporate activities.

During fieldwork in the Negev, Bedouin friends often tried hard to convince me that the corporate descent group (*khams*) was concerned only with the defence of its members, and was clearly separate from the domestic group. When they insisted that the members of the descent group were 'close' (*qarib*) to one another, I assumed they were talking about kinship, because the Hebrew term for closeness (*qirva*) derives from the same root and denotes, among other things, kinship. When I asked them whether maternal and affinal relatives were 'close', my Bedouin informants said that they were definitely not; on the contrary, they were distant (*ba' id*). It took me some time to realize that closeness (*qaraba*) referred to agnation, the putative descent link that conferred membership of the corporate descent groups. It also covered the relationships between two allied and intermarrying descent groups (Marx 1967: 174–6). I also discovered that while the Bedouin recognized numerous categories of kin, they thought about each kinsman or kinswoman as a potential partner in a useful dyadic, or at the utmost triadic, relationship. Thus a mother's brother was someone who could be fully relied upon, more than even a father or brother, to help his sister's son in any contingency. Brothers were expected to assist one another; in practice, a man or a woman could make unspecified demands on each of their siblings, from each according to his or her capabilities and resources. The Bedouin felt that their connections with these individuals were the outcome of sharing, or having recently shared, the same domestic environment. They never thought of kinship as a way of ordering relationships, as the organizing principle of groups. They did not even think that kinsmen participated in an ego-centred network of closeknit or loose-knit relationships. It did not occur to me at the time that kinship was a Western concept which anthropologists were applying, by hook and by crook, to all societies. While David Schneider's *American Kinship* (1968) showed conclusively that the anthropologists' conception of kinship was embarrassingly similar to what the American man-in-the-street thought about kinship, we have not yet fully embraced the lesson that kinship is a Western ethnocentric construct. Many anthropologists still consider kinship to be a special field of study (see, for instance, Peletz 1995).

After reading Eickelman's (1989: 156–61) discussion of 'closeness' in Morocco (there too, as elsewhere in the Arab world, it is called *qaraba*), I have come to the conclusion that the term is better translated by corporateness, while being aware that other scholars (for instance Abu-Lughod 1986: 51) translate *qaraba* both as kinship and agnation. Eickelman shows that in the urban neighbourhood he studied the term closeness may refer to 'participation in factional alliances, ties of patronage and clientship, and common bonds developed through neighbourliness' (1989: 156). Members of these corporate groups recognize common interests and have the capacity to act jointly in their implementation. Their association is often expressed as a blood tie, especially when it deals with defending the members' bodily safety. The group is then activated whenever a member's blood has been spilt. The principle applies even when only a drop of

blood was drawn, and in the absence of a wound they can be relied upon to discover a scratch and define it as a wound. Such associations may then be further elaborated as joint descent groups, so as to fit them into the image of a segmentary society made up of a series of ever more encompassing descent groups.

The Sinai Bedouin forced me to discover the many varieties of corporate groups that were operating all around me. For their descent groups were non-corporate and functioned mainly as imaginary links between households and the territorial tribe. Bedouin said that the tribe was corporately responsible for blood revenge, but could not cite any examples from the recent past. On the contrary, they often argued that 'for the last fifty years there has not been a single instance of murder in South Sinai', so that it was hard to see the tribe in action. Yet I could observe the assembled tribes at the various annual pilgrimages. Only when I managed to break away from the notion that corporateness was linked to descent was I able to see the many ongoing corporate activities.

Why do Some Bedouin Remain in South Sinai and Why do Other Bedouin Leave?

I would like to take the argument one step further, by arguing that the Bedouin can live in South Sinai only because they are part of urban civilization. Without it they would simply disappear from the face of Sinai. Consider that the 10,000 Bedouin living in South Sinai find very little pasture for their flocks, and invest much money and labour in herding and in buying supplementary feed for the animals. Their orchards, wrought at great expense out of the rocky ground, do not produce enough fruit and vegetables to satisfy their own needs. Most of their income derives from the city, and almost all their consumption goods are imported from the city. If this argument is correct, then why do the Bedouin bother to stay in such an arid and inhospitable environment as South Sinai? One would rather expect them to move to the city.

There is a twofold answer to this question. First, at least half the members of each generation of Bedouin do actually leave Sinai, or rather do not return to Sinai from their labour migration period. They settle their households permanently in the cities of the Nile valley but even there live among their own tribesmen and retain links to the homeland. They retain the option of a return to the fold of the tribe.

Second, those Bedouin who remain in South Sinai participate in the market economy in many ways, besides horticulture and herding. Life there suits the numerous men and women who produce goods or perform paid services for others. We saw that there are a great variety of such specialists.

South Sinai is good for those Bedouin who use the region as a haven of retirement. When their sons are old enough to take over from them as labour migrants, which may be from the age of 15 onward, these men may be 35 or 40 years old. They look

forward to many years of life at home, in the intimate circle of family, relatives and friends, and to enjoy the social security net provided by these links. They partly rely on the fruit and vegetables grown in their own orchards, drink of their own wells and look forward to the occasional meat dish, preferably from an animal slaughtered by neighbours. For the rest, they are subsidized by the work of their labour migrant sons and through them remain connected to the market economy. By maintaining the orchards and small flocks in good condition, the elders prepare the ground for the time when their sons in their turn choose to retire. As there is one Bedouin whose flock of sixty sheep and goats allows him to eke out a living, and several vegetable farmers in Tarfat Qadren, west of Santa Katarina, who make a living from the sale of vegetables and fruit to local customers, they possess 'living proof' that this economy is workable.

Sinai is also good for men engaged in the smuggling trade, which offers large incomes and honour to the entrepreneurs, and small but often quite regular wages for their employees. The drug trade contributed in the 1950s and 1960s an estimated 30 per cent to the national economy of South Sinai. It has played a major part in the development of oases and orchards in South Sinai. It has convinced the Bedouin tribes that they must live in peace with one another, as a precondition for conducting smuggling operations across tribal boundaries and for keeping the state authorities at bay. This situation has shaped the Bedouin's self-image of being peaceful people among whom, they say, 'there has not been a case of murder for fifty years'. Smuggling has also led the leaders of gangs to establish close links with representatives of the army, police and other agencies of the state. Through these links the smugglers have been able not only to keep the smuggling routes open, but also to avert dangers such as the alienation of tracts of land, and to protect themselves against the arbitrary acts of local officials. While smuggling was very limited during the Israeli occupation of Sinai (1967–82), the smuggling organizations were not disbanded. The smugglers lay low, waiting for a change in the political situation.

South Sinai is good for most women. Strange as it may sound, women have an interest in continuing the system of labour migration. While most of the men work outside the region, the women stay at home and lead relatively protected, and yet independent, lives. They have many responsibilities: they raise the children, run the household, maintain the gardens and, with the help of their daughters, take out the flock to pasture, foster the links with kin and spend the money remitted by their husbands. Thus they accumulate considerable power, which is shown in their selfassured interaction with both members of their own circle and outsiders, such as tourists. As life in the mountains is good for the wives of labour migrants, they do all they can to make the husbands extend their work periods. Returning husbands not only live at a reduced income, but also deprive the wives of their power and independence. No wonder most women are dissatisfied with the husband who returns home (see also Lavie 1990: 122). Women are also quite reluctant to give up their relative independence when the husband desires them to resettle near his place of work.

It appears that a large number of Bedouin wish to remain in, or return to, South Sinai. Yet their number is likely to dwindle over time, as military and commercial installations encroach on their land and, even more critically, on their water supply. The amount of water available to the Bedouin is strictly limited. When the army sets up installations in Sinai, it taps the local water supply with little consideration for the needs of a disenfranchised civilian population like the Bedouin, but also trucks in water from large distances. Hotels on the seashore will also use some local water, but supplement it with desalination plants. The Bedouin have access only to the water left in the ground by these stronger contenders and have no supplementary sources. Therefore, wherever a successful hotel is set up and the number of tourists grows (Hobbs 2001: 212), the Bedouin's water supply is reduced. This happened in the Santa Katarina area, in Nuweba, Dahab and elsewhere. The amount of water available to the Bedouin for irrigation and even for drinking declined in all these areas. When the Bedouin themselves seek to increase cultivation the results are similar. When the owners of gardens in the Firan oasis installed mechanical pumps and increased the cultivated areas in the 1950s, the water level fell rapidly and all the wells had repeatedly to be deepened. When Bedouin developed the Tarfat Qadren oasis near Santa Katarina in the 1960s, the water supply in downstream Firan oasis declined even faster. Eventually the gardeners dispensed with the pumps and returned to the less wasteful manual watering devices.

Neither the hotels nor the agencies of the state employ many Bedouin; they prefer to bring in their own workforce. The Bedouin are not literate enough and their traditional skills are not required by these city-dwellers (Aziz 2000). Both the Bedouin's chances of employment and of improving their indigenous economy thus decrease as South Sinai is 'developed'. The outlook for the Bedouin is bleak. If the young labour migrants do not return to South Sinai, the chain of generations built on the fragile indigenous economy of flocks and orchards will break. The vivid orchards, which have for almost two millennia been an outstanding feature of the high mountains, and are today a major tourist attraction, will dry up. Then the Bedouin will gradually abandon the mountain ranges of South Sinai. More and more of them will join the many Qararsha, Huwetat and Aleqat Bedouin who have already settled in the cities and villages of mainland Egypt. Others will gravitate toward the tourist centres along the east coast of Sinai. As there will no longer be work for all the specialists who were part of the Bedouin economy, they too will gravitate to the mainland or to the tourist centres in Sinai. The smugglers, however, will thrive in the tumultuous tourist market. The Bedouin as individuals will survive and adapt to the changed conditions, as they have always done (Lange 2003). But a unique and complex social configuration will be no more; it will be reabsorbed by the city from which it emerged.

Acknowledgements

References

Abu-Lughod, Lila. 1986. *Veiled Sentiments: Honor and Poetry in a Bedouin Society.* Berkeley: University of California Press.

Abu-Rabia, Aref. 1994. *The Negev Bedouin and Livestock Rearing: Social, Economic and Political Aspects.* Oxford: Berg.

Aziz, Heba. 2000. Employment in a Bedouin Community: the Case of the Town of Dahab, *Nomadic Peoples*, 4(2): 28–47.

Beck, Lois. 1991. *Nomad: A Year in the Life of a Qashqa'i Tribesman in Iran.* London: Tauris.

Burckhardt, Johann Ludwig. 1822. *Travels in Syria and the Holy Land.* London: Murray.

Deluz, Christiane. 2001. Bédouins et pèlerins d'Occident au Sinaï, une difficile rencontre, in *Le Sinaï de la conquête arabe à nos jours*, edited by J.M. Mouton. Cairo: Institut français d'archéologie orientale (Cahier des Annales Islamologiques, no. 21), 183–96.

Eckenstein, Lina. 1921. *A History of Sinai.* London: Society for Promoting Christian Knowledge.

Eickelman, Dale F. 1989. *The Middle East: An Anthropological Approach* (second edition). Englewood Cliffs, NJ: Prentice-Hall.

Éthérie (Egeria). 1957 (385). *Journal de voyage*, Hélène Pétré (transl. and ed.), (Sources Chrétiennes, no. 21). Paris: Edition du Cerf.

Ginat, Joseph. 1987. *Blood Disputes among Bedouin and Rural Arabs in Israel.* Pittsburgh, PA: University of Pittsburgh Press.

Henein, Nessim Henry. 1988. *Mārī Girgis: Village de Haute*-Égypte. Cairo: Institut français d'archéologie orientale.

Henninger, Joseph. 1955. Ist der sogenannte Nilus-Bericht eine brauchbare religionsgeschichtliche Quelle? *Anthropos*, 50: 81–148.

Hobbs, Joseph J. 1995. *Mount Sinai.* Austin: University of Texas Press.

——. 2001 The Sinai Bedouin at the Dawn of the Twenty-first Century, in *Le Sinaï de la conquête arabe à nos jours*, edited by J.M. Mouton. Cairo: Institut français d'archéologie orientale, 207–13.

Humbsch, Robert. 1976. *Beiträge zur Geschichte des osmanischen* Ägyptens: *nach arabischen Sultans – und Statthalterurkunden des Sinai-Klosters.* Freiburg: Schwarz.

Jakubowska, Longina. 1992. Resisting 'Ethnicity': the Israeli State and Bedouin Identity, in *The Paths to Domination, Resistance, and Terror*, edited by C.

Nordstrom and J. Martin. Berkeley: University of California Press, 85–105.

Lancaster, William. 1997. *The Rwala Bedouin Today* (second edition). Prospect Heights, IL: Waveland Press.

Lange, Katharina. 2003. Zwischen den Kategorien – Nomaden, Halbsesshafte, Sesshafte? Das Beispiel der Welde. *Orientwissenschafliche Hefte*, 9: 253–87.

Lavie, Smadar. 1990. *The Poetics of Military Occupation: Mzeina Allegories of Bedouin Identity Under Israeli and Egyptian Rule.* Berkeley: University of California Press.

Leder, Stefan and Streck, Bernhard (eds). 2005. *Shifts and Drifts in Nomadic–Sedentary Relations.* Wiesbaden: Ludwig Reichert.

Mafeje, Archie. 1975. Religion, Class, and Ideology in South Africa, in *Religion and Social Change in Southern Africa: Anthropological Essays in Honour of Monica Wilson*, edited by M.G. Whisson and M. West. Cape Town: David Philip, 164–84.

Marx, Emanuel. 1967. *Bedouin of the Negev.* Manchester: Manchester University Press.

——. 1977. Communal and Individual Pilgrimage: the Region of Saints' Tombs in South Sinai, in *Regional Cults*, edited by R.P. Werbner. London: Academic Press (ASA Monograph, no.16), 29–51.

——. 1980. Wage Labor and Tribal Economy of the Bedouin in South Sinai, in *When Nomads Settle: Processes of Sedentarization as Adaptation and Response*, edited by P.C. Salzman. New York: Bergin, 111–23.

——. 1984 'Changing Employment Patterns of Bedouin in South Sinai', in Emanuel Marx and Avshalom Shmueli (eds). *The Changing Bedouin*, New Brunswick: Transaction, 173–86.

——. 1987. Labour Migrants with a Secure Base: Bedouin of South Sinai, in *Migrants, Workers and the Social Order*, edited by J.S. Eades. London: Tavistock (ASA Monograph, no. 26), 148–64.

——. 1992. Are there Pastoral Nomads in the Middle East? in *Pastoralism in the Levant*, edited by O. Bar-Yosef and A. Khazanov. Madison, WI: Prehistory Press, 255–60.

——. 1996 Are there Pastoral Nomads in the Arab Middle East? in *The Anthropology of Tribal and Peasant Pastoral Societies*, edited by U. Fabietti and P.C. Salzman. Pavia and Como: Collegio Ghislieri and Ibis, 101–15.

——. 1999. Oases in South Sinai, *Human Ecology*, 27(2): 341–57.

——. 2004a. Roving Traders Among the Bedouin of South Sinai, in *Customary Strangers: New Perspectives on Peripatetic Peoples in the Middle East, Africa, and Asia*, edited by J.C. Berland and A. Rao. Westport, CT: Praeger, 57–69.

——. 2004b. Bands and other Corporate Hominid Groups in Acheulian Culture, in *Human Paleoecology in the Levantine Corridor*, edited by N. Goren-Inbar and J.D. Speth. Oxford: Oxbow, 89–104.

——. 2005a. Nomads and Cities: the Development of a Conception', in *Shifts and Drifts in Nomadic–Sedentary Relations*, edited by S. Leder and B. Streck.

Wiesbaden: Ludwig Reichert, 3–15.

——. 2005b. The Bedouin's Lifeline: Roving Traders in South Sinai, in *Social Critique and Commitment: Essays in Honor of Henry Rosenfeld*, edited by M. Al-Haj et al. Lanham, MD: University Press of America, 193–206.

——. 2006a. The Political Economy of Middle Eastern and North African Pastoral Nomads, in *Nomadic Societies in the Middle East and North Africa: Entering the 21st Century*, edited by D. Chatty. Leiden: Brill, 78–97.

——. 2006b. Tribal Pilgrimages to Saints' Tombs in South Sinai, in *Archaeology, Anthropology and Cult: The Sanctuary at Gilat, Israel*, edited by T.E. Levy. London: Equinox, 54–74.

——. 2008. Hashish Smuggling by Bedouin in South Sinai, in *Organized Crime: Culture, Markets and Policies*, edited by D. Siegel and H. Nelen. New York: Springer, 29–40.

Mouton, Jean-Michel. 2000. *Le Sinaï médiéval: Un espace stratégique de l'Islam*. Paris: Presses Universitaires de France.

Mouton, Jean-Michel (ed.) 2001. *Le Sinaï de la conquête arabe à nos jours*. Cairo: Institut français d'archéologie orientale (Cahier des Annales Islamologiques, no. 21).

Niebuhr, Carsten. 1968 (1774). *Reisebeschreibung nach Arabien und den umliegenden Laendern*, 3 vols. Graz: Akademische Druck – und Verlagsanstalt.

Palmer, Edward Henry. 1871. *The Desert of the Exodus*, 2 vols. Cambridge: Deighton, Bell.

Peletz, Michael G. 1995. Kinship Studies in Late Twentieth-century Anthropology, *Annual Review of Anthropology*, 24: 343–72.

Peters, Emrys L. 1990. *The Bedouin of Cyrenaica: Studies in Personal and Corporate Power*, edited by J. Goody and E. Marx. Cambridge: Cambridge University Press.

Robinson, Edward. 1867. *Biblical Researches in Palestine and the Adjacent Regions*, 3 vols. London: Murray.

Salzman, Philip Carl. 1980. Introduction: Processes of Sedentarization as Adaptation and Response, in *When Nomads Settle: Processes of Sedentarization as Adaptation and Response*, edited by P.C. Salzman. New York: Bergin, 1–19.

——. 2004 *Pastoralists: Equality, Hierarchy, and the State.* Boulder, CO: Westview.

Schneider, David M. 1968. *American Kinship: A Cultural Account.* Englewood Cliffs, NJ: Prentice-Hall.

Schwarz, Klaus. 1970. *Osmanische Sultansurkunden des Sinai-Klosters in türkischer Sprache.* Freiburg: Schwarz.

Shuqair, Na'um. 1916. *Ta'arikh sina al-qadim wal-hadith wa-jighrafiatuha (The History and Geography of Ancient and Modern Sinai)*. Cairo: Ma'arif Press.

Stewart, Frank H. 1991. Notes on the Arrival of the Bedouin Tribes in Sinai, *Journal of the Economic and Social History of the Orient*, 34: 97–110.

Werbner, Pnina. 2002. *Imagined Diasporas among Manchester Muslims*. Oxford: Currey.

Wilson, Charles W. and Palmer, Henry S. 1869–71. *Ordnance Survey of the Peninsula of Sinai*, 5 vols. Southampton: Ordnance Survey.

Chapter 3: Seductive Communities: On Non-representational Constructions

Haim Hazan

In this chapter I offer a reading of three different narratives of 'community' as enacted in an urban renewal project in an impoverished neighbourhood whose lived-in experience is thus reconstructed and revitalized as a dreamt-of reality (Hazan 2001). This reading will demonstrate how 'community' is an image, whose allure allows it to simulate and substitute basic reality without reflecting it.

My intent in focusing on 'community' as a cultural image is to show how 'culture' is itself a narrative, an emergent, multi-layered, rhetorical enactment rather than a pre-established, already-given, static and objective abstraction 'out there'. The view of 'culture' as a narrative rather than an objective entity (see Carrithers 1992; Munich and Smelsa 1992) has opened up a whole array of critical self-examination in the social sciences. Not only the social, but also the social sciences have come to be regarded as a narrative – such as 'the writing of culture' (Clifford and Marcus 1986; Tyler 1986; Clifford 1983), a kind of rhetoric (Rosaldo 1987) or second-hand representation (Myerhoff and Ruby 1982; Pels and Nancel 1991).

This 'narrative moment' (Maines 1993) in the social sciences brings it closer to views such as that of Baudrillard, who conceptualizes contemporary mass cultures in terms of 'simulations.' The notion of narrative replaces the former functionalist mimetic models of sociology with a literary emphasis on the ambivalence of representation. This ambivalence, and its ensuing search for alternative models of representation, have come to occupy an important place in postmodernist sociology (see Denzin 1991; Tyler 1986; Lash 1990). This interest has certainly gained Baudrillard a wider audience. However, his theoretical work is more often being dismissed than favoured by 'mainstream' sociologists. In the words of Bogard (1990: 1),

> Baudrillard's only self-proclaimed sociological work, 'In The Shadows of the Silent Majorities' (1983a), which he cites elsewhere as his effort to put an 'end to the social, or at least the concept of the social,' will not win him much favor among true believers (cf. Baudrillard 1987: 184). The latter will probably dismiss such remarks out of hand. This, however, would be

unfortunate, for beyond being merely provocative, many of Baudrillard's views on contemporary society, culture and social theory are both original and incisive.

In my analysis of 'community' I offer a closer reading of one central aspect of Baudrillard's work by using the ethnography presented here as a case study in his successive phases of the image. As far as I know, not many sociologists have attempted to demonstrate the practical applicability of Baudrillard's ideas to 'concrete' social ethnographies. I therefore offer the reading presented here as such a demonstration of Baudrillard's relevance to anthropology.

The ethnographic scene which I now turn to invoke is an Israeli 'renewal neighbourhood'. This concept became part of Israeli social life after the initiation of 'The Program for Rehabilitation and Renewal of Neighbourhoods in Israel' in 1977 by the government of M. Begin (see Carmon and Hill 1984, and The International Committee on Project Renewal Evaluation, 1981). The following section is an invocation of one such renewal neighbourhood, beginning with the 'hard facts' that determined the day-to-day conditions of existence of the residents, and then proceeding to the world of images and metaphors which residents 'live by' (Lakoff and Johnson 1980; Wagner 1986).

1. The Field

The neighbourhood in which the research was conducted – which I shall call Arod – is part of a city situated in the centre of the country, and has a population of about six thousand (for more details see Hazan 1990). It is located on the outskirts of the town and is separated from most of its other residential areas by open land. Having had a nucleus population of Yemenite origin, Arod absorbed two main waves of immigrants: one, in the mid-50s, was composed of Ma'abara evacuees (residents of 'transition camps' for new immigrants, erected in the 1950s) of various countries of origin; the second consisted of east European couples who settled in the area in the early 1970s.

A combination of unemployment, crime, poor housing conditions and inferior standard of education classified Arod as a 'deprived' area[1] and turned it into a Project Renewal controlled borough in 1979. Project Renewal was set out to replace the 'ruins of past neglect and deterioration' with a whole new way of life. Within this context of 'urban rehabilitation', the idea of community played a cardinal theme around which

---

[1] In 1982, the rate of those with partial or complete elementary education among the 15–44 age group in the neighbourhood was 30 per cent, and the rate of non-employed was above the national average: 48.7 per cent among men and 72 per cent among women (the respective rates in the general population are 32.9 per cent and 55.8 per cent). The level of family income was also relatively low, standing at 34 per cent below the national average.

the project revolved and toward its accomplishment it was geared. A unique rhetoric of community hence emerged alongside with the Project, enmeshing the 'deprived locality' to be 'rehabilitated' within the symbolic realm of the state of Israel, Zionist aspirations and the Diaspora. This rhetoric turned the Project into a self-generating myth offering a narrative of creation and genesis bridged over cultural diversities and social tensions.

Pervasive and intensive as it was, the advent of the project was meted out with a great deal of apprehension, cynicism and mistrust from its designated targets – the inhabitants of the 'renewed' neighbourhood. However sceptical of the objectives of the grand scheme proposed by the project, the residents of Arod were mindful of the cultural channel through which benefits could be obtained. The vocabulary of 'community' thus became omnipresent among them and project staff alike. It is the rhetorical nature of that omnipresence, as well as its social accountability, that this chapter seeks to unravel. The fact that it was the idea of 'community' which was chosen as the magic potion for transforming 'deprivation' into 'renewal', can be explained by that idea's unique discursive properties. Community is supposed to efface and surpass diverse interests, conflicting relationships and personal idiosyncrasies to provide its members with a sense of belonging to and identifying with. To use Durkheim's (1982 [1895]) expression, it is a social representation whose temporal dimensions reach the past, be it historical or mythical, and extend into the future, be it foreseeable or millennial. This mixture of commonly-accepted denotation and lack of definiteness, of international meaning (Warren 1973; Schneider 1979) as well as unique locality (Cohen 1985), render the concept of 'community' analytically questionable, turning 'community studies' into a myth, as Stacey (1969) suggested. It was precisely that myth-like opaqueness, however, which made the concept of community into the perfect symbolic vehicle for the purposes of Project Renewal.

Indeed, Project Renewal played a decisive role in imparting, justifying and disseminating the language of community. Residents learned that in order to communicate usefully with project officials they would be advised to adapt their speech to the argot of the organization that concentrated such vast resources. The ability to negotiate a share in the distribution of resources, such as housing allowances, establishment of various clubs in the vicinity of their homes, positions on the Project's staff etc., was largely determined by the residents' contacts with local centres of power. These centres – local committees, office-holders owing their livelihood to the Project, various community services – were all part of the project's organizational structure, and thus partook in the ongoing organizational discourse. The communicative power of the Project's language thus acquired a local dominance that relied on ideological hegemony, according to which the Project's objective was to enable the local community to merge with (or become acculturated to) 'mainstream' Israeli society while abolishing the neighbourhood's (and residents') original identity/stigma.

According to the neighbourhood's residents, the concept of 'community' was not in daily usage before the project entered their lives. Residents defined 'community' not

on its own grounds but always in contrast to other terms – namely, that it was neither a 'development area', nor a 'special attention area', nor a 'deprived neighbourhood'. It signified rather a framework that transforms all these into a desirable way of life without any negative connotations, suited to the modern, Western world to which it aspires. This Western world was represented in the Project as an ideal model comprising quality of life, technological sophistication, comfortable housing and attractive surroundings. Thus, Project Renewal worked hard to introduce computer courses, various educational clubs and sports activities into the neighbourhood. All these were supposed to comprise a cultural mechanism whereby the symbolic entity called 'residents' was to elevate itself from the stigma of its neighbourhood. 'Community' as an embodiment of Western progress was also a concrete entity manifested by the adopting twinned communities from the Diaspora.[2] These were regarded not only as a bountiful source of funds that would help the neighbourhood to develop into a 'community', but also as a model to be emulated, and to which the neighbourhood was now linked by a strong, continuous bond, expressed in reciprocal visits, hospitality and joint planning of services and facilities, which had replaced the 'haphazard and casual connection of the past'. The concept was freely bandied about at every opportunity. It was easier to talk of 'communities' rather than of 'neighbourhoods', since the expression did not require territorial definition or accuracy, was free of unpleasant connotations, and enabled the establishment of a rhetorical relationship of equality between adopter and adopted, purveyors of services and their clients, residents and establishment figures. It replaced existing images with new ones, and belonged to the language of 'renewal' and 'well-being', rather than the language of 'development' and 'welfare'. However, the most important characteristic was probably the nucleus common to every settlement or group of people seeking a future with a quality of life independent of any previous economic, social or cultural background. The proliferation of 'community settlements' during this period, and the attendant media campaign encouraging the young, energetic and ambitious to join them, added a taste of be1onging to a different Israeli society, one that was able to shed the impediments of past divisions. The aspiration to become a 'community' was presented by the Project workers in Arod as a vision of a different social future, which the neighbourhood's residents would be able to realize only if they were to change beyond recognition. The binding quality of the concept of 'community' facilitated rhetorical patching up of differences and rivalries that were presented as the outcome of local disputes, most of which involved different ethnic groups. Ethnicity was indeed an additional characteristic of 'neighbourhood' life that had to be blunted in order to join up with 'mainstream Israel', or 'First Israel', as it was labelled in contrast to the 'second Israel' of the margins.

---

[2] The principle of 'adoption' by a 'twin' community from the diaspora was one of Project Renewal's innovations, and enabled direct transfer of funds to the adopted community, and active participation on the part of the adopting community in the physical and social planning and in the distribution of resources in the locality.

2. Three Narratives of Community

The following section describes three accounts of how the neighbourhood was cleansed of its residents in order to emerge as a 'community'. That is, these accounts tell and enact the story of cultural effacement and change. The first two accounts maintain representational relationships with the neighbourhood – albeit by means of a reflexive wink in the second case whereas the third dispenses with representational relationships and creates a simulation as an alternative to both the neighbourhood and the residents.

A. 'The Disguised Egg'

The neighbourhood's elderly residents constituted only a secondary target for the Project. A large and active day centre had been opened for them, but their status within the 'community' was problematic. They were seen as representatives of the former, pre-Project ethnic neighbourhood identities, and many of them received welfare support. The welfare services were perceived as a threat to the creation of the community within the Project, since their existence perpetuated the stigma of poverty. It had thus been decided to separate physically the community centre from the welfare office, which had been moved to another street. Nevertheless, a series of activities for the elderly were initiated, aimed at integrating them into the community. The first and most important of these was a literacy program. The idea of integration into the community was presented by the wish to develop inter-generational relationships between grandfathers and grandmothers and their grandchildren, by means of enabling the grandparents to assist their grandchildren with their homework and in reading stories. Only elderly women participated in the course, since the neighbourhood's old men were literate in Hebrew and spent most of their time in the local synagogues, which served as social centres for them. The participants did not attach much importance to the stated objective of learning to read and write. Some even ridiculed the idea, claiming that the grandchildren did not need their assistance in doing homework, adding that the relationship with them was based, as one of them put it, on 'what's in the heart and what's in the pot'. As is usual in literacy classes for new immigrants, the course material was very simple, consisting mainly of children's stories. At the end of the course the participants were asked by their instructor to write an essay that would demonstrate their command of the language by describing the particular study material that had made the strongest impression on them. The essays were collected and published in booklet form for local distribution. Without exception the women chose one story – 'The Disguised Egg' – by Dan Pagis, a well-known Israeli poet and writer. This story recounts the adventures of an egg that became fed up with its eggy identity and decided to seek its fortune in other roundish forms in which it hoped to find true happiness. This journey of self-revelation passes through a balloon, an apple, a ball and other oval-shaped objects, but brings only disappointment. The egg eventually

reaches the conclusion that it cannot change into something else, returns with relief to its egg shape and hatches a chick. The women, each in their own words, offered almost identical explanations as to why the story had struck a chord with them. They saw the text of the story as an allegory of their own fate. They had immigrated to Israel from the countries of their birth, whose culture they had internalized. In Israel they had passed through various phases, at each of which an attempt had been made to transform their basic identity into something else. In the immigrants' camp they had been expected to become Israelis, and later on in the neighbourhood others had tried to impart a culture foreign to them. They claimed, however, that their true selves were to be found only in their origins, in their undisguised ethnic affiliations. Some went still further in developing this theme, remarking in their essays that only once it had accepted itself for what it was could the egg become fertile. In their case, too, only once they had resigned themselves to what they were, not through necessity but by choice, would they be of any use to their grandchildren. The main lesson to be learned from the parable of the disguised egg is therefore that one's disguise must be removed before one can know his true self and view himself as a complete person. Removal of these disguises, however, means the abrogation of Israeli reality in general and that of the neighbourhood in particular. Thus, the story is employed as meta-narrative capable of re-narrating experience.

B. 'The Natural Selection'

In order to mark the Project's first anniversary, the Project board met to discuss ways of celebrating the event. The participants, many of whom were residents of the neighbourhood, made various light-hearted proposals as to 'how to entertain our residents'. Someone suggested a boxing contest, since 'if they don't see blood they won't be satisfied'. Another suggested a 'Moroccan feast', because 'that's what suits them and what they'll enjoy most'. There was a general consensus that this type of entertainment would please the residents, attract an audience and meet their expectations. The initial proposals, however, which had obviously been made in jest, and betrayed more about attitudes towards the residents and the neighbourhood than any intention of implementing them, were rejected. The reasons put forward for their rejection were of the kind that 'the Project can't be identified with events such as these', and in particular: 'Who can tell how the residents will behave? Perhaps they will go wild and run riot, and then what will we look like to the invited guests?' The consideration of the community's appearance to outsiders was sufficiently weighty to induce more serious suggestions. Thus, for example, the idea was put forward to invite the 'Pale Tracker' (Hagashash Hachiver) entertainment group, well-known for its ethnic humour, 'so that the residents can laugh at themselves a bit', but this was rejected because of the high cost involved. Agreement was eventually reached on holding an exhibition of armaments to be set up by the army unit adopted by the residents, and on inviting a performance by 'The Natural Selection' group, to be held at the centre

of town rather than in the neighbourhood. This is a musical ensemble that strives to lend an authentic classical air to oriental music, thereby staking its claim to status and legitimacy in the field of serious music. The ensemble had won critical acclaim and recognition and was considered to be 'civilized' entertainment as opposed to 'cassette music' with which popular oriental music is often identified in Israel. The choice of the city as the venue for the performance was not fortuitous. The excuse that no suitable place was available was unconvincing, since well-attended events had taken place in the neighbourhood, where a suitable hall had been found. The territorial detachment was part of the cultural isolation. Bringing Arod's residents to the city symbolized the neighbourhood's abolition both for its residents and the Project team. The event was widely publicized, but demand for tickets was small, and much of the audience at the performance was made up of non-residents. The complete disappearance of the neighbourhood and its residents will be demonstrated in the following narrative.

C. 'An Invitation to the Community'

The inauguration ceremony for Arod's community centre was held before a large gathering. Careful examination of the invitation sent to guests reveals some of the main characteristics of the concept of community and the ways of implementing it. Invitations were sent out to neighbourhood notables, activists in Project Renewal, Jewish Agency officials, representatives of government ministries, senior municipal officials; and to many other guests from all parts of the country, and in particular to contributors from the twin community in the United States. The invitation's design indicates its purpose and significance. It was printed on top quality paper in Hebrew and in English. Apart from its value as a communication bridge between the two communities, the use of English establishes our community's standing within a wider, modern, Western and universal culture, one of whose ingredients is the English language. Thus, the neighbourhood not only breaks out of the narrow confines of 'the second Israel', but also extends beyond mainstream Israel and Israel altogether. With a mere wave of an invitation the neighbourhood becomes part of the sought after, rational, scientific, free and 'cultured' world.

The invitation was headed by the emblems of the various bodies involved in establishing the centre – first, the coat of arms of the city in which the centre was located, followed by the insignia of the Jewish Agency, of the twin city abroad and finally of the neighbourhood itself. The name attached to the illustration betrays the problematic nature of the centre's essential function. In Hebrew it is called a 'palace of culture and sport', whereas in English it is a 'community centre'. The Hebrew language imparts a combination of a 'modern' meaning to the centre (culture) and a religious one (palace). This combination has become part of everyday speech, indicating a site for ongoing activity, unconditional of any specific time and place. The concept of 'community', on the other hand, is identified with a given time and place, and as we have seen, the people of the neighbourhood seek to break out of the problematic confines of their place

and time, in other words, of the neighbourhood. This internal contradiction between the necessity to be part of an existing reality and the desire to repudiate it and to regard it as part of an alternative and sought after life cycle is evident from the other parts of the document in question.

As in other invitations to family events, such as weddings, circumcisions or barmitz-vahs, the parents/in-laws' also appear in Arod's invitation. In this case, these are the Jewish Agency, the municipality and the neighbourhood committee together with the twin Jewish community. These three 'take pleasure in inviting you to the inauguration ceremony of the Palace of Culture and Sport ... which will take place on ...'

As in other family events, the inauguration ceremony was a rite of passage; that is, a festive-symbolic designation of the important and marked change that has taken place in the identity of the participants. During the ceremony outlined in the invitation, a neighbourhood 'becomes' a community. Yet this narrative of 'becoming', rooted in the very essence of the project of 'renewal', was actually a simulation, a staged production bearing little or no connection to the lived experience of residents.

During the Inauguration Ceremony, the heads of Project Renewal and its patrons made speeches before a selected audience that had gained admittance to the grounds of the centre by invitation only. An appropriate and evenly balanced audience was thus ensured: an audience that could be relied upon not to disrupt the ceremony and to bring credit to the organizers. The *mezuzot* (parchment scrolls) were fixed in place amid much song and dance led by a well-known rabbi from another part of the country. This is an interesting fact, when one considers the large number of synagogues and rabbis in the neighbourhood. The invitation of a rabbi from the outside did not only avoid local factionalism, but also imbued the neighbourhood with an aura of belonging to the entire religious establishment. Following a tour of the building's components, in particular of the library, as befits a 'palace of culture', groups of neighbourhood women appeared before the guests, dressed in original ethnic attire. The picture was one of cultural diversity whose roots were not in the present time and place, and which, moreover, presented the 'authentic' people of the neighbourhood as a piece of folklore. The gulf between the 'exotic' apparel and ornaments and the modern, European dress of the visiting members of the American 'twin' community placed the former in the category of a museum exhibit, in contrast to the modern power and vitality of the latter, who, moreover, were economically in a position to enforce their way of life on the local residents. The image of this cultural subjugation was artistically formed during the following stage of the ceremony.

The program was presented to an invited local audience that filled the lavish hall to capacity at the beginning of the evening, but which steadily dwindled, with only a few people remaining to the end. This did not prevent the program running its full course. It included an appearance by the municipal orchestra, which set the seal of approval on the acceptance of the out of the way neighbourhood into the fold of the city to which it belonged. The inclusion of the neighbourhood in the city was affirmed immediately afterwards during a series of greetings and speeches made by the mayor,

a senior government minister who read out a telegram of greeting from the Prime Minister, representatives of the neighbourhood and of the Project, and a representative of the twin community. In the cultural performances that followed, the centre's dancers appeared in jazz dance, devoid of any folkloric elements, while a local school choir sang songs from musicals, many of them from *Fiddler on the Roof.* The high point of the evening was a performance by a well-known singer who combined 'good old Israeli' songs, with which the new community could now identify, with a visual 'then and now' slide show. Pictures of the neighbourhood's degenerate and neglected past were shown side by side with those portraying the present achievements of an exemplary, well-tended, clean and orderly place. The contrast was so blatant that there seemed to be no connection between the two periods, and the audience was no doubt torn between its degrading past as residents of an isolated neighbourhood and the present with its promise of a bright future as citizens of the nation, the country and the world. In fact, only a few of them remained in their seats to the end of the event. Most of them made their exit during the early stages of the evening; leaving behind them the young people and the local hacks.

This vision of the future community created by the Project, spells the abolition of the neighbourhood and its residents as a significant entity worthy of mirroring or even winking at, as was the case in the former ceremony described. It is also an abrogation of every system of symbols that connected the neighbourhood and its residents to the concept of the community. The community has; in fact; become a self-display with no 'self, a signifier with no signified', or in the words of Baudrillard (1983b; 1988a; 1988b), a 'simulation'. I elaborate on the symmetry between Baudrillard's successive phases of the image and the residents' views of 'community' in the conclusion.

3. Conclusion: The Death of the Social?

Having thus told three illustrative accounts of the concept of 'community' as a mediator of cultural effacement and change, I now offer a new reading of them by using Baudrillard's theory of the social or presumably the death of the social. First I argue for the symmetry between Baudrillard's phases of the image and the three narratives of 'community'. Then I proceed to suggest how Baudrillard's ideas can be incorporated into 'Continental' sociological thought without having to relinquish the analytical framework of contemporary sociology. In other words, I offer this case study as an example of how to 'upgrade' sociology by adding a 'Baudrillard software' to its current hardware.

For Baudrillard, the term 'simulation' – which I previously associated with the third narrative, the inauguration of community – describes an essential property of modern mass cultures. It refers to the reproduction of a state of affairs which simultaneously masks the absence of the state of affairs it claims to represent (cf. Baudrillard 1983b). A simulation 'pretends' to be something which no longer exists because it was re-

placed by its simulation. To use another sociological jargon, simulations – like staged TV documentaries (the Gulf War included), Hollywood cinema, wilderness 'resorts' or Disneyland are hyper-commodified cultural reifications. There are, however, two distinct types of simulation. These two types correspond to specific historical phases which mark a difference in the logic and function of 'images'. It is the third phase that creates a space for simulation, while the fourth phase marks it perfection. The first type of simulation (the third phase) still retains a reality principle that distinguishes between a state of affairs and its representation. In the second type of simulation (the fourth phase) the reality principle disappears, and signs of the real substitute the real itself. The ultimate example of this fourth stage, for Baudrillard (1988b: 56, 104), is America: 'a cinematic society ... a Disneyland where the cinema and TV are our reality'. In America one can also find a possible historical script of the change from the third to the fourth phase of simulation. Modern American society, so this script goes, has largely known itself through the reflections that Hollywood produced (Ewen and Ewen 1979), while postmodernist American society has further internalized that process, so that 'the real' (which exists only as long as it is screened) is now judged against its cinematic-media counterparts (see also Denzin 1991; Jameson 1991).

A summary of this four-phase transformation is found in Baudrillard (1983b: 11), where he states that the successive phases of the image are as follows:

(The image) is the reflection of a basic reality.

It masks and perverts a basic reality.

It masks the absence of a basic reality.

It bears no relation to any reality whatever: it is its own pure simulacrum.

This chapter portrayed three contexts for the narrating of community: a literacy class, the 'Project Renewal' anniversary, and the 'community' inauguration ceremony. All three contexts involved a group of people telling and enacting their story of 'community', of the possibility of change, of renewal. All three contexts present a rather negative and futile perspective of cultural change. The elderly women of the literacy class tell the story of the 'disguised egg', the story of the impossibility and futility of changing one's original cultural identity. The subtext of this narrative (later clarified to me during private talks with many of these elderly women) spells the impossibility and futility of the concept of 'community', which comes to stand for the imposed process of cultural change. This narrative of 'community' belongs to Baudrillard's second phase. It regards 'community', or any other disguise of an imposed cultural change, as masking and perverting a basic reality: the reality of one's core cultural identity. This narrative, as Baudrillard suggested, is premised on a former, more basic (and perhaps more 'naive') phase of the image, namely that it is the reflection of a basic reality. Community, in the eyes of the elderly narrators, should be (and exists only as long as it is) the reflection of a basic reality.

The two following narratives abandon these two 'naive' premises, or phases, altogether. They represent, in the words of Baudrillard, a higher level of seduction. The seducing power here is evidently the spectre of 'community'. In the second narrative, the anniversary of Project Renewal invokes and enacts the image of community to mask the absence of a basic reality. It is simulation, but of the first type. The existence of a reality principle in that context is revealed in the 'humorous reference' of the Project Board members to the residents of the neighbourhood. Finally, in the third narrative, that of 'community inauguration', no reality principle is sustained. The reality principle has disappeared along with the ceremony's audience. This third narrative in fact exists in a world of its own, a world devoid of audience participation or reality principles, a world of pure simulation, of bricolage. It signified no reality, only ideological facades.

The simulatory nature of community, it should be noted, was grounded in several concrete facets, or dilemmas, of everyday life in Arod as a 'renewal neighbourhood'. The first dilemma arose with the arbitrary delineation of geographical boundaries serving as a physical layout of the sponsored community. The incumbents of this geographically-zoned community were thus artificially selected, although involved in variegated life-worlds and differing in sociocultural and demographic aspects. This territorial delineation, common to many of today's designated 'communities', has contributed to the distance between the concept of 'community' on one hand and the basic reality of life in the neighbourhood, on the other. The second dilemma was generated by the complex task of 'renewal', which wishes to discard stigmatic elements of 'ethnicity' and 'deprivation' through an ablution in 'community' involvement, although the materials for the content of such involvement are inevitably drawn from the pool of ethnic images and social problems. Thus, by reinforcing 'community', the undesirable stigmatic elements are further highlighted rather than dimmed. The third dilemma stems from the attempt to establish a self-contained, relatively independent enclave of local government, based on the false assumption that outside intervention should and can be avoided. Having to draw on external support in order to set up community symbols such as recreational centres and care facilities, the advocacy of an autonomous community becomes a ritualized metaphor rather than a viable social force. Finally, the fourth dilemma draws on the imminence of the termination of Project Renewal, on the one hand, or the ominous prospect, for some people, of being trapped in it indefinitely, on the other.

Exposing the ideological facades of 'community' and its practical failure in terms of participation and identification, our three narratives tell a similar story. They signify not acculturation into 'community' but the opposite – namely the collapse of the ideology of acculturation, the bankruptcy of the idea that a new culture can be freely imposed on some passive recipients. The story of modernization, so often told by development anthropologists and 'Project Renewal' officers alike, is completely missing from the first narrative, and appears in the second and third as an ideological facade, a simulation masking, perverting and ultimately discarding the basic reality of 'the

residents' subjected to it. It appears to be one of the disguises worn by the egg from the first story. Simulations, therefore, are exposed here from their dark side, as mechanisms of cultural regulation. It is a side which Baudrillard, at least after his 'For A Critique of The Political Economy of the Sign' (1981), has immersed himself in rather than attempted to criticize.

Having recounted these three narratives of acculturation, it is time to consider the fourth. In spite of the variety and richness to be found in Israeli society, anthropologists who have researched the 'communities' emerging in immigrant settlements or deprived neighbourhoods did it in a straightforward, functionalist manner. Generally speaking, anthropologists have largely refrained from elaborating the views of residents regarding their 'community', and almost never attempted to describe it as a playful, winking performance; or, even worse, an empty simulation. In contrast, anthropologists (in Israel and elsewhere) largely imposed their own serious view, their own functional narrative of community-asdevelopment. In this they were no doubt reproducing the ideological worldview of the state institutes (in Israel, usually the Jewish Agency) employing them, and the regime of 'nation building' as indoctrinated by the Israeli political establishment, of which they were part (see also Kimmerling 1992; Ram 1989). Many socioanthropologists doing 'community studies' among marginal social groups (such as the elderly, new immigrants, African-American people, Third World residents, etc.) have in fact reproduced the simulation of 'community', thus participating in the cultural regime of control that produced and imposed this simulation in the first place. This critique of anthropology and its 'colonial encounter' or the politics of ethnography in 'developing' societies have been piling up (see Escobar 1991; Apffel-Marglin and Marglin 1990; Asad 1973; Bennet and Bowen 1988; Clifford and Marcus 1986). While Baudrillard himself has 'closed down the social', abandoning theoretical critique of the culture industry (an abandonment fully rationalized and argued for in his later works), his insights into mass culture's seductive mechanisms of regulation are very relevant to such a critique. As Bogart (1991: 3) notes,

> Baudrillard's reflections on the contemporary atomization and levelling of social life are deeply rooted in a long tradition of social and critical, theory ... From Marx's critique of alienation and commodification, through Durkheim's study of anomie, Weber's 'iron cage', Adorno's fragmentation of culture, Habermas's colonization of the lifeworld, Foucault's end of man, and Bataille's notions of a general economy of excess and the contemporary devaluation of inner experience.

If we maintain that simulations, while being a central part of contemporary society, are not all-encompassing, we may achieve a more elaborated, multifaceted view of the social. The analysis offered in this chapter was both premised on and trying to demonstrate how various cultural narratives, various phases of the image, can be read into one social text. For this purpose, I have focused my analysis on how a social text can simultaneously contain all the phases which Baudrillard conceptualizes

within a historical scheme. This social text, to borrow Konrad Lorenz's phrase (1952 [1949]), exemplifies how ontogeny (the particular implementation of project renewal) recapitulates phylogeny (the general, abstract phases of the image). Transitory periods of 'renewal', 'regeneration' and 'acculturation' arguably offer many texts like this. Sociology should learn how to undo the various phases of the image from the social texts of contemporary society. The growing scholarly preoccupation with the characteristics of cinematic society might provide sociology with cues as to the metamorphosis from the death of the social into its simulated alternative as seen on TV. Thus community might be doomed to social disappearances and yet, like love (Swidler 2001), community could be rekindled by projecting its enacted notion on small and big screens.

References

Amit, Vered. 2002. Reconceptualizing Community, in *Realizing Community*, edited by V. Amit. *Social Relationships and Sentiments*. London and New York: Routledge, 1–20.

Apffel-Marglin, Frederique and Marglin, Stephen. A. 1990. *Dominating Knowledge: Development, Culture and Resistance*. Oxford: Clarendon Press.

Asad, Talal (ed.) 1973. *Anthropology and the Colonial Encounter*. New York: Humanities Press.

Baudrillard, Jean. 1981. *For a Critique of the Political Economy of the Sign*. St Louis: Telos Press.

——. 1983a. *In the Shadow of the Silent Majorities ... Or the End of the Social*. New York: Semiotext(e).

——. 1983b. *Simulations*. New York: Semiotext(e).

——. 1987. *Forget Foucault*. New York: Semiotext(e).

——. 1988a. *The Ecstasy of Communication*. New York: Semiotext(e).

——. 1988b. *America*. London: Verso.

Bennet, John and Bowen, John (eds) 1988. *Production and Autonomy: Anthropological Studies and Critiques of Development*. Lanham, MD: University Press of America.

Bogard, William. 1990. Closing down the Social: Baudrillard's Challenge to Contemporary Sociology. *Sociological Theory*, 8(1): 116.

Caplan, Pat. 1988. Engendering Knowledge: The Politics of Ethnography. *Anthropology Today*, 4(5): 8–12; 4(6): 14–17.

Carmon, Naomi and Hill, Moshe. 1984. Project Renewal: An Israeli Experiment in Neighborhood. *Habitat International*, 8: 117–32.

Carrithers, Michael. 1992. *Why Humans Have Cultures*. Oxford: Oxford University Press.

Clifford, James. 1983. On Ethnographic Authority. *Representations*, 1(2): 118–46.

——. 1986. On Ethnographic Allegory, in *Writing Culture: The Poetics and Politics of*

Ethnography, edited by J. Clifford and G. Marcus. Berkeley: University of California Press, 98–121.

Clifford, James and Marcus, George (eds). 1986. *Writing Culture: The Poetics and Politics of Ethnography.* Berkeley: University of California Press.

Cohen, Anthony P. 1985. *Symbolic Construction of Community.* London: Tavistock

Denzin, Norman K. 1991. *Images of Postmodernism: Social Theory and Contemporary Cinema.* London: Sage.

Durkheim, Emile. 1982 (1895). *Rules of Sociological Method.* New York: Macmillan Press.

Escobar, Arturo. 1991. Anthropology and the Development Encounter: the Marketing of Development Anthropology. *American Ethnologist*, 18(4): 658–83.

Ewen, Stuart, and Ewen, Elizabeth. 1979. *Channels of Desire: Mass Images and the Shaping of American Consciousness.* New York: McGraw-Hill.

Hazan, Haim. 1990. *A Paradoxical Community: The Emergence of a Social World in an Urban Renewal Setting.* Greenwich, CT: JAI Press.

——. 2001. *Simulated Dreams: Israeli Youth and Virtual Zionism.* New York: Berghahn.

Hoben, Allan. 1982. Anthropologists and Development. *Annual Review of Anthropology*, 11: 349–75.

International Committee on Project Renewal Evaluation. 1981. *Guidelines for Evaluating Research.* Jerusalem: The Jewish Agency for Israel.

Jameson, Frederick. 1991. *Postmodernism, or, The Cultural Logic of Late Capitalism.* Durham, NC: Duke University Press.

Kimmerling, Baruch. 1992. Sociology, Ideology, and Nation-Building: The Palestinians and Their Meaning in Israeli Sociology. *American Sociological Review*, 57: 446–60.

Lakoff, Mark and Johnson, George. 1980. *Metaphors We Live By.* Chicago: Chicago University Press.

Lash, Scott. 1990. *Sociology of Postmodernism.* London: Sage.

Lorenz, Konrad. 1952 (1949). *King Solomon's Ring.* London: Methuen.

Maines, David. 1993. Narrative's Moment and Sociology's Phenomenon: Toward a Narrative Sociology. *The Sociological Quarterly*, 34: 17–38.

Munich, Richard and Smelsa, Neil (eds). 1992. *Theory of Culture.* Berkeley: University of California Press, 29–63.

Myerhoff, Barbara and Ruby, Jay (eds). 1982. Introduction, in *A Crack in the Mirror: Reflexive Perspective in Anthropology.* Philadelphia: University of Pennsylvania Press, 1–35.

Pels, Peter and Nancel, Lorraine. 1991. Introduction: Critique and the Deconstruction of the Anthropological Authority, in *Constructing Knowledge*, edited by P. Pels and L. Nancel. Sage Publications, 1–22.

Ram, Uri. 1989. Civic Discourse in Israeli Sociological Thought. *International Journal of Politics, Culture, and Society*, 3(2): 255–73.

Rosaldo, Renato. 1987. Where Objectivity Lies: The Rhetoric of Anthropology, in *The Rhetoric of the Human Sciences*, edited by J. Nelson. Madison: Wisconsin University Press, 87–109.

Schneider, David. 1979. Kinship, Community and Locality in American Culture, in *Kin and Communities*, edited by A.J. Lichtman and J.R. Challmore. Washington DC: Smithsonian Institution.

Schneider, Harry. 1988. Principles of Development: A View from Anthropology, in *Production and Autonomy: Anthropological Studies and Critiques of Development*, edited by J. Bennet and J. Bowen. Lanham, MD: University Press of America, 61–80.

Stacey, Margaret. 1969. The Myth of Community Studies. *British Journal of Sociology*, 20: 134–47.

Swidler, Ann. 2001. *Talk of Love: How Culture Matters*. Chicago, IL: University of Chicago Press.

Tyler, Stephen, A. 1986. Postmodern Ethnography: From Documents of the Occult to Occult Documents, in *Writing Culture: The Poetics and Politics of Ethnography*, edited by James Clifford and George E. Marcus. Berkeley: University of California Press

Wagner, Roy. 1986. *Symbols that Stand for Themselves*. Chicago, IL: University of Chicago Press.

Warren, Roland. 1973. *The Community in America*. Chicago, IL: Rand McNally.

Part II: Mirage

Chapter 4: Israeli Soldiers, Japanese Children: Fieldwork and the Dynamics of Participant-Observation and Reflection

Eyal Ben-Ari

During the end of the 1980s and the 1990s I concurrently worked on two main research projects: one related to a Japanese day-care centre and the other involving an Israeli infantry battalion where I served as a reservist. The opposition between these two projects is related to contrast I explore in this chapter: between a 'conventional' kind of fieldwork on 'an-other' culture and fieldwork in one's very 'home' where one is often a committed participant. Monaghan and Just (2000: 13) suggest that 'ethnography is based on the apparently simple idea that in order to understand what people are up to, it is best to observe them by interacting with them intimately and over an extended period.' Indeed, the term participantobservation encapsulates something anthropologists are well aware of (be they of more radical or conventional inclinations): fieldwork – like all social research – is a social enterprise.

The past decades in anthropology have been marked by considerable attention devoted to the problematic of fieldwork. The concerns that have been dealt with have ranged from practical 'how to' matters addressed by prescriptive approaches (Pelto and Pelto 1978), through to the newer interest in the interrelationship of self and other in the field and in texts (Rosaldo 1989). Continuing recent theorizing, I argue that understanding fieldwork implies a range of issues spanning a longer period than the one actually spent in the field (Shore 1999: 26). In the framework of this chapter I use the contrast between my two projects to examine two sets of issues: the tension inherent in the hyphenated 'participant-observation' and its implications for fieldwork; and the complex kind of self-reflection entailed by carrying out such differing projects over the same time period. Let me briefly outline the two projects in order to frame my analysis.

The first project involved a Japanese day-nursery where I had carried out fieldwork for a total of four months in 1988 and 1994. Given Japan's meteoric economic suc-

cesses between the 1960s and the 1980s, this was a period during which the country's organizations and management methods were a fashionable object of study in the social science. Consequently, I thought it would be interesting to analyse how Japanese children learn to become organizational members within the first formal organization they encounter, the preschool. My reasoning centred on the idea that the order and dynamics of Japanese organizations result in part from the peculiar forms of socialization and control found in institutions of early childhood education. On the basis of the personal network I had developed in a previous stint of research in Japan, I entered a medium-sized day-care centre to carry out intensive fieldwork. This project led to the publication of two books (and a series of articles) about the preschool (Ben-Ari 1997a; 1997b) within which I took up an analysis of the body in socialization and an examination of preschools as organizations.

The second project entailed research on an Israeli military battalion in which I served during the first Palestinian Uprising (1987–92). While my unit was deployed in Hebron and performed all of the 'usual' activities IDF units were entrusted with in the occupied territories (roadblocks, patrols, and arrests), I found myself asking questions I had never asked before. These were questions about the normalization or naturalization of Israel's military occupation and the manner by which individual soldiers make sense of their deployment in the occupied territories. I attempted to answer these questions through a piece based on my observations as a deeply troubled participant. It was published in the journal *Cultural Anthropology* (Ben-Ari 1989) and in Hebrew in a book devoted to studying the Intifada and Israeli society. But as I wrote that piece I began to be preoccupied by wider issues related not only to the Palestinian Uprising but to Israeli army service. Consequently, I carried out a study of the unit resulting in an ethnography (and another series of articles) (Ben-Ari 1998). In this volume I examined the interpretive models that soldiers use to make sense of their actions and the environment. These were the models that supplied (and to a great degree still supply) the basic frames of reference Israeli troops use to illuminate soldierly behaviour, formulated ideas about enemies and imagine ideals of military masculinity.

The two arenas where I carried out research were very dissimilar. One represents a culture divorced from my everyday life, while the other was one that I had grown up within; while the first was a world of children and women, the second was the exceptionally masculine world of male soldiers; and, if work in the former entailed a process of slow entry and gradual comprehension, the latter involved gaining a critical distance from the field. Yet with the benefit of hindsight I now understand that in terms of my interests the two projects also represent fascinating similarities: both are formal organizations with a rich underlife of informal dynamics, both involve complex processes of organizational socialization and the creation of identity, and both entail use of the body and of emotions to create cultural selves. In what follows I trace out the two projects in greater detail and then go on to explore the analytical issues I have set out.

Fieldwork in a Japanese Preschool

While I have since gone back to Japan for more research on preschools (and other subjects), here I focus on the institution, *Katsura Hoikuen* (lit. Katsura day-care centre), where I carried out my first extended research on early childhood education. The centre, catering for children of working parents (in effect mothers) was a medium sized institution with about one hundred and ten children cared for by twenty-six teachers (all women). The parents of the children were overwhelmingly middle-class (slightly less than ten per cent were welfare-related cases). A typical two-storey urban institution found throughout the country, the centre is located in Kyoto, the country's old capital.

How did I Carry out Fieldwork in Japan?

To begin with, I attended the centre as many days a week as possible, often up to five times a week. Each time, however, I usually spent about four or five hours so that I could go home to write-up my field journal. Apart from joining day-today activities (and witnessing formal and informal play sessions), I also attended teachers' meetings and outings, lunch and snacks, birthday parties and morning greeting ceremonies, and observed encounters between parents and teachers (most often when children were brought to, or taken from, the centre). In addition, I gathered a very large variety of forms, documents, and texts used in the institution as well as pictures, copies of songs and other curricular material. Rich data was also gathered by simply 'hanging around' when there were no organized activities going on since this allowed me to observe unpredictable occurrences. Along these lines I found myself listening in to casual conversations, witnessing episodes of name-calling and fighting between the children, and a whole plethora of activities that take place at 'the interstices' between more structured events (such as lining up, preparing rooms, or ad-hoc storytelling while waiting).

I also held interviews with all of the centre's teachers (including the cooks and the administrator), with a few city officials in charge of day-care centres and some parents. The interviews included background questions and inquiries about the role of caretakers, methods of teaching and learning, problems of catering, and activities at the centre. Having previously done research in Japan for over two years between 1981 and 1983, my linguistic ability was adequate and I carried out all of the interviews in Japanese. What I did find, however, was that I had to learn the children's language which included not only slang but also various adaptations of adult usages. Thus, especially at the beginning of fieldwork I would often ask teachers to 'translate' what the children were saying into standard Japanese.

The data itself – not unlike my findings in other research projects – were recorded in a journal and entered chronologically. My fieldwork journal grew to include observations, long and short parts of conversations, transcripts of interviews, and some very

elementary thoughts about analysis. I transcribed all of the interviews by myself very quickly so that I could generate questions which I fed back into the field research.

Gender, Fieldwork and Positioning

A set of attributes formed my initial persona in the field: I was a university teacher, male, outsider to the centre (not one of the staff or parents), a foreigner (nonJapanese), and possessing a rather large body size. In the Japanese context the first two attributes were important because the few men in preschools most often function as heads of institutions; university professors for their part do not only have a relatively high status in Japan but are also seen as experts on a variety of issues. Thus (my own feelings notwithstanding) from subsequent conversations with teachers it turned out that many were apprehensive because I was initially perceived as a threatening figure who had the power to criticize what was going on at *Katsura Hoikuen*. This situation was exacerbated by the fact that the deputy head of the centre introduced me not only as a university professor but as having a Ph.D. from Cambridge University.

Amplifying these dynamics was the fact that as a man, I had ventured into a world overwhelmingly populated by women and children. I understood that the day-care centre is part of a field in which female teachers are in a certain structural position controlled by older, often male individuals. More widely, gender-related dynamics between the fieldworker and host community influence adjustment to the field, his or her relationship with the study population, the type of data collected, and its subsequent interpretation. While at the time, I did not formulate it in such theoretical terms, I did sense the underlying apprehension towards me and created a role for myself – that of student – by acting and talking in a certain manner. On most days, I wore T-shirts and shorts and turned the teachers into the experts they were by asking questions and seeking their opinions.

No less important, I strove as much as possible to help in a variety of activities such as playing, overseeing, or showering the children and comforting them when they fell. In fact, after a few weeks at the centre, I read (while translating) a book about toilet training that I had brought with me from Israel (*Sir Hasirim*). The different manner by which Israelis handle this process (much like middle-classes around Western societies) elicited a number of interesting conversations with the caretakers. In these cases, participation, at one and the same time, aided me in getting accepted, feeling better, and gathering information. It turned out that another way of becoming less threatening was by telling people how our older son had attended Israeli, English and Japanese day-care centres and how our younger son was going to an Israeli one. Finally, I think that like many foreign anthropologists in Japan (Bestor et al. 2004) I was also a curiosity and fulfilled that role diligently by being 'Israeli': I explained about our foods (pita bread), folk songs and dances, and became a religious 'expert' on Judaism for a few Christian teachers.

I did not think that the teachers felt uncomfortable with me. The older teachers (then in their fifties and myself in my thirties) treated me properly while some of the younger and bolder ones flirted with me. Over the course of a few weeks, I became friend and confidant of some of the teachers and went out with a group of them for coffee and the occasional meal after work. Perhaps because I was an outsider and would not be staying around, a number of caretakers allowed themselves to talk about sensitive issues like conflict within the centre, personal disappointments, or prospects for marriage and children. It may well be that my status as a male stranger phase was shortened by my interest, respect and increasing skill in knowledge ascribed to the female teachers. I also think they got the feeling that I respected even felt awed by their efforts and by the high standard of care they gave the children. Over time, like many instances of fieldwork, I was gradually incorporated into the social order of the day-care centre. At the beginning, as I was still insecure with my Japanese language ability (not having spoken the language for four years) I waited until I established more secure and relaxed relationships which included joking. I finally felt their trust when I was invited to change diapers and asked to help put the children to sleep (Ben-Ari 1996).

Fieldwork in an Israeli Military Unit

The battalion in which I served belongs to one of the elite infantry brigades comprised completely of male reservists. Like many reserve units, the atmosphere tended to be informal with the formal aspects of rank deemphasized. All served under similar conditions: the same beds and barracks, food and canteen services, or clothes and equipment. This relative egalitarianism was all the more notable since the battalion was quite heterogeneous in terms of (Jewish) ethnic groupings (more than half of the soldiers were from Middle-Eastern backgrounds) and religious adherence (it had a very sizeable group of observant Jews). The men were occupationally diverse and included students, managers, mechanics, store owners, farmers, industrial labourers, clerks, or accountants. Socially, the battalion was characterized by relatively high cohesion, strong primary groups and a certain sense of a shared past. I myself was a staff officer, an adjutant and had been in such a bureaucratic support role for most of my army career (I served in this specific unit between 1985 and 1992).

Fieldwork

During the 1980s and early 1990s, units like our battalion were often called for periods totalling forty days. Essentially, I utilized every stint in the army (and occasionally other periods) to collect data. I told people that I was gathering material for a book about the battalion, and gave copies of the Hebrew version of my paper on the Intifada to whoever was interested. In my official capacity I attended all staff

summing-up and personnel meetings. Moreover, I often accompanied (acting as driver) the battalion's commander, his deputy, or other staff officers during visits to places where the battalion's men were training or deployed. Almost invariably, I would be invited to the ubiquitous coffee sessions (and the attendant exchanges) that characterize Israeli army life in the reserves. Other times I visited people on guard duty for chats or longer conversations. During live-fire exercises (I played a very small role) I walked around with a notebook entering my observations. After a number of people had read my piece about the *Intifada*, they would initiate discussions with me about my project or tell me jokes (I had made a habit of collecting quips related to military life). Not infrequently fellow officers made fun at how I was furthering my career by turning them into informants.

I carried out about thirty interviews with the battalion's commanders, combat and staff officers, non-commissioned officers, 'ordinary' soldiers, and logistics personnel. Interviews with soldiers from the ranks were especially important in order to get out of an overly 'officer-centric' view of things. In this endeavour I was aided by my formal capacity and by my fieldwork persona. Being in charge of personnel, part of my job can be likened to that of a social worker with whom it is legitimate to talk about grievances or problems. My persona, a bit of a 'softy' and a scholar studying something vaguely related to psychology, probably helped in getting people to open up as well. Here I repeatedly sensed that I was lending an attentive ear to people's interests rather than them answering questions arising out of my research agenda. Topics ranged from past career and personal motivation for military service, through to more intimate things like fear in combat, going AWOL, or moral stances towards the *Intifada*. Here again, I recorded the data in a chronological journal while some of the taped interviews were transcribed by research assistants.

Did the Men 'Level' with Me?

This question bears importance since I was an officer in the unit. First, openness and honesty marked my interactions simply because one cannot hide one's feelings and actions while on reserve duty because you are in the public gaze for so many weeks. Second, people like to talk and even regarding intimate topics once a modicum of trust has been built are willing to explore difficult matters. Even a short list of the issues discussed seem to indicate a high level of candour: hating senior commanders, feelings of being exploited, taking responsibility for casualties, or problems associated with civilian life (unemployment, financial predicaments or medical problems). Moreover, I think that I discerned a great deal from muted criticisms or taciturn voices as they appeared in subtle comments, ironic demonstrations or silent resistances.

Participation and Risks

In researching my army unit, participant-observation was very heavily skewed towards participation. Along these lines, in contrast to my other sites of inquiry – Japanese suburbs, Japanese day nurseries, and even the graves of Jewish saints in Israel (Bilu and Ben-Ari 1992) – I had very different stakes in the game. I had little possibility of leaving the field and probably if not a greater than at least a different kind of accountability to the researched subjects. To put this point pointedly, military service was a legal obligation and sanctions could be used against me if I refused to serve. In addition, take the hazards this research could have posed for my (potential) army career such as the possibility of being characterized as a troublemaker or raising questions about whether I was giving away unauthorized state secrets. Such a situation could have posed a danger for the continuation of my (then) army career and could have ended up in disciplinary action against me.

These hazards are akin to the kind of dangers that anthropologists who would dare to study the micro-relations of power in the anthropological community face. By this point I refer not to the vast 'literature of anguish' about fieldwork in which (predominantly American) anthropologists have undertaken confessions about (colonial, exploitive, or ethical) sins committed in the field. Rather I refer to the seriously unstudied subject of power relations within the anthropological academic community. Anthropologists are all for reflection about the 'field', but beyond hints, intimations or the occasional biographies or historical reviews they have not really faced issues attendant on the internal social processes of their community (Ben-Ari 1998b; Ben-Ari and Van Bremen 2005). The reasons for this lack are of course related to the same apprehensions related to careering that I had in writing about my military experience.

Critical Distance: Studying My Own Society

A closely related question is whether my membership in the unit is not a sign of methodological weakness? Like the role of any researcher, so my position presents strengths and weaknesses, because, of course, knowledge is always relative to the knower. In this sense, my interrogation of the army unit is important against the background of an increasing number of studies entailing 'anthropology at home'. Here, it would be worthwhile for British and American readers to remember that Israeli anthropology – like the anthropology of mainland Europe – has had a need to devise techniques, not so much for getting *into* a new culture, as for getting *out* of all too familiar surroundings. To be sure, the advantages of participation in the unit centre on being closest to the way military meanings are 'naturally' actualized and my ability to use my native understandings of soldiering as a resource. Yet the major disadvantage, it may be argued, is the lack of proper 'distance' from the unit and very basic (emotionally loaded) issues such as masculine identity, citizenship, militarism, or nationhood. Ultimately then the strength of my work was predicated on my ability to

achieve a reflexive stance: an ongoing effort not to rely not only on introspection but to record, describe, analyse and formulate my findings in a way that would allow them to be critiqued by others.

What methods did I use to achieve this distancing? One approach which has always been part of the anthropological armature has been the study of language. Hence, I realized early on that an inspection of the language used in the Israeli military may uncover the meanings attached to military service. For example, the translation of army terms into English prompted me to face their Hebrew connotations. In this respect, the IDF (like all armies) has its own rather specialized language ranging from formal jargon and acronyms through to vernacular idioms and slang. Throughout my writing I consistently encountered reminders of just how specialized this military argot is. While I am usually fully bilingual and switch effortlessly between Hebrew and English, I found myself making extensive use of military dictionaries. There were many terms that I could easily use in Hebrew but just did not know in English: Armoured Personnel Carrier (*nagmash* or informally, *Zelda*) or operational deployment (*batash*), for instance.

The next method of distancing had to do with deliberate attempts at defamiliarizing my material (Marcus and Fischer 1986). I did this by relating my material to theoretical formulations elicited in other contexts. For example, I linked my data to explanations of small group formation in the American army in Korea and Vietnam and to the Wehrmacht in the Second World War, to feminist examinations of gender identity among policemen, or to the social scientific study of embodiment and of emotions. All of these operations forced me to reflect about my data from a more detached vantage point. To be sure, I undertook similar actions in regard to my Japanese data but since I was so close to my Israeli data, the very act of theorizing forced me to make sense of what I was taking for granted. Two examples are the risk-seeking behaviours at bottom of motivation for some military missions or the mechanisms of turning actions against Palestinians civilians into 'normal' military activities.

Juxtaposed Projects

In many respects, work in one place greatly helped me to look at the material I was generating in the other context. First, the very existence of another case placed a comparative perspective on things. For example, in thinking about socialization to army life I used concepts I found in writings about Japanese workplaces; in examining the small group dynamics of military units I was led to considering the formation and maintenance of work groups in Japan. The point in these instances lays not so much in the similarity of the actual cases as that their juxtaposition constantly forced me to consider things in a new light. At times, the opposition of the two projects took on a peculiar turn: I would be thinking of how to analyse the actions of soldiers running after Palestinians and an image of Japanese children playing 'hide and seek' would emerge;

other times, I would be reading things about spiritual education in Japanese schools and would suddenly realize its relevance to the 'fighting spirit' of Israeli combat units. Thus, the juxtaposition of the two cases allowed me to see underlying similarities in their organizational activities despite their very differing aims and cultural contexts.

On a second and more concrete level, simultaneously working on two cases aided me in reciprocally relating native Japanese cultural concepts to Israeli notions of military activities. For example in a class about Japanese society, I tried explaining the Japanese cultural stress on *gambaru*, (persistence or perseverance). I gave numerous examples of this persistence like the tenacity needed in finishing a children's 'marathon' race or in the long run up to university entrance examinations. Then suddenly I hit upon something: I suggested to my Israeli students that *gambaru* is like *devekut bamesima*, a term used in the IDF which literally implies being 'cemented' or 'glued' to one's mission but connotes carrying out the mission despite all of the difficulties of a combat situation. In this way by relating one cultural term to another I gained insight into both cultures.

Readerships: Not Up, Not Down, But Across

The issue of 'outsider-ness' and 'insider-ness' raises another problem, that of readerships (Brettel 1996). In both the Japanese and Israeli cases I found myself writing for an audience that has been debated very much in contemporary anthropological thought for I found myself writing not so much down or up but across. I was not writing about the third world or underprivileged groups in my society or about social elites especially privileged groups. Rather my texts were about the middle-class to which I belong. In creating my texts about Japan, it was always clear to me that my primary readerships were in academia and included scholars interested in Japan and anthropologists or researchers concerned with early childhood education (reflected in the journals I published in). A secondary readership entailed Japanese experts or former interviewees whom I thought would take an interest in my work. I had sent copies of articles and of a previous ethnography (Ben-Ari 1991) to Japan but was met with indifference (related perhaps to the limitations of having published in English). In the case of the preschool, I also sent copies of my ethnographies to the deputy head of the daycare centre but again received no reaction except for a polite thank you upon receiving the books.

In regard to what I imagined to be my primary academic readership, the themes I took up followed two sets of considerations. In the first, I attempted to develop, through the case of the preschool, contemporary theorizing about the body in socialization. In the second (perhaps because I was an army bureaucrat), I developed my interest in organizational life. Accordingly, I wrote about the daycare centre as an organization governed by a formal division of labour, complex dynamics, patterns of coordination and control, and routines and regulations. I attempted to argue that it

may prove illuminating to bring to the foreground – to look 'at' rather than 'through' – the organizational issues involved in caring for children. Underlying both volumes, moreover, was a critique aimed at American academic writings on early childhood education in Japan. Many of these excellent texts belonged to a genre best termed 'learning from Japan'. Accordingly, I focused my critique on the assumptions at base of these studies: for instance, the purported binary opposition between the Japanese and American cases, and the importance of the wider competition between the two countries. Compared to my writings about the Israeli military, however, even this topic was not very politically loaded or emotionally loaded.

When I wrote about my military experience I found myself, in Shokeid's (1989: 26) fitting formulation, writing for a plurality of audiences but with only one text. I imagined addressing three readerships: fellow social scientists interested in the military (most prominently sociologists and social-psychologists), scholars interested in 'things military', and a vague category of 'concerned' Israelis. This mix of audiences was difficult to handle since each group, I thought, had different expectation about analysis and approach. For fellow social scientists I offered a little using perspective, an ethnographic one, using recent theoretical concepts from cognitive anthropology. Moreover, because much of military sociology and social-psychology are dominated by quantitatively oriented researchers, I added methodological appendices to my ethnography (and was roundly criticized by one anthropological reader for being too apologetic). For scholars and generalists interested in 'things military', I extended an in-depth study of one of the prime examples of military units: a combat battalion. Furthermore, in contrast to most texts about the military, which are written from the point of view of senior commanders, I brought in the voices of ordinary soldiers and junior officers. Finally, for concerned Israelis I was advancing an analysis of a central institution in our society and which has figured prominently in the lives of many of its Jewish citizens. In this respect, my ethnography could be read as a comment on the basic categories of thought in contemporary Israel. Indeed, this was a period when scholars in the humanities and social sciences were preoccupied with deconstructing Israeli militarism. At the same time, however, I now understand that publishing in English, like Bakalaki's case (1997: 513), made my work inaccessible to many of the people I studied but accessible to Israeli intellectuals and international academic scholars. And, to add the obvious, this chapter is paradoxically written in the English language as well.

Cultural Critiques and Positioning

Writing for concerned Israelis also forced me to be more aware of my social position and the 'movement' towards inquiry. Here the problem is related not only to the fact that I was studying my own 'home'. Gupta and Ferguson (1997: 17) state that scholars studying their own society (they talk about minorities, postcolonials and 'halfies'), may be regarded with suspicion, as people who lack the distance necessary to do good

fieldwork or as being thrust into speaking for their identity and thus being forced into the prison-house of essentialism. My main problem was political: I was writing about and within a violently divided society (Smyth and Robinson 2001). What did this problem entail?

First, I encountered difficulties in publishing work about the military within anthropological circles because the study of the armed forces by anthropologists is a politically fraught area. To be sure, while any topic within anthropology is subject to a host of connotations and pre-conceptions, the topic of the military is one where these ideas may be particularly strong. I would cautiously suggest that because the American military is viewed in terms of the Vietnam debacle it is often understood as somehow morally tainted and therefore not 'worthy' of anthropological research. Perhaps no less importantly, it is now also viewed in terms of what can be called 'mass-spectator militarism' – a fascination with 'things military' as depicted in the electronic media (the coalition forces in the Gulf War, for example). Finally, the disciplinary stress on the underdog, the exploited has made anthropologists shy away from studying perpetrators and especially the military (Ben-Ari 2004). I found that when I wrote about the Israeli military, I had to justify my positioning in ways I never encountered in regard to my Japanese texts.

Second, given that I was a member of the unit studied, it became much clearer how my very identity as a Jewish Israeli man was embodied and continuously reinforced through the dynamics of the battalion. While creating my text, I began to formulate thoughts I had only vaguely comprehended before: how the complex of soldier-man-citizen as a core of Israeli-ness has been internalized throughout the lives of many individuals like me and how this internalization underlies the normalization of war and of military service in our country. In this respect, writing as an 'insider' in the unit, an anthropologist, a scholar dealing with military matters, and concerned Israeli is not a sign of weakness. It is expressly the juxtaposition of these different identities in my text that allowed me to answer such questions as who I and (more collectively) who we Israeli-Jews are? And what it means to be men and soldiers? Such questions spanned (at the same time) personal, professional, and cultural domains (Narayan 1993: 682), and allowed distance and reflection.

Conclusion

The decade during which I carried out fieldwork and wrote about my fieldwork in Japan and Israel was a rather strange time. I was both an Israeli soldier and a sort of teachers' aide in a Japanese kindergarten. I felt close to both places and people but through the process of writing also found myself distanced from them. Throughout this period I was constantly reflective about the process of research and writing. But this was not only a matter of introspection, of self-reflection – what has been termed becoming an audience to oneself. Rather, I think that my experience reveals

something about the process by which one's case is 'bounced' off against another. In strict methodological terms, my experience bears some similarity to what Skocpol and Somers (1980) term the contrast-oriented approach in comparative analysis. In such classics as Reinhard Bendix's (1978) *Kings or People*, Ronald Dore's (1973) *British Factory, Japanese Factory* or Clifford Geertz's (1968) *Islam Observed* a seemingly arbitrary move between two cultures is aimed at enriching a view of both. In these treatises, scholars use the contrast-oriented method to highlight the unique features of each particular case they discuss and to show how these features affect the working out of putatively general processes.

My argument is that this kind of process takes place within each one of us anthropologists (and other scholars, of course). Each anthropologist has a range of interpretive fields comprised of the projects he or she has been involved in. While differing from the more ordered and systematic methodology used by scholars cited by Skocpol and Somers, these differing projects nevertheless constantly radiate or rebound against one another in surprising ways that contain contrasts or similarities but very often render new insights and understandings. In other words, within each anthropologist a kind of interpretative dialogue goes on that not only allows her or him to understand the peculiarity of one or the other culture of study but to create distance and reflection. In this sense, because participant-observation often entails strong emotional ties and implications, the need for distance and reflection may be stronger than in other disciplines.

Consequently, given the intense experience of fieldwork, the mutual reflection and refraction of different fields we participate in may be a fascinating area for further discussion and analysis. An understanding of the emotional involvement of anthropologists in many of their fields leads me to my final point.

Epilogue

The Al-Aqsa *Intifada* has been going on for more than six years and Israel's second Lebanon War is but part of a long, drawn-out struggle. For me, studying the Israeli military since the end of the 1980s, this has been a fascinating if at times unsettling experience. I am fully aware that my work, alone or with colleagues (LomskyFeder and Ben-Ari 2000; Bar and Ben-Ari 2005; Ben-Shalom et al. 2005), can be used by different groups to be read as either condoning the killing of Palestinians or as an anti-Israeli tract. Rather than leading my analysis in an explicitly political direction, I have purposely taken a distanced view to understand the intense experience of Israeli troops. Going over my writings, I found that at the end of my first essay on the army (Ben-Ari 1989: 386), I found that in a strange manner my two projects found a textual expression:

> During the summer I spent over two months doing fieldwork in Japan [on a preschool]. It was only at the end of this period, however, that I finally felt

that I could commit my experience to writing. I had to literally travel away from my society in order to travel to its edge, to be able to look at it from an external vantage point. Yet I am still very much part of my society.

Re-reading this piece prompted me to think about what is common to both projects, to reflect on my wider attitude towards the people I study.

Critiquing the assumption found in many of our discipline's texts that native anthropologists are somehow more privileged and caring than outsiders, Dominguez (2000: 363–4) suggests that anthropologists, as other scholars, who write about people they really care about face serious problems involving negative depictions of the peoples or societies studied. These kinds of problems are not only the preserve of 'natives' who were born, raised and active in local, regional, or national communities of which they have long been members. Indeed, she goes on to caution us not to not make the mistake of assuming only long-time 'insiders' are ever driven by love. Dominguez (2000: 366) goes on to propose a different criterion of value than identity:

> genuine love, respect, and affection, not categorical 'identity' ... the kind of love we feel for family members, tough love at times but never disengagement or hagiography. Love, yes love, – the thing most of us are not open about in our scholarly writing, the kind most of us have been socialized into excising from our scholarly writing.

I take her proposal to mean that in writing about the people we study we should be wary of condescending, romanticizing, or presenting only positive aspects while eliding conflicts or feeling so guilty about our own geopolitically defined position that we treat those we consult with kid gloves, both in the 'field' or in our published writings. Reflecting on my work, I think that I have been very much moved by this kind of love. In the Japanese case this means genuine affection for the children and teachers who shared their lives with me. In the Israeli case this means deep fondness and respect for the men who served with me. In both cases this does not mean adoration or glamorization but an ability to be critical, to depict the conflicts and struggles involved and to understand the wider context within which they lived and acted.

References

Bakalaki, Alexandra. 1997. Students, Natives and Colleagues: Encounters in Academia and in the Field. *Cultural Anthropology*, 12(4): 502–26.

Bar, Neta and Ben-Ari Eyal. 2005. Israeli Snipers in the Al-Aqsa Intifada: Killing, Humanity and Lived Experience, *Third World Quarterly*, 26(1): 133–52.

Ben-Ari, Eyal. 1991. *Changing Japanese Suburbia: A Study of Two Present-Day Localities.* London: Kegan Paul International.

———. 1989. Masks and Soldiering: The Israeli Army and the Palestinian Uprising, *Cultural Anthropology*, 4(4): 372–89.

———. 1996. From Mothering to Othering: Culture, Organization and Nap Time in a Japanese Preschool, *Ethos*, 24(1): 136–64.

———. 1997a. *A Japanese Childcare: An Interpretive Study of Culture and Organization.* London: Kegan Paul International.

———. 1997b. *Body Projects in Japanese Childcare: Culture, Organization and Emotions in a Preschool.* London: Curzon.

———. 1998a. *Mastering Soldiers: Conflict, Emotions and the Enemy in an Israeli Military Unit.* Oxford: Berghahn Books.

———. 1998b. Colonialism, Anthropology and the Politics of Professionalization: An Argumentative Afterword, in *Anthropology and Colonialism: The Japanese and Dutch Experience in East and South-East Asia*, edited by J.V. Bremen and A. Shimizu. Honolulu: University of Hawaii Press and London: Curzon Press, 384–411.

———. 2004. Review Article: The Military and Militarization in the United States, *American Ethnologist*, 31(3): 340–48.

———. and Jan van Bremen. 2005. Asian Anthropologies and Anthropologies in Asia: An Introductory Essay, in *Asian Anthropology*, edited by J.V Bremen et al. London: Routledge, 3–44.

Bendix, Reinhard. 1978. *Kings or People: Power and the Mandate to Rule.* Berkeley: University of California Press.

Ben-Shalom, Uzi., Lehrer, Zev and Ben-Ari, Eyal. 2005. Cohesion during Military Operations? A Field Study on Combat units in the Al-Aqsa Intifada, *Armed Forces and Society*, 32(1): 63–79.

Bestor, Theodore C., Patricia Steinhoff G. and Bestor, Lyon Victoria (eds). 2004. *Doing Fieldwork in Japan.* Honolulu: University of Hawai'i Press.

Bilu, Yoram and Ben-Ari, Eyal. 1992. The Making of Modern Saints: Manufactured Charisma and the Abu-Hatseiras of Israel, *American Ethnologist*, 19(4): 672– 87.

Brettel, Caroline B. 1996. Introduction: Fieldwork, Text and Audience, in *When They Read What we Write: The Politics of Ethnography*, edited by C.B. Brettel. Westport, CT: Bergin and Garvey, 1–24.

Dominguez, Virginia. 2000. For a Politics of Love and Rescue, *Cultural Anthropology*, 15(3): 361–93.

Dore, Ronald P. 1973. *British Factory – Japanese Factory: The Origins of National Diversity in Industrial Relations.* London: Allen and Unwin.

Finlay, Andrew. 2001. Reflexivity and the Dilemmas of Identification: An Ethnographic Encounter in Northern Ireland, in *Researching Violently Divided Societies: Ethical and Methodological Issues*, edited by M. Smyth and G. Robinson. Tokyo: United Nations University Press, 55–76.

Geertz, Clifford. 1968. *Islam Observed: Religious Development in Morocco and Indonesia.* New Haven, CT: Yale University Press.

Gupta, Akhil and Ferguson, James. 1997. Discipline and Practice: "The Field" as a Site, Method and Location in Anthropology, in *Anthropological Locations: Boundaries and Grounds of a Field Science*, edited by A. Gupta and J.
Ferguson. Berkeley: University of California Press, 1–46.

Lomsky-Feder, Edna and Ben-Ari, Eyal. 2000. *The Military and Militarism in Israeli Society*. Albany: State University of New York Press.

Marcus, George E. and Fischer, Michael M.J. 1986. *Anthropology as Cultural Critique: An Experimental Moment in the Human Sciences*. Chicago, IL: Chicago University Press.

Monaghan, John and Just, Peter. 2000. *Social and Cultural Anthropology: A Very Short Introduction*. Oxford: Oxford University Press.

Narayan, Kirin. 1993. How Native is a "Native" Anthropologist? in *Situated Lives: Gender and Culture in Everyday Life*, edited by L. Lamphere et al. New York: Routledge, 23–41.

Pelto, Pertti J. and Pelto, Gretel H. 1978. *Anthropological Research: The Structure of Inquiry*. Cambridge: Cambridge University Press.

Rosaldo, Renato. 1989. *Culture and Truth: The Remaking of Social Analysis*. Boston, MA: Beacon Press.

Shokeid, Moshe. 1988. *Children of Circumstances: Israeli Emigrants in New York*. Ithaca, NY: Cornell University Press.

Shore, Cris. 1999. Fictions of Fieldwork: Depicting the "Self" in Ethnographic Writing (Italy), in *Being There: Fieldwork in Anthropology*, edited by C.W. Watson. London: Pluto Press, 25–48.

Skocpol, Theda and Somers, Margaret. 1980. The Uses of Comparative History in Macrosocial Inquiry. *Comparative Studies in Society and History*, 22(2): 174–97.

Smyth, Marie and Robinson, Gillian (eds). 2001. *Researching Violently Divided Societies: Ethical and Methodological Issues*. Tokyo: United Nations University Press.

Chapter 5: Privileged and Volatile Nomadism

Shifra Kisch

Anthropological journeys are often journeys that have started long before one has become a conscious student of social phenomena. Moreover, continuous movement between diverse social settings and realities is not reserved to anthropologists, nor are the multiplicity of visions such movement can provide. It is this ceaseless human movement and exertion – which redefine, resist and co-produce the endless versions of reality – that defines the landscape of the anthropological journey. No single location offers those who travel, inhabit and shape it an encompassing survey of this vast landscape and its shifting grounds. Text attempting to make the processes that form these landscapes mutually intelligible cannot be written from a vantage point overlooking them but rather require a dialogical and dynamic stance moving between various visions, languages, discourses, emic and etic.

In what follows I wish to reflect on two aspects of my own journey. First, the gripping and at times unsettling nature of the conflicting versions of reality that different social settings offer us. And second, the similar unease that our own displaced presence may confront our hosts with, and the ways those who bear with us accommodate our intrusion, mend and manoeuvre to make us intelligible and pave those routes we can travel.

I was initially confronted with the multiple versions of reality through language. Dutch and Hebrew simply did not constitute the same reality; as a child I experienced this as a betrayal of language. Formerly language presented itself as a means to order the world rather than reveal its discrepancies. Gradually we learn that the same world we thought to be so tangible and real is real in many different ways simultaneously.

> My young Israeli classmates had repeatedly corrected me for labelling light blue, 'blue'. Even 'a kind of blue' was, for them, an unacceptable label. In Hebrew light blue is granted a separate distinct name 'Tkhelet'. My labelling was considered to be an error, a mistake in recognition or classification. I was learning a new language, trying to figure out what seemed to be already known to others. As a young migrant I was puzzled with many things in my new surrounding and language. But, I was confident about what I knew about colours, confident and stubborn.

By adding some white paint to blue or green, I had attempted to demonstrate to my classmates how light blue was just the same as light green – to which they did not assign a different name – it was just a kind of green. They were unimpressed and remained completely unconvinced to apply this logic to 'light blue'. Several days later, it hit me! I added some white to red paint and the result was an entirely different colour. For me pink was not a kind of red, I considered it a reputable autonomous colour. Not only did language order reality inconsistently, different languages constitute different realities.

Scholars have long recognized that while the fabrics of reality are fluid and unclassified, different languages order and classify the world in different ways. Rather than the workings of language, I wish to allude here to the revealing of such discrepancies and the possible unsettling nature of such revelation.

I recall that as a child-migrant I initially found this notion rather unsettling. For some time I believed comfort was to be found in undertaking the mission of familiarizing myself with all these versions my reality seemed to contain. I soon learned such horizons were unattainable. But by then I was mesmerized by the rewards of the mere attempt, keen to learn from those who seemed to be nourished by visions alternative to mine.

Those glimpses I believe I gained were inspiring and intriguing, and often still unsettling. However, I have since extended being unsettled to be my vocation. Through anthropology I learned to find the consolation in 'not to be at home in one's home.'[1] For, as Edward Said reads this expression 'there is merit in the practice of noting the discrepancies between various concepts and ideas and what they actually produce. We take home and language for granted; they become nature, and their underlying assumptions recede into dogma and orthodoxy.' (Said 2000: 185).

Thus, it is not merely additional versions we set out to familiarize ourselves with. The versions we were socialized into, often implicitly subjugate, erase or dismiss other versions. It is not simply a Rashomon[2] effect of multiplicity that needs to be revealed, by means of moving between versions we become increasingly aware of the impacts they have.

Critical Anthropological movement between different versions of reality allows us to detect sites of friction between them and explore both their oppressive effect and the creative ways by which different social actors manoeuvre in and between them. The various repertoires of such manoeuvres are not only the possible objects of our

---

[1] 'It is part of morality not to be at home in one's home'. In his *Minima Moralia* 'Adorno saw all life as pressed into ready-made forms, prefabricated "homes"' (Said 2002: 184).

[2] The concept of a Rashomon is derived from Akira Kurosawa's film 'Rashomon' (based on a short story by Akutagawa) unfolding the irreconcilable accounts of the same murder incident. While a Rashomon can be understood to be the effect of individual perception, subjectivity and recollection I here consider the various collective versions of reality constituting the social Rashomon.

study but are also its product. These discrepancies should not and cannot always be reconciled or pacified, but their investigation assumes they can be made mutually intelligible.

The most common images of anthropological journeys and transgressions refer to anthropologists' geographical displacements. Yet, it is often the adjacent 'neighbours' points of view' rather than a distant archaic 'native point of view', than can reveal the paradoxes of social life. Rather than the temptation of the distant or exotic, my own anthropological movement became increasingly informed by the striking and at times troubling proximity of mutually blind, oblivious or even hostile social worlds.

During my first fieldwork session residing with a Bedouin family in one of the (formally unrecognized) Bedouin settlements in the Negev, I joined my hosts to a trip to Beersheba, the district capital of the Negev and the location of the school where I received my high school education. I still have several relatives and friends there and have regularly visited the town since I had moved away many years ago. The city had grown and changed since, but not beyond recognition. However, that day and in the period that followed, the familiar city where I grew up, revealed an unfamiliar intensity, leaving an awkward space for my past experiences and the memories it itself evoked.

> We left early in the morning of a hot spring day, slowly making our way along the dusty dirt roads, trailing clouds of thinly grinded dust. After reaching the nearest highway we reached the outskirts of Beersheba within twenty minutes. I was cramped into the car driven by the head of my host family Abu-Bassam. Next to him sat his eldest son, and in the back two of his wives, two children and myself. First we entered the industrial zone dropping off his son, working in a local garage.
>
> Then, I joined my host and his first wife Umm-Bassam to one of the many government offices spread in town, to try and resolve a bureaucratic matter. His second wife and the two children remained in the car. Many people were waiting outside for the offices to open. Abu-Bassam waited in line with several other Bedouin men. His wife and I moved away upon recognizing the men and found shelter from the sun under some thin bushes at the corner of the building where several other Bedouin women were squatting. The offices opened but the queue moved slowly, as time went by Abu-Bassam decided to leave Umm-Bassam there, and return later. I was curiously following the women's greetings and conversation, when Abu-Bassam signalled me to come over, Umm-Bassam was urging me to hurry and answer her husband's call. Heads turned as Abu-Bassam and I conversed in Arabic, their eyes conveyed observing 'things out of order'.
>
> Abu-Bassam instructed me to give the queue number to Umm-Bassam, and come along. As he moved away I realized I was not expected to ask any questions. I was to return and follow him back to the car, where Na'ima,

his second wife had been waiting. In a hurry to settle other matters, Abu-Bassam dropped us, Na'ima the children and me, off at a clinic in another part of town, I was to translate. Several hours later I was sitting with Na'ima outside waiting for Abu-Bassam to pick us up. It was getting late. We were getting tired and somewhat hungry. Across the road I recognized a familiar row of shops, I used to pass there on my way to school and remembered there used to be an ice-cream stall in the roofed passage between the little shops. I suggested to Na'ima we waited in the shade of the passage. It was cooler and we could easily observe the entrance to the clinic, and the place we were squatted at, but she was reluctant. I told her that there used to be several tables outside, where we could sit and maybe have an ice-cream. She laughed, we should not sit there, it was improper. We waited another hour and a half before Abu and Umm-Bassam arrived and we headed home.

Before leaving town we stopped at a gas station. Abu-Bassam tanked the car, and parked it aside. He was gone for some time when he returned with a bottle of Cola and several packed falafel breads from the road eatery at the station. UmmBassam preferred water, I offered to fetch it, but I was not to enter this eatery; there were too many men there, I was explained.

The previously familiar urban space transformed and revealed an alternative topography, one that I have been passing unnoticed for many years. I did not think I was. As with my previous mention of language, we might be aware of the bearers of such topographies,[3] and even consider them part of our worlds, but remain exclusively unaware of their alternative experiences and the insights they may offer us.

Though Arab and Jewish Israeli residence and schooling are mostly segregated, the Bedouin living in the Beersheba district have not been transparent to me, from a young age we partially shared various spaces. As a child we visited a somewhat exceptional little clinic attended by many Bedouin patients. This was the clinic of Doctor Ben-Assa, the paediatrician wife of the first 'doctor of the Bedouin' in Beersheba. It was for me, as an immigrant child, a fascinating combination of the unfamiliar and the familiar, as this kind elderly Dutch-speaking paediatrician spoke to me in my mother-tongue. I had also often joined my mother to the local market to buy fruit and vegetables. But, as most middle-class customers, we had never entered the many stalls selling clothes or staple food, which I later visited with various Bedouin women. I didn't know you could buy jute sacks with 50 kilos of sugar, wheat flour or rice, nor could I imagine that I would regularly purchase such sacks myself as holiday gifts. Similarly, I had often crossed the little plaza adjacent to the government offices I visited that morning with Abu and Umm-Bassam. However I had never squatted under the thin bushes

---

[3] Inspired by Simmel's (1903) *The Metropolis and Mental Life* such topographies have often been referred to as Mental Maps comprised of various images and conceptions of spatial relationships.

decorating it, nor had I noticed their sparse shade before. I had often tanked at the gas station at the outskirts of town. I had never eaten at the attached eatery, but I had also never considered it to be out of bounds.

Before leaving town that day I also realized I should have brought along my laptop that had run out of battery, the village was not connected to electricity. I had never mapped the city for all the different public places where one could plug-in to accessible electricity sockets to recharge portable appliances. I had later elaborated a map in my mind which I then often used to plan the stations of my excursions into town.

The social realities we move between are neither distant nor near, but overlapping and entangled, and yet at times irreconcilable. Let me return to another visit to further complicate this web of realities.

> Several months later I travelled into Beersheba with two young Bedouin women, a young university student and her elder sister whom I escorted (this time with my own car) to the same clinic I had visited with Nai'ma. After the medical test was completed we waited for the young student who had gone to the university library and wished to drive home with us. Unaccompanied by men and equipped by mobile phones the student suggested we wait for her at the little shaded passage. There we settled enjoying an ice-cream. From there I observed the corner of the street where I had previously squatted with Nai'ma and the two kids. I could imagine her gaze directed at me, now with the two younger Bedouin women, and many years ago as a high school girl.

It is important to remain aware of the heterogeneity such marginal social realities encompass. Not all Bedouin women share the same gendered topography of the city. However, the young Bedouin student too was fully aware of Na'ima's gaze, also in her absence. It framed her presence and choices. Unlike me this gaze has always been part of her life; she takes it into account and negotiates it. The dominant Jewish majority is mostly unaware of this heterogeneity, and therefore frequently observes a young Bedouin woman settled at such places as 'things out of order', which makes the Bedouin student even more aware of her location within different social topographies.

Thus, it is not the often referred to 'double vision', ascribed to anthropologists or other 'professional strangers'[4] I wish to allude to. Such an assumed epistemic privilege is no longer reserved to various alleged marginal types, be them anthropologists, or others. The majority of mankind are nowadays engaged in and confronted with multiple lifeworlds. Many of the young Bedouin women and men studying in the Beersheba University engage in a social world unfamiliar and unimaginable to many of their kin. Like Abu-Bassam's son, many young Bedouin workers, (some of whom work and reside in Israel's largest urban centre, only to return on weekends) manoeuvre within such multiplicities. At another occasion, when accompanying a young Bedouin woman

---

[4] A term coined by Agar (1996).

on a visit to her sister at the maternity ward, I ran into an old classmate of mine, now a doctor at the district hospital. He actually grew up in one of the city's rather impoverished neighbourhoods; certainly his hospital reality too was a different one. His realities too were not merely different but also inherently multiple, and possibly conflicting. One does not need to migrate, belong to a minority group or down classes to embody this multiplicity and occupy distinct social positions in diverse networks and settings. Inhabiting multiple lifeworlds implies living in various social worlds in which we are differently situated, valued, and in which we often also embody a different social persona: different style of dress, languages and body languages.

While the multiplicity of lifeworlds, as feminist scholars and others have argued, may be experienced as a liberating factor, it still remains that many people who inhabit radically different or irreconcilable lifeworlds do not choose to do so. Less so do migrants, workers, members of ethnic minorities or refugees choose the often invisible social worlds they come to inhabit – social worlds that are in many ways products of exclusion and marginality. Labour migrants often wish to inhabit social worlds that subsequently prove to be practically inaccessible to them. Consequently new social worlds evolve on the margins of the desired ones. They might also assume their migration interim and rely on what they consider home as an enduring base. Emanuel Marx, in his studies on Bedouin migrant workers from the Sinai, has revealed how illusive such notions might be. Workers employed in an instable labour market often seek security in the assumption that they could always return to their pastoral or rural homes, while in fact these are becoming increasingly impoverished or are no longer sustainable and would be incapable of absorbing them back (e.g. Marx 1987a). Thus labour migrants as political refugees and exiles often long for the social worlds they used to inhabit, but their movement might imply these can no longer be recovered.

Ironically, it is often precisely such marginal social worlds that anthropologists choose to explore. Furthermore, anthropologists do not, as a result of their expedition to what often remains an alternative social world, break with their existing social bonds – on the contrary; such expeditions may promote the further establishment of the anthropologist's position, at least in the academic context.

The anthropological movement can thus be understood as a privileged choice to inhabit those marginal social realities that surround us and may otherwise remain invisible, intertwined in our lives in obscure ways. Their invisibility is not simply because we were socialized elsewhere, they are often made invisible by the relationship they have with the one we are familiar with. The undocumented migrants that build our cities, the seasonal workers that harvest the fruit we consume, the agencies that design our social space, next-door worlds behind bars, worlds concealed by professional authority and discourses, or the worlds that silently await us (as in Hazan studies of old age homes, e.g. 1994). Many of these constitute the back stages of our social worlds. Invisible, but as consequential for us as those we already know.

Anthropologists ground their journey in tangible realities: complex compositions of others, their homes and languages. The anthropologist is said to leave home to travel

to the (almost fetish) anthropological 'field'. These 'fields' are not *terra nullius,* nor unexplored terrains; the 'field' is by definition someone else's home.

We thus migrate between social realities constituted by others, and inhabit space we did not carve out ourselves. As we (awkwardly) move about within realities we are studying, those who bear with us mend and patch-up to mark the space we can occupy and the avenues we can explore. The anthropologist's transgressions both reveal discrepancies and require their accommodation. Studying the social (or a previously unfamiliar social world) does not merely require presence and observation; it can only be realized by means of interaction and dialogue.

Anthropologists often confront their hosts with similar if not greater unease than their own. Anthropologists' often individual intrusions cannot as easily be framed as other social encounters, and are often justifiably met with suspicion. Most intruders can be more easily framed within a social structure such as the foreign bride or groom, the migrant, the merchant, the tourist, the expert etc. In such cases the individual strangers are commonly seen as part of a structural phenomenon and are consequently framed within existing social roles and boundaries.

This does not mean that the presence of an anthropologist is not bound to be interpreted in the context of existing ethnic, class, gendered or other power relations. (It is precisely this inevitable context that requires the author of anthropological texts to reflect on and account for these relations.) However, it is also this abundant context that alludes to the anthropologist's misplacement. It is the voluntary displacement of an individual, yet unfamiliar with the social reality he invades that creates the setting for the labour of patch making and accommodation required from his hosts. The anthropologist is not always aware of this labour, though some of the discrepancies are obvious.

When I first appeared, as a young unmarried Jewish Israeli woman, in the (formally) unrecognized Arab-Bedouin settlement where I aspired to conduct fieldwork, I could easily identify some of the challenges and discrepancies presented to my hosts by my presence and by my hope to gain their trust and collaboration. However, I was not always aware of my hosts' more subtle efforts to manage these challenges.

Physically Bedouin settlements in the Negev are no more isolated than some Jewish towns are from one another. However in social terms, the Bedouin settlements constitute a different social sphere. Most Jewish inhabitants of the Negev never ventured into Bedouin towns and villages. This is even more so the case with regard to those Bedouin settlements which are formally unrecognized by the state, and thus lack basic infrastructure (i.e. roads, running water, electricity, sewage and waste disposal).[5] The

---

[5] In 1965 a law regulating the status of municipalities and regional councils throughout the country left most Bedouin settlements in a void of illegality with no municipal affiliation. The majority of Negev Bedouin were thus overnight turned into unlawful tenant in their own homes. Roughly half of the current Negev Bedouin population resides in townships established by the state during the 70s and 80s. The remaining half, unwilling to relinquish their landownership claims, inhabits settlements still unrecognized by the state.

few Jewish-Israeli civilians that would travel the dirt roads leading to such settlements are commonly staff members of the scarce schools and clinics, and thus travel these roads on set routes and hours of the day. (I once nearly had a car accident after which the driver of the car behind me had insisted that, although he saw the car signalling, he could not imagine the blonde driver he saw from behind would actually take the turn off the main road leading to one such unrecognized village.)

Most of these settlements are kin based and thus even unfamiliar Bedouin visitors are easily noticed. Moreover, when unexpected Jewish-Israelis do enter these settlements, it is too often in the function of inspectors and other agents of state institutions such as those announcing and enforcing the demolition of houses. (Though such intruders are rarely women and would rarely come unaccompanied.)

Manifest power relations thus frame almost any interaction between Arab and Jewish Israelis. I thus needed to carefully consider how to explain my presence and intensions, to negotiate and explore the range of conduct available for me.

I gradually became aware of my hosts' own attempts to surmount some of these challenges. For instance, only much later did I learn that several members of the community assumed I was not actually Israeli. It seemed some of my hosts accentuated my foreign (Dutch) origin, and for many I became the Dutch woman.[6] For others who were fully aware of the fact that, though I was not Israeli born and not an Israeli citizen, I grew up in Israel, accentuating my Dutchness was among others a way to reduce the indeed charged context of Arab–Jewish Israeli relationship.

Upon moving into one of the domestic household in the community, gender classifications also increasingly posed challenges – to me and to my hosts – which required negotiations and redefinitions.

While in many ways Bedouin women and men constitute separate social worlds, it would be wrong to assume that within a gender-segregated society, gender classifications can be reduced to simply dichotomies. One of Emanuel Marx's early and innovative articles examined the dynamics that characterize interpersonal gender relations throughout the life cycle among the Negev Bedouin (Marx 1987b). He carefully outlined the multiplicity of positions held by Bedouin women and men as daughters or sons, wives or husbands, and mothers or fathers. Hence, gender classifications are dynamic and negotiable.

However, despite the existence of such multiple gender classifications, I didn't quite belong to any single one of them. Prima facie, the longer I stayed, I was increasingly restricted to gender segregation. Yet, circumstances revealed possible alternatives and even inversions. Here too many of the adjustments to such discrepancies were the products of my hosts' creativity.

---

[6] 'Dutch' also became my sign-name. In sign languages, sign-names are used mainly in reference rather than address. The study I was conducting was related to the high incidence of deafness in this community and I had established a close relationship with many of the deaf people and their families, I subsequently learned they would often refer to me as 'the Dutch woman with the car', 'the Dutch woman who sleeps here' or simply Dutch.

Although it was known that I occasionally smoked, I initially refrained from smoking when on fieldwork. The head of my host family himself was a smoker but forbade his son to smoke. Very few women my age did ever smoke (and never publicly or in the presence of men), only a few of the older women regularly smoked. The head of the family, who would offer me an occasional cigarette before I became resident at his first wife's household, noticed my abstention and appeared pleased at this. He stopped offering me a cigarette when he lit up one for himself, and he only seldom strayed from this custom.

One evening, he sent his youngest wife to call for me. When I arrived, he instructed her to bring me a chair and brew us some tea. He wanted me to go through a pile of papers and find the invitation for his youngest wife's appointment for an ultrasound examination. His wife had asked me to join her for the check-up the next morning and we needed to coordinate some other matters.

The very fact that he commanded his youngest wife to serve us already framed the present situation. The topic of discussion revolved around my 'manly' resources: a car and driver's license. But when his wife left to prepare the tea just the two of us were left in the unlit courtyard. In order to indicate beyond doubt that this was a 'man to man' talk he offered me a cigarette.

The cigarette signified a social interaction between men. On this occasion it was also used to define the relationship between us. In these terms, the cigarette or rather the very offer of the cigarette was used to intercede in a situation that could be perceived as too intimate. At the same time, one could not ignore the connotations of smoking together, whose social significance was intimacy and collaboration, but the kind that was more typical between men. Such occasions had reoccurred several times and gradually I learned to read these subtle hints and markers. Eventually such codes were less required as the nature of our relationship and my presence in his first wife's household and the neighbouring encampments became more obvious and accustomed.

Before I had actually moved into the domestic household, my presence (or my smoking for that matter) was less misplaced. I was an outsider. I was a guest. It required less accommodation, redefinition and reclassification. I was initially introduced by a local schoolteacher who then introduced me to his relatives and other male heads of households. I was hosted in the *Shiq* (the formal male hosting space): prototype formal guests were men, often not allowed in the domestic space. At those visits I also met fewer women and more often spoke Hebrew given that most Bedouin men enrolled in the Israeli labour market are fluent in Hebrew. Moreover, it was not unusual for them to interact with unacquainted women. Of course, Bedouin women also pay mutual visits, but most commonly to familiar households. Only on special events or in the

absence of men are women hosted in the formal hosting space. Most visiting women would enter straight into the domestic space joining the women of the household in the cooking shed, kitchen or courtyard.

When I started residing in the village and moved into Umm-Bassam's household I became increasingly more subject to gender segregation. This should not be understood merely in term of a constraint. While my position and interaction among the women required similar guidance, negotiation and accommodation, it was of a different nature. Women's accommodations were more calculated towards making me more acceptable among women as well as among men, while most men considered my position among women as less consequential.

From the women I gradually learned the wisdom of selectively avoiding the company of men, which reduced dependence and subordination, creating a social space which was less subject to men's authority or control. To a certain extent, also gender mixed company could be understood as the impact of different versions of reality. Subsequently, I gradually avoided travelling into town with other men, as in the first excursion mentioned earlier. Guided by several women, who declared my car out of bounds for men, I travelled mainly accompanied by other women. This allowed more autonomy in planning the trips, spared us the endless waiting in cars and street corners, and conveniently allowed the visiting of female relatives on our way back. Consequently, my network extended mostly through women. I was seldom introduced again to a new encampment or household through men; I was however introduced through women also to their brothers, spouses and fathers.

When embarking on my first fieldwork session I did not consider gender questions central to my study. However, in this new social setting, gender presented one of the primary challenges to my social and personal dispositions. Moreover, the social worlds of Bedouin men and women increasingly appeared to be overlapping and at times irreconcilable social realities themselves. Consequently gender also became more central to my understanding of various social dynamics.

I do not wish to suggest that anthropologists are merely subject to the accommodations and manoeuvres of their hosts. The self-presentation and positioning of anthropologists in their research field has been long understood as a mutual process. However, it is still often in terms of acceptance or rejection, collaboration or abstention, silences and manipulations by informants that the role of our hosts has been incorporated. Much less attention has been dedicated to the diverse accommodations required and offered by those hosts to make the anthropologists' often misplaced presence intelligible and possible. Yet it is this dialogical aspect of the creative manoeuvring within converging and diverging social realities that inform the serendipitous and volatile nature of most anthropological journeys.

References

Agar, Michael H. 1996. *The Professional Stranger: An Informal Introduction to Ethnography*. San Diego, CA: Academic Press.

Hazan, Haim. 1994. *Old Age: Constructions and Deconstructions*. Cambridge: Cambridge University Press.

Marx, Emanuel. 1987a. Labour migrants with a secure base: Bedouin of the Sinai, in *Migrants, Workers and the Social Order* (ASA Monographs, 26), edited by J. Eades. London: Tavistock, 148–64.

——. 1987b. Relations between Spouses among the Negev Bedouin. *Ethnos*, 52(I–II): 156–79

Said, Edward. 2000. Reflections on Exile, in *Reflections on Exile and Other Essays*. Cambridge, MA: Harvard University Press.

Simmel, Georg. 1950 (1903). The Metropolis and Mental Life in *The Sociology of Georg Simmel*, edited by K. Wolff. New York: Free Press, 409–24.

Chapter 6: Becoming a Triple Stranger: Autoethnography of a Kibbutznik's Long Journey to Discoveries of Researchers' Faults

Reuven Shapira

Introduction

I have recently concluded that my desire to solve the basic problems of my own society, kibbutz, led me to engage in a 'long effort applied to oneself which [has converted] ... one's whole view of ... the social world' (Bourdieu 1990: 16), and this has exposed the true extent of kibbutz stratification and how this stratification was missed by all previous students (Shapira 2005). However, if the kibbutz is a venture in utopia (Buber 1958[1947]; Spiro 1955), it is plausible that its utopist intentions have been a major reason for my long and winding route to exposing students' blindness to its social reality. An individual has many desires, and efforts to satisfy them depend on a variety of factors, while their relative importance and their effect on life choices change with age, experience, etc. Thus, if my abovementioned desire explains the long effort that converted my entire view of kibbutz, a major question is how this desire was born and how it retained its primacy for so long despite the stubbornness of kibbutz conservative social realities which frustrated it repeatedly. Considered by itself, my desire could barely explain the many choices I have made, while facing various constraints and opportunities which were decisive in shaping my career, leading to the exposure of a social reality which hundreds of students had failed to discern.

Although other questions of ethnography at home are relevant here (e.g., Peirano 1998), I cannot explore them for reasons of space. Using Bourdieu's concepts, I investigate how my habitus, the capitals I have accumulated and the social fields in which I acted, caused a departure from both conventional managerial and academic careers, and led me to persevere with efforts at innovative problem-solving and in the search for better understanding of social reality in order to find new solutions, leading to major discoveries. I find that my unconventional career is explained by an early habitus of academic prowess and technical innovativeness, and by accumulation of cultural

capital as an activist and manager which enhanced my ambitions to solve major problems of my society, ultimately leading to combining careers. Multiple careers taught me lessons which conventional ones do not teach, alerted me to unstudied problems, and encouraged their study. This enabled me to find answers that other students had missed. However, my innovativeness estranged conservative leaders and my findings antagonized the kibbutz scientific coalition (e.g., Collins 1975: ch. 9), while other academics did not like my leaning towards applied research, making me a triple stranger whose work could have been helpful to many audiences had it been published, read and used, but which was not. Moreover, estrangement caused career mistakes which prolonged the route to discoveries by minimizing feedback from colleagues, which could have helped my thinking, publishing and gaining recognition. Combining home anthropology and efforts at reform of my own society proved scientifically fruitful, but also very problematic in career terms, social terms and scientific impact of findings.

Early Habitus of Academic Prowess and Technical Innovativeness

Looking back at my kibbutz childhood (1940s) and youth (1950s), I can discern major ambitions which habituated preference for gaining competence and seeking innovation. One was academic ability: I was almost always the best student in my class, and was very disappointed even with a mark of 97 on an exam because I was used to getting 100%. Another area of great interest was machines: From the age of four, I designed and built tiny tractors, trucks, combines and other agricultural machinery made of punched metal parts, shafts, wheels and screws of an Englishmade Meccano toy. Later, as a teenager, I was fond of operating agricultural machinery, and while in the army, I liked tank driving. However, in high-school, my main interest was in Marxist literature, in accord with the leftist ideology of Kibbutz Artzi Federation (hereafter KA; Shapira 2008) to which my kibbutz, Gan Shmuel, belonged. As a promising ideologist in an ideological culture, soon after finishing army service, I was chosen as youth instructor of the KA in Haifa, and was involved in political campaigns of KA's party, Mapam, there.

After two years, I returned to Gan Shmuel and continued my activism, both on the kibbutz and in the adjacent town, in addition to participating in kibbutz committees. I was chosen as delegate to KA conventions, as editor of the local newsletter and as head of the political-ideological committee. While this encouraged my activism, it soon brought chagrin as I confronted conservatism and problematic democracy in many kibbutz sectors, including the plant where I worked and became a department manager. My activism was encouraged due to a social vacuum: Most members chose inactivity in the wake of KA barren leftist politics of USSR admiration even after Khrushchev's exposure of Stalin's dictatorship horrors and the brutal suppression of Hungarian democ-

racy in 1956. Leftism bolstered the power and camouflaged the conservatism of KA oligarchic old-guard, which dominated by curtailing democracy, promoting and privileging loyalists, and suppressing critics and innovators, causing their sidetracking and exiting (ibid: chs 10–11), in accord with Michels's (1959[1915]) 'Iron Law of Oligarchy' and Hirschman's *Exit, Voice and Loyalty* (1970). I did not understand it at the time, but discerned that Marxist answers to kibbutz problems were futile, and I asked the Education Committee to study sociology at university, hoping it would help me find acceptable solutions.

Unconventional Choice of Sociology and Utopian Hopes

This choice frustrated my parents and workmates in the plant who hoped I would study engineering in accord with the plant's needs and my success as a department manager. They tried to change my mind, and sent me to a five-month intensive industrial management course at Ruppin College. I enjoyed it but insisted on sociology which was deemed 'non-functional', and was barely legitimate in this era on kibbutzim which usually financed only 'functional' studies like this course or other applied studies. 'Non-functional' studies were allowed and financed only for a few high-status veteran KA functionaries, disabled members, etc. In order to lessen animosity and receive consent, I continued managing the department and studied largely during the evenings.

This prevented excelling at university as I had in high-school, but that did not bother me as I did not aim at an academic career, but only at better understanding of my society in order to solve its problems. I missed major facts that would have thwarted my hopes, facts which were ignored by kibbutz literature: The kibbutz field, with some 150,000 participants, was dominated by conservative, veteran oligarchs who headed large federative organizations (hereafter FOs) like KA leaders (Shapira 2008: chs 2–8). A kibbutz was usually dominated behind the scenes by a few members who after being its managers became FO heads or functionaries loyal to such heads, who used their prestigious jobs, privileges that symbolized high status and other intangible capitals gained in FOs for patronage over short-term younger local officers who aspired to FO jobs (ibid: chs 12–17). Overcoming the conservatism of these entrenched oligarchies, which was the prime source of the problems I saw in Gan Shmuel and which blocked their solution, was a formidable task, requiring much more than understanding and creating new solutions, but I could not know this from reading kibbutz literature that ignored this reality.

According to Wallerstein (2004: 15) 'We must recognize that scientific choices are informed by values and intents, as much as by knowledge and efficient causes. We need to incorporate utopistics into the social sciences'. Unknowingly that understanding local problems and proposing new solutions based on this knowledge would not help

without elimination of FO oligarchies, I decided to study sociology and psychology, but the latter was quite unhelpful; political science was required to understand the kibbutz field which resembled a mini-state (Lanir 1990). I was a good student, though not as good as I could have been, had I aimed at an academic career and stopped investing my efforts in developing my department. No one suggested such a career, since no one knew how essential it would be for coping with problems which were graver than any member imagined. During my last year of BA studies, I was chosen head of the kibbutz members' Welfare Committee, and due to the department's success, as soon as I received my BA, I was nominated as production manager at the age of 30, an honour I could not defer. This was aimed at both enhancing production and suppressing my social activism, which almost succeeded: I became immersed in the promotion of productivity by innovation, and was successful: production doubled with only a marginal addition of manpower and profitability soared. However, like kibbutz members and its students I believed in *rotatzia*, periodic managerial succession which supposedly prevented oligarchy (Leviatan 1978), but in fact enhanced it: FO heads violated *rotatzia* by their ample power which was enhanced by *rotatzia*'s weakening of kibbutz managers and FO functionaries. Hence, I left the job after three years. Had I studied political science instead of psychology, I would have delved into the kibbutz power structure and would have exposed the *rotatzia* bluff, understanding that it enhancing oligarchy. In that case, I might have stayed on the job, succeeded more and advanced to be plant manager, a power position which would have enabled me to introduce reforms.

Soon after, I was offered a promotion to kibbutz chief economic officer, but I rejected this offer and lost another opportunity to advance to a power position from which I could have introduced reforms had I known the true causes of kibbutz main problems, those which I untangled only two decades later by summing up my ethnographies. However, had I accepted this offer and furthered my managerial career, I could not have exposed kibbutz research mistakes which bolstered the conservatism of kibbutz leaders. Like many previous radicals, I would have allowed minor innovations to solve pressing problems, but not career advance and major innovations that would have threatened hegemony of the old-guard; sooner or later I would have left kibbutz frustrated like many others (Shapira 2001, 2008: chs 11–15).

Marginalization in all Organizations

I opted for MA studies in social anthropology as I still felt that my society's problems required intelligent solutions. Once more I did it largely in the evenings while being in charge of the plant's new department, missing only two days a week from work. It gained me kibbutz approval, but marginalized me further in the plant, preventing major innovations in the department. One engineer asked: 'Are you an industrialist or a sociologist?' I saw no reason to leave office without any competent successor in

the offing, and continued to manage the department even when, toward completion of my MA studies, I became a part-time research fellow of the Kibbutz Research Institute. This was a major mistake: At the age of 37 it marginalized me in the plant, the Institute and the academy, barring career advance in any of them, just as Izraeli (1982) explained how part-time work prevented women's promotion in organizations. In each of them I was suspected of being committed to the other; hence, I was not offered any job/ task which might have enhanced my career. No one alerted me to the price I was paying, since I was very productive in both the Institute and the plant, while in the academy my teachers awaited my MA thesis, which was deferred for a year because the Institute denied me the month's leave I needed to write it, as the Institute evaded FOs in accord with kibbutz leaders efforts to conceal FOs' quasicapitalistic culture (Shapira 2005).

At the Institute, I did the arduous fieldwork of research surveys which enhanced careers of seniors (Platt 1976), while I became disillusioned with this type of research as a tool for solving kibbutz problems. However, as I did not yet grasp how surveys crippled students' abilities to understand kibbutz, I continued at the Institute for five years; besides its major studies I undertook applied ethnographic studies of 20 kibbutz plants which helped Ph.D. study of an FO plant (Shapira 1979, 1980, 1982). However, due to my marginalization and being so busy, I missed how kibbutz scientific coalition, led by functionalist sociologists of Jerusalem's Hebrew University (Ram 1995), collaborated with old-guard efforts to conceal FOs' anti-kibbutz culture by not studying FOs, while I needed tools of political sociology to expose the role of this culture in bolstering old-guard's power and standing. Had I turned to this discipline, I might have discovered this collaboration much earlier and left the Institute to find an academic job that would give me independence. However, in such a case, I would not have conducted the applied ethnographic studies and their lessons would not have informed my Ph.D. study.

Further Career Mistakes, Further Marginalization

The further study of the same FO plant I had studied for an MA, for my Ph.D. dissertation, was scientifically fruitful, untangled its social processes and resulted in an analysis which all my studies and readings ever since have proven correct. My work was illuminating while preceding many authors who have sought to explain organizational trust (e.g., Kramer and Cook 2004). From a career point of view, however, one mistake was the minimal efforts to publicize my MA findings, and an even worse mistake was continuing at the Institute despite its objection to my FO research. Continuation suited the expectations of my family and fellow kibbutz members as I also continued the plant part-time job, but it prevented my gaining recognition and furthering research needed to understand kibbutz society. I should have become an assistant to a professor who,

besides mentoring my Ph.D., would have helped my academic advance by advising participation in conferences and publication.

I left the Institute when I received a Ford Foundation scholarship to write my Ph.D. dissertation and I submitted it in 1982, but instead of seeking an academic job I returned to manage another plant department and waited for two and a half years for Tel Aviv University's dissertation ratification, with no change. This extra long period clearly signalled disrespect or even levity due to my marginality. It might have also signalled animosity to my mentor, Professor Emanuel Marx. However, had I been a university employee as an assistant and had frequently met my abusers, it would have been harder to defer ratification for so long, while I would have had a better chance for post-doctoral study abroad and for enlarging my network of academic relations, something implausible at the age of 44 with only a few academic connections.

Frustrations: Hebrew Publication Efforts and Unwanted Teachings

While waiting for ratification, in the evenings I wrote articles on my findings which kibbutz journals turned down, as they exposed the ugly face of FO mismanagement and oligarchic rule; only the journal of young Mapam activists published two short ones. Academic journals also rejected my articles, presumably due to the animosity of the dominant kibbutz scientific coalition whose failures I exposed, although at the time I did not realize it. Thus, I published only in a halfacademic economics journal and a non-academic one. Estrangement from the academic community helped missing Collins' (1975: ch. 9) explanation of such coalitions; hence I did not know who thwarted my publication efforts and why, and I did not attempt to publish abroad. Then I made another major mistake: I turned down the only university job I have ever been offered, as it required leaving the kibbutz for the Negev Research Institute of Ben-Gurion University in the far south. The main reason was that I did not yet grasp how well-fortified the conservative kibbutz establishment was due to collaboration with this scientific coalition (Shapira 2005). I believed that if I published my findings in a book, my critique would move kibbutz officers to reform mismanaged FOs. Only later did I learn that my hope was doomed; kibbutz officers sought advance to FO jobs, rather than any reform of FO heads' conservatism (Shapira 2008: chs 6–8).

With Ph.D. ratification, I left the plant to become a lecturer at Ruppin College. This helped my efforts to turn the dissertation into a Hebrew book, but was disappointing academically as my colleagues were uninterested in FOs. Hence, I lacked feedback which could have benefited my book. Secondly, I tried to teach FO mismanagement and other faults of kibbutz elites to young officers who sought diplomas in order to join these elites and receive their privileges, those which I criticized as curbing FOs' efficiency and effectiveness. In addition, my book's timing was unfortunate: The kibbutz debt

crisis estranged the Israeli public from kibbutzim; thus, a book about kibbutz received minimal attention, while in kibbutzim it was not read either, as it seemed irrelevant to their grave problems since it dealt with a specific FO's problems which seemed marginal in the crisis. The kibbutz establishment banned publicity of the book in its newspapers, and my respected publisher, on the verge of financial collapse, did little to publicize it. Had I written in English, my chances of publication and of being read were better as, at that time, international academic interest in kibbutz was considerable. However, due to my kibbutz schooling, my English writing ability was poor, while I incorrectly believed in the probable impact of a Hebrew book.

Four Kibbutz Ethnographies and Failed Local Change Efforts

In 1986, I forsook efforts to publish Hebrew articles, both due to failures and to the realization that, at age 46 no university would give me a lecturing job even if I had published extensively. I opted for further kibbutz understanding with an ethnographic study of a large veteran successful kibbutz, and, to my surprise, it resulted in publications in academic journals (Shapira 1990a; 1990b) and a textbook chapter (Shepher and Shapira 1992). Thus, I continued ethnographies of three more kibbutzim, though sparing myself some work by returning to two kibbutzim previously studied by other ethnographers (Fadida 1972; Topel 1979). I studied a third kibbutz by participating in a large research project headed by a professor whose book I had positively reviewed.

Between ethnographies I also studied the exceptional radical KA activist Mordechay Shenhabi (1900–83) as it suited my habitus: Shenhabi, like me, preferred the intrinsic rewards of gaining competence by studying hardest problems and solving them creatively for the common good. He remained unknown by most KA members even after having been the source of major innovations because he was suppressed by conservative leaders who feared his ascendance. However, it took me a year of interviewing many who helped him and reading some of the thousands of documents he had left to understand that his suppression had been part of KA leaders' oligarchic rule. KA archives, which helped with my interviews, received my material and enabled David Zait (2005) to use it in Shenhabi's biography with minimal credit to me, another sign of my marginalization.

In 1991, as I was finishing ethnographic fieldwork, winds of change permeated my socially conservative Kibbutz Gan Shmuel. I felt that the time was ripe for solving some of its problems by innovation, and agreed to head the nominations committee, and then a team aimed at saving energy, which had become costlier and with considerable waste. In both roles I tried to introduce major innovations, which cannot be detailed here. Similar innovations have been successfully used by other kibbutzim, and I proposed their adoption with adaptations to Gan Shmuel's needs. Alas, they were

rejected, not because they were wrong, but because they were right. For instance, my 1993 solution for the problem of car use was introduced in 2006 by loyalists of local notables, without crediting my proposal of 1993. As I was not such a loyalist, notables defeated my innovations in 1992–93 to defend their own power. Only after failures, when reading my student study, *The Decision-Making Stratum of Kibbutz Tzidon* (fictitious name; Dangoor 1994), I grasped that in Gan Shmuel like in Tzidon and other kibbutzim, power elites of patrons and their loyal clients ruled. However, their power and standing were insecure: patrons' power was unofficial and loyal clients who were formal managers were subject to *rotatzia*; hence, they defended power by suppressing innovators (Shapira 1995a, 1995b, 2008).

The Turn to English Writing and its Failures

I then realized that the summary of my 27 case studies and ethnographies had created a very different paradigm of kibbutz society than that of current literature. As my articles were rejected by Hebrew journals, and considering that I saw no effect of my 1987 book, I turned to publishing English articles. However, except for a congress of anthropologists in Zagreb in 1988, I did not lecture on my findings at international conferences and got no feedback or ties with colleagues that could facilitate publication. My college did not finance trips abroad while my strained relations with Gan Shmuel notables barred the possibility of my kibbutz financing such trips either.

A trip abroad was essential to get feedback that would alert me to omissions and mistakes I made while interpreting what I had seen in the field. This was impossible in Israel where only two researchers, Israel Shepher and Gideon M. Kressel, defied the dominance of kibbutz scientific coalition and were marginalized like me. Trips abroad were also essential to establish connections with scientists in order to make the right decision about the type of publications: articles or a book. I erred in writing articles which exposed only a part of researchers' mistaken kibbutz paradigm, and worse still, they were mostly rejected because reviewers were coalition members, as indicated by reviews I received. Only in 1998 did I find financing for participation in the International Communal Studies Association Conference in Amsterdam where it became obvious that only a book could expose, explain and prove my different kibbutz paradigm.

However, without more international conferences I had only a handful of colleagues to review the book drafts and manuscript. This prolonged my writing to five years (1998–2003). Nevertheless, by participation in Israeli conferences and international ones held in Israel I received some feedback on the book drafts which helped me amend it to answer questions raised by expert readers. However, the dominant coalition managed to prevent its publication by two major publishers who intended to publicize it. I found a New York publisher for the book in 2003, but soon felt animosity to my manuscript by a reviewer who wrote a baseless, illintended critique. As my publisher

also received a positive review, she sought a third reviewer; it took her half a year to find one, and then it took eight months for her/him to react in this way:

> I've read the manuscript but not had time to write up my report. I can tell you now though that whilst the manuscript is interesting (and very controversial), in order to be published it would require a significant rewrite. At the moment, analytically, it's far too unfocused; stylistically it's far too messy; structurally, it's far too, well, unstructured.

Some of this critique may be correct, but such a few words after so long a time and so many violations of the promised review timetable, such general assertions without a review report and with no detailed explanation, for instance, of what rewriting was required, makes the reviewer suspect of barely reading the manuscript which at least, stylistically, was good, having been shaped by a proficient English editor. The publisher felt that her efforts to get a review that would help to edit the manuscript had failed and abandoned my book, and it seemed that other publishers, who later did the same, also suffered from this problem. One sign of this was that none of those publishers, whose rejection was accompanied by compliments to the manuscript and a wish for my success elsewhere, attached a review that could help me, unlike some journal editors who attached reviews when rejecting my articles with compliments for their ideas.

Other Unsuccessful Publication Efforts

In the meantime, I resumed efforts to publish in Hebrew, but they were unsuccessful as far as the aim of impacting kibbutz research was concerned. I translated and enlarged one of the manuscript chapters into a Hebrew article which I submitted to a respected journal and was treated as an inferior: It was accepted, but for publication two years in the future unless I shortened it into a half length, although its length was standard. I mistakenly agreed, despite doubts about whether its message would survive so drastic a cut. I did not suspect, however, that after making this effort, the editors would change it further, including its title, without my consent (Shapira 1999). It seemed that they did this, guessing that I could do little about such treatment without ties in major academic circles.

I was given quite similar treatment recently by another Hebrew journal, but in this case, after I shortened my article by a quarter with the help of two veteran professors, I did not agree to further shortening and referred it elsewhere, though to no avail as yet. I also translated the 2001 English article into Hebrew, with minor changes and submitted it to major journals, but the fact that it had been published by a respected journal, *Sociological Inquiry*, did not help; it was rejected.

Conclusions and Discussion

The general picture is clear: My triple estrangement due to efforts to combine specializations and careers in order to solve major problems of my society without violating egalitarian principles has been my Achilles heel, while this combination has also been to my scientific advantage, enabling major discoveries. I mistakenly stuck to this unconventional combination even after realizing that most kibbutz research was grossly mistaken and that only independent research would enable rectification. I missed the fact that my parttime jobs had marginalized me in the kibbutz, at the Kibbutz Research Institute and in academia. This can be explained by my:

1. missing the causal connection between part-time jobs and marginalization;

2. not knowing about the collaboration of the dominant scientific coalition with the kibbutz establishment that evaded FOs study, leading me to violate researchers' conventions and to suffer repression;

3. not learning Gan Shmuel's power structure, missing dominance of conservative patrons and their loyalists who opposed my innovations, wasting my time and energies to no avail.

On the one hand, my opponents failed: I furthered my research, exposed the field's power structure and the dynamics behind its egalitarian and democratic façade, and disproved the mistaken analysis by both the dominant coalition and critical students such as Ben-Rafael (1997) who also did not study FOs and missed their pivotal role in the kibbutz field. My publications proved that without FO study, this field is inexplicable, since FOs became Trojan horses of capitalist society, introduced its values and practices into kibbutzim, and eventually ruined their culture and then their profitability. On the other hand, my opponents succeeded: Though many of my findings were published and the rest would be published sooner or later, I did not receive recognition for them, nor were they read and used by the many audiences for whom they could have contributed decisive inputs, especially kibbutzim. There is a Jewish saying 'There is no prophet in his own city,' and my case is confirmation: I failed to be a 'prophet' whose teachings impacted kibbutzim.

However, my case proves that home anthropology can advance social science beyond adding authenticity (Narayan 1993), to the point that it transforms a whole field of study. Marx (1985: 147) pointed out that '[t]he hardest part of [ethnographic] research is the discerning of the context of phenomena;' while outside ethnographers failed to discern how the FOs capitalistic-like context shaped the kibbutz field structure, dynamics and practices. Home anthropologists Kressel (1974, 1983), Topel (1979) and Shepher (1980, 1983), guided by critical Tel Avivian sociologists and anthropologists (Ram 1995), discerned many of these capitalistic practices, helping my exposure of the rest by studying FOs under similar guidance and then kibbutzim with Professor

Marx help, ultimately leading to comprehension of the field. In accord with Hazan's (1995) reiteration that texts and their syntax have to be explained in their contexts, I explained kibbutz culture by the impact of FO context. While my nativity engendered some minor errors which slightly prolonged my route to discoveries, the major mistakes, which prolonged this route by many years and minimized the impact of my findings, were largely independent of nativity. They were mainly engendered by two factors:

1. My belief in the amending of kibbutz democracy and egalitarianism by combining two careers without knowing the formidable task I was assuming, nor the price I would pay by triple marginalization that prevented accumulation of intangible and symbolic capitals needed to overcome the interference of the dominant scientific coalition.

2. Misunderstanding the power structure of the field and missing its researchers' collaboration with leaders which I exposed after much study. Hence, I could not know who was impeding my efforts beforehand and how to cope with them effectively until quite recently.

Thus, most of this route to discovery was essential, while my nativity was the decisive factor in its success from a scientific point of view, encouraging perseverance and giving me 'the profound intuitions gained from personal familiarity' (Bourdieu 1988: 3). Had I opted early on for an academic career, I presumably would have become a high-status academic, but without the executive experience which led to the study of kibbutz plants and industrial FOs. There would have been no findings which disproved researchers' belief in *rotatzia*, and no efforts to explain its impact on kibbutz management by four ethnographies. Moreover, as a 'usual' academic, it was unlikely that I would have carried out so many ethnographies which led me to discern researchers' mistaken paradigm, since the academic rule of 'publish or perish' encourages capitalizing on minimum ethnographies by maximum publications. An ethnographer usually studies twothree communities/ tribes/organizations, to which s/he sometimes returns later (Marx 1985). With such a career, I would have missed the many insights I received from studying many kibbutzim and plants, with hundreds of grassroots informants. In addition, an academic's high status deters her/him from involvement in lowstatus grass-root problem-solving efforts and learning from them, while with my mid-level status, I had no such problem. Furthermore, finishing my Ph.D. relatively late in life, prevented both post-doctoral studies and a university teaching position, while as a college lecturer, my career was largely independent of publishing and I therefore made more ethnographies.

Kibbutz activism, which hindered my academic advance, also helped, often directing me to the right questions, although it could not substitute for ethnographies which provided answers that were tested by more ethnographies. In addition, nativity helped me to enter kibbutzim and FOs, gaining members trust and encouraging their disclosure of privy information.

My case proves that home anthropology can be a major means to discovery, especially in cultures whose past transformative leaders (Burns 1978) had encouraged critical thinking and activism. In such cases, some young radical talents have habituses of preference for the intrinsic rewards of gaining competence and learning by coping with major social problems innovatively. Those who become social activists or academics involved in activism, or who support activism may learn how complex and recalcitrant such problems are (Heifetz 1995). Every new solution produces its drawbacks, so that they may choose ethnography to gain deeper understanding of problems and find new possibilities for better solutions that will overcome the limitations of extant ones. Since social fields are often oligarchic and conservative (Bourdieu 1984, 1988, 1996), as the kibbutz field became quite early, new solutions will mostly be rejected and many of the radical talents will abandon seeking solutions in favour of career concerns. However, if their efforts are rewarded by some successes and they maintain their expectations for greater successes, this will motivate perseverance, until the roots of mistakes which blocked radically better solutions are exposed and breakthrough achieved.

An example could be W.F. Whyte's career which he concluded by recommending Participatory Action Research which seeks a *Social Theory for Action* (1992). He has been leaning towards activism ever since 1937, when he was an economics student in Harvard and studied a Bostonian slum, producing landmark ethnography (1940). In accord with the above model, during this period, Roosevelt's New Deal social climate encouraged seeking new solutions to social problems, and Whyte believed (1973: 281–3) that he could contribute to alleviating poverty by exposing how slum dwellers cope with it. This resembled my case, as Whyte turned to anthropology from another discipline for the same reason: belief in his own ability to help solve a major problem of his own society. Thus, in addition to the required habitus and encouragement of radicals by transformative leaders, a third factor of home anthropology which may lead to major discoveries, is the willingness of radicals to embark on unconventional careers of changing specializations/ disciplines. Such changes enable them to use early knowledge for later new specializations: My early technical interest was a major factor in my successful industrial management which enlightened the study of mismanaged plants, as Whyte's economic knowledge helped ethnographies of workplaces, and his early learning of Italian helped him study cooperative cultures in its sister language, Spanish (Whyte and Whyte 1988). Multi-specialization accords Wallerstein's (2004) call for the elimination of disciplinary barriers among the social sciences and the humanities (also: Peirano 1998: 123), while our cases point to the need for acquiring additional expertise types such as technological skills (as I did) or knowledge of languages (like Whyte). Thus, elimination of discipline barriers seems to be promoted by those who commence in one expertise/discipline and change to another, motivated by a desire to solve major public problems for which the first discipline/expertise is insufficient.

Are there plausible solutions that will prevent career mistakes like mine? As far as I can see, the prime solution might be coaching of young radicals by veterans. In fact, this is one of the advantages of patronage which also brings one of its drawbacks

to mind: encouraging conservatism. However, I have found that, unlike usual patronage, coaching and patronising young radicals by veteran expert radicals encouraged youngsters' innovation; as such veterans did not worry that the success of youngsters' innovations would hurt their status (Shapira 1980; 2008: ch. 16). This often happens in academia, usually without formal rewards, although it takes up the coaching professors time and energy; hence, it is common only in advanced degrees or afterwards, when coaching rewards professors by collaborative publication. However, if my Tel Aviv University professors had been formally rewarded for coaching me during my first BA year, and had understood the goals of my studies, they presumably would have diverted me from psychology to political science which would have prevented some later mistakes and shortened the route to discoveries. Such coaching might have also prevented my later mistakes, continuing so long in the Kibbutz Research Institute despite realizing it being unhelpful, and continuing my part-time job in the plant instead of taking a university job which would have gained me relevant intangible capitals and could have prevented both the disrespectful treatment of my Ph.D. thesis, and my mistaken rejection of the offer for an academic job at the Negev Research Institute.

The other main solution I can see for preventing such career mistakes is more egalitarian and democratic workplace cultures because they are more innovative, encourage radicals and tend less to punish their unconventional career choices, career competition in such cultures is less intense and altruism is more common; so even without formal rewards for coaching of junior radicals, it happens often (Shapira 2008: ch. 16). In such cultures decision-making processes are more transparent; radicals can better target true conservative power-holders who block their innovation efforts, as well as their true allies (e.g., Freeman 1974), align themselves to the latter and make fewer career mistakes. Such cultures tend to be high-trust (Fox 1974; Shapira 2008); hence, knowledge and information are widely distributed. A radical can better understand participants' behaviour and better forecast how his/her career choices will affect the success of activism, while in high-trust cultures, seniors are less worried that juniors' successes due to innovation will diminish their status; so they encourage innovation (Dore 1973: ch. 9).

Finally, an essential problem is publication of discoveries made due to unconventional careers and nativity. This touches on another major problem, the limitation of dissemination of non-English written findings to which Peirano (1998: 121) has referred. More home ethnography which is more activismoriented, and more ethnographers whose main thrust is local problems, means more non-English writing scholars aimed at local audiences, but who often discover solutions to universal problems. The problem of translation is marginal as against the problem of publication decision-making which is largely made by Anglo-Saxon elites whose interest decides what is publishable in view of what they grasp as interesting to English readers and what English-speaking reviewers prefer. This is bound to lead to major publication mistakes, and may cause an entire field of study to be mistakenly researched for sixty years, as was kibbutz. Early mistaken kibbutz ethnographies carried out by foreigners

who ignored most FO effects on kibbutzim, received publicity in prestigious outlets due to approval by foreign reviewers who were remote from the field, and to the dominance of Anglo-American functionalist sociology. Lacking 'the profound intuitions gained from personal familiarity' (Bourdieu 1988: 3), these reviewers did not suspect that FOs were part and parcel of the kibbutz field; hence, they ratified mistaken publications (Shapira 2005, 2008). This was the decisive mistake which enabled the kibbutz scientific coalition to remain dominant for half a century, suppressing critics up to now.

This situation calls for a revision of ethnographic publication decision-making practices, whether book manuscripts or journal articles, to diminish suppression of critical radicals by dominant scientific coalitions. One way is the use of natural sciences practice: even if there is only one reviewer among three-four who approves a manuscript or an article, the reviewing process proceeds to additional reviewer/s to make sure a worthwhile work is not rejected. A second method is adding 'native reviewers' who are insiders in the language and culture of the native ethnographer and are familiar with the field he has studied, even though they hold lesser academic offices. This may help anthropology to change towards less Anglo-Saxon-ethnocentric publication decision-making. A third measure may be the removal of journals' blind review process, making the names of authors, reviewers and associate editors public, along with reviewers' recommendations, review cycles, and articles under review so that other colleagues can comment on articles, and an open archive of rejected articles, as Weber (1999) proposed from a long experience as editor and reviewer.

References

Ben-Rafael, Eliezer. 1997. *Crisis and Transformation.* Albany, NY: SUNY Press.

Bourdieu, Pierre.1984. *Distinction.* London: Routledge.

——. 1988. *Homo Academicus.* Cambridge: Polity.

——. 1990. *The Logic of Practice.* Cambridge: Polity.

——. 1996. *The State Nobility.* Cambridge: Polity.

Buber, Martin. 1958[1947]. *Paths in Utopia.* Boston, MA: Beacon.

Burns, James M. 1978. *Leadership.* New York: Harper.

Collins, Randall. 1975. *Conflict Sociology.* New York: Academic.

Dangoor, Eyal. 1994. The Decision-Making Stratum of Kibbutz Tzidon, seminar paper (Sociology Dept., The Open University, Hebrew).

Dore, Ronald. 1973. *British Factory – Japanese Factory.* Berkeley: University of California Press.

Fadida, Michael. 1972. The Dynamics of Career Patterns among Political Activists in a Young Kibbutz. MA thesis (Sociology and Anthropology Dept., Tel Aviv University, Hebrew).

Fox, Alan. 1974. *Beyond Contract.* London: Faber.

Freeman, Jo. 1974. The Tyranny of Structurelessness, in *Women in Politics*, edited by J.L. Jaquette. New York: Wiley.

Hazan, Haim. 1995. *The Anthropological Discourse.* Tel Aviv: Hahotza'a La'or, Misrad Habitahon (Hebrew).

Heifetz, Ronald A. 1995. *Leadership without Easy Answers.* Cambridge, MA: Belknap.

Hirschman, Albert O. 1970. *Exit, Voice and Loyalty.* Cambridge, MA: Harvard University Press.

Izraeli, Dafna N. 1982. Women in the World of Work, in *The Double Bind, Women in Israel*, edited by D.N. et al. Tel Aviv: Hakibbutz Hameuchad, 113–71 (Hebrew).

Kramer, Roderick M. and Cook, Karen S. (eds). 2004. *Trust and Distrust in Organizations.* New York: Russell Sage.

Kressel, Gideon M. 1974. *Stratification versus Equality in a Kibbutz.* Tel Aviv: Cherikover (Hebrew).

——. 1983. *To Each According to His Needs.* Tel Aviv: Cherikover (Hebrew).

Lanir, Yoseph.1990. *The Kibbutz as a Political System.* Ramat Efal: Yad Tabenkin (Hebrew).

Leviatan, Uriel. 1978. Organizational Effects of Managerial Turnover in Kibbutz Production Branches. *Human Relations*, 31: 1001–18.

Marx, Emanuel. 1985. Social-Anthropological Research and Knowing the Arab Society', in *To become Acquainted with Neighbouring Nations*, edited by A. Har'even et al. Jerusalem: Van Leer, 137–52 (Hebrew).

Michels, Robert. (1959 [1915]). *Political Parties: A Sociological Study of the Oligarchical Tendencies of Modern Democracy.* New York: Dover Publications

Narayan, Kirin. 1993. How Native is a "Native" Anthropologist? *American Anthropologist*, 95: 671–86.

Peirano, Mariza G.S. 1998. When Anthropology is at Home: The different Contexts of a Single Discipline. *Annual Review of Anthropology*, 27: 105–28.

Platt, Jennifer. 1976. *Realities of Social Research.* London: Sussex University Press.

Ram, Uri. 1995. *The Changing Agenda of Israeli Sociology.* Albany, NY: SUNY Press.

Shapira, Reuven. 1979. *Women's Employment in the Kibbutz Industry.* Tel Aviv: Kibbutz Industry Association (Hebrew).

——. 1980. *The Absorption of Academicians in Kibbutz Plants.* Tel Aviv: Kibbutz Industry Association (Hebrew).

——. 1982. *Workers, Managers and Expertise: Trust and Market Relations in an Automatic Processing Plant.* Ph.D dissertation (Tel Aviv University, Hebrew).

——. 1987. *Anatomy of Mismanagement.* Tel Aviv: Am Oved (Hebrew).

——. 1990a. "Automatic" Rotation and Organizational Conservatism in a Kibbutz. *Megamot*, 32: 522–36 (Hebrew).

——. 1990b. Leadership, Rotation and the Kibbutz Crisis. *Journal of Rural Cooperation*, 18: 55–66.

———. 1995a. The Voluntary Resignation of Outsider Managers: Interkibbutz Rotation and Michels's "Iron Law". *Israel Social Science Research*, 10: 59–84.

———. 1995b. "Fresh Blood" Innovation and the Dilemma of Personal Involvement. *Creativity and Innovation Management*, 4: 86–99.

———. 1999. Stratification in the Kibbutz Society: A Sociological Blindness. *Teoria Ubikoret* 15: 97–108 (Hebrew).

———. 2001. Communal Decline: The Vanishing of High-moral Leaders and the Decay of Democratic, High-trust Kibbutz Cultures. *Sociological Inquiry*, 71: 13–38.

———. 2005. Academic Capital or Scientific Progress? A Critique of Studies of Kibbutz Stratification. *Journal of Anthropological Research*, 61: 357–80.

———. 2008. *Transforming Kibbutz Research: Trust and Moral Leadership in the Rise and Decline of Democratic Cultures.* Cleveland, OH: New World Publishing.

Shepher, Israel. 1980. The boundaries of kibbutz, in *A Composite Portrait of Israel*, edited by E. Marx. London: Academic Press.

———. 1983. *The Kibbutz: An Anthropological Study.* Norwood, PA: Norwood Editions.

Shepher, Israel and Shapira, Reuven. 1992. The Individual and the Community in the kibbutz, in *The Kibbutz Society: Change and Continuity.* Unit 9, Tel Aviv: The Open University (Hebrew).

Spiro, Melford. 1955. *Kibbutz: Venture in Utopia.* Cambridge, MA: Harvard University Press.

Topel, Menachem. 1979. *To Build and Be Built: Power Elite in an Egalitarian Community.* MA thesis (Sociology and Anthropology Dept., Tel Aviv University, Hebrew).

Wallerstein, Immanuel. 2004. *The Uncertainties of Knowledge.* Philadelphia, PA: Temple University Press.

Weber, Ron. 1999. The journal review process: A manifesto for change'. *Communications of the Associates for Information Systems* 2, Article 12, August.

Whyte, William F. 1940. *Street Corner Society.* Chicago: University of Chicago Press.

———., 1992. *A Social Theory for Action.* Newbury Park, CA: Sage.

———. and Kathleen K. Whyte. 1988. *Making Mondragon.* Ithaca, NY: ILR Press.

Zait, David. 2005. *The Dreamer and the Implementer.* Givat Haviva: Yad Yaari (Hebrew).

Chapter 7: Some Cautionary Tales from an Anthropological Romance with Jews from Libya

Harvey E. Goldberg

Nomadism is one of the romantic images running through anthropological work. Studies of nomadic groups, whether foragers or herders, and whether anchored in arid or ample rain-fed zones, have not fully dispelled that image. Anthropology has decidedly contributed ethnographic concreteness to the notion of 'nomads'. Their life-ways have been depicted as comprehensible responses to environmental constraints and interaction with other populations. With time, the involvement of nomadic groups with states came to the centre of attention (Khazanov 1984). Emanuel Marx's Bedouin of the Negev (1967) was an early and clear demonstration of how state structures and policies affected nomads by limiting the area in which they could reside and raise their flocks. But the progress of social analysis does not necessarily eliminate challenges presented by widespread images that enter into ethnographic work.

Such challenges are found in popular romantic notions, but also may be built into the very vocabulary of discussion. When Marx describes the administrative limits dictated to Bedouin in southern Israel, he uses the term reservation. An American reader (anthropologist, or otherwise), coming across that term cannot help but be reminded of Native Americans in the United States. To what extent does this association point to a helpful comparison, and to what extent does it blur significant differences in the settings? Another dilemma is built into the very designation Bedouin. The term is still a significant category in Israeli society, but some have questioned its current relevance. Many people placed in that rubric have become engaged in the Israeli economy in ways quite disconnected from nomadic pastoralism. An anthropologist seeking ethnographic verisimilitude and analytic precision may thus find himself in a struggle with his readers, whether among his colleagues, the people about whom he writes, or a wider field of popular representations.

The dilemmas of representation become even more complex when participants in a society, which in the past has been the object of study, join in the effort to produce ethnography or history. In a region not far from the groups studied by Marx, Andrew Shryock (1997: 31–2) has shown the subtleties involved when Bedouin began adapting their oral histories cum genealogies to print media. An anthropologist seeking to make

a contribution in such a complex context faces many choices, and never can be sure how they will be appreciated by various audiences that s/he may engage. Among other dilemmas, the very notion of anthropology, and images of what anthropologists do, may be called into question. The attempt to trim romantic stereotypes down to size may become entangled in a variety of representations of what anthropology is about.

With these various dilemmas of discourse in mind, I offer some examples of my own experience as an anthropologist who first studied, in the 1960s, a group in Israeli society that carried romantic connotations: the 'cave-dwellers of Tripolitania' (Goldberg 1972). This image was highlighted in the wake of the large-scale immigration after 1948 that brought some Jews from 'remote' areas of the Middle East to Israel. The image was not created in that country, however, and appears in reports on Jews in North Africa from the beginning of the twentieth century. This enables one to trace the image and its implications in various settings.

Moreover, as my research on Jews from Tripolitania (in Libya) expanded, both in scope and in method, the nature of the discursive engagements shifted. From a situation in which I concentrated on ethnographic accuracy within an accepted anthropological paradigm of trimming 'myths' down to size, I found myself, in the 1970s, entering the bourgeoning Israeli arena of 'heritage' and a 'politics of identity'. Such developments, along with the persuasive changes in the technologies of representation attendant upon the digital era, create an ever-changing and bewildering set of issues regarding representation that an anthropologist must take into account in the attempt to portray a group, institution, or social field (Ortner 1998). While attention to such challenges is an inevitable part of anthropology today, I argue that the discipline's classic (or even 'romantic') task, that now has many detractors, of exploring ways of life poorly understood by the group from which he originates, constitutes a goal still worth pursuing.

Depicting Jewish 'Cave-Dwellers'

In the course of the nineteenth century, Jews in Europe, themselves only recently having experienced emancipation and citizenship in nation-states, showed an increased awareness of Jews in the 'East' (Schroeter 2002). This term could apply both to Eastern Europe and to Jewish communities in the Balkans and Muslim lands. This expanded 'imagined community' could fit into a range of ideologies. It informed the activities both of the Alliance Israélite Universelle (AIU), founded in 1860, which sought to integrate Jews within their wider local societies (Rodrigue 1993), and also appeared in Zionist ideology as it emerged later in the nineteenth century.

One person who had a foot in both these institutional and ideological worlds was Nahum Slouschz, who grew up in Odessa and pursued advanced studies in Geneva and in Paris. Slouschz was involved with Zionism from a young age, but also became connected to the AIU in Paris. That organization partially supported his research trip

to North Africa in 1906, where the Ottoman governor general in Tripoli authorized him to 'travel throughout the maritime oasis of Libya and the three Jebels of the Sahara' (Slouschz 1927: vi). In these travels, he was accompanied by a local Jew, Mordecai Ha-Cohen, who was familiar with the region through his commercial activities, but who also had scholarly interests. At the time of Slouschz's trip, Ha-Cohen was composing a book on the history of the Jews of Libya, its institutions, and its customs (see below). Discussed at length elsewhere (Goldberg 2004), I highlight here one phase of Slouschz's trip and writings: his stay among the Jewish 'cave-dwellers' in the Gharian plateau south of Tripoli. The phrase referred to a type of house construction prevalent in the region that involved excavating down and creating a large courtyard, and then digging out rooms off the courtyard in a familiar Mediterranean pattern. A long section of Slouschz's *Travels in North Africa* (Slouschz 1927: 115–53), an English version of materials he published previously in French and in Hebrew, is dedicated to this community.

Slouschz's account exemplifies several 'cardinal sins' of representation, concerning which anthropology and other disciplines have warned us. It is evolutionist in outlook and occasionally uses explicit racist phrasings. The Jews in the Gharian are seen to represent earlier stages in Jewish history; at times Slouschz even calls them 'prehistoric'. Johannes Fabian's (1983) analysis of the non-contemporaneity of the objects of ethnographic description easily applies to his writings, as does Edward Said's notion of 'orientalism' (Said 1978). The term 'troglodytes' with reference to the dwellings in the Gharian, which Slouschz traces back to Herodotus, provides an objectivizing 'learned' discourse that allows his own perceptions and understandings to dominate the account. Yet Slouschz was one of the few scholars to pay attention to groups like these at all, and the fact that he became fairly well-known (though controversial) in the then small republic of Hebrew letters meant that his account of the cave-dwellers became available to a wide, even though scattered, audience of Hebrew readers and writers.

The exotic notion of Jews living in caves had wider appeal too. The Second World War and its aftermath provided another context for training attention on this group. When the Eighth Army completed its takeover of Libya, some Jewish soldiers from Mandate Palestine wanted to make contact with communities there (cf. Urbach 2008: 129–34). One of them was Polish-born Dov Noy (personal communication), later to become Professor of Folklore at the Hebrew University. Noy was familiar with Slouschz's writings and when he heard that one of the officers was travelling to the Gharian and would make contact with Jews there, he asked to come along. As elsewhere, the meeting between the local Jews and soldiers from the Land of Israel was emotional. Noy relates that after leaving the place, he realized that someone had secretly cut off a small piece of his military jacket, apparently to retain it as an object charged with symbolic value. Noy was later tried by his military superiors for this neglect, while the incident demonstrates both the powerful attraction of 'Zion' in the eyes of local Jews, and the appeal that these communities had for those committed to Zionist ideals.

When Jews from Middle Eastern regions began arriving in Israel after independence, and became objects of general and academic curiosity, Slouschz's writings were among the limited sources available for learning about their life in North Africa.

I also read Slouschz's *Travels in North Africa* in preparation for my doctoral fieldwork. After arriving in Israel in the autumn of 1963, I decided to study an immigrant village (moshav) of people from Tripolitania. Various factors fed into this choice. One is that as a BA student several years earlier, I spent about six weeks on such a village, named Shalva, which is south of Qiryat Gat in the Lachish region (see below). At that time I knew little of anthropology, but when my interests developed in that direction, and was told that there had been little study of 'Tripolitanians' in Israel, it appeared as an attractive choice. My training at the time oriented me to look for one of the more traditional communities among the fourteen villages that were largely inhabited by people from Libya (Goldberg 2010 [1973]). When I discovered that a possibility existed with regard to Moshav Porat (in earlier publications I assigned the pseudonym of 'Even Yosef' to the moshav), and realized that these were the same 'cave-dwellers' whom Slouschz had described, this became my village of choice, and the cradle of my growth as a field anthropologist.

Like other anthropologists and rural sociologists at the time, I described aspects of the moshav's social structure and how that evolved while adjusting to a new society (Goldberg 1972). At the same time, I found myself constantly delving into the past of the community, both by querying villagers and seeking any written sources I could find. The resulting dissertation, and the book that evolved from it, had as full a 'microhistory' of the community as I could manage to put together. One aim of this history was to de-mystify the romantic notions that adhered to the image of 'cave dwellers'. Yet, while choosing a title for the book, I allowed myself to indulge in what I had hoped would be a tantalizing title, attracting the attention of potential readers.

The title *Cave-Dwellers and Citrus Growers* was partially inspired by an article on the moshav that had appeared in an English-language publication with the caption: 'Caves to Cottages' (Fenson 1951). Seeking, partially, to capitalize on the romantic appeal of this image, I, at the same time, sought to distance myself from the caption by using the coupling word 'and' rather than 'to'. This shift was motivated by two considerations.

Anthropologists are trained to be very cautious about any conceptual scheme that smacks of evolutionism. Putting the story of the moshav (or any group) in a 'from–to' framework went against anthropological grain. A second reason stemmed from what I had learned in the course of my research. Far from being a postZionist (the notion did not exist at the time), my studies regarding Jews in Libya had taught me that the mass move from there to Israel was only one development that could have taken place (and in fact did), but other historical scenarios might have evolved under different circumstances. In addition, a certain proportion of the Libyan Jewish population did not follow that route. Thus, life in the Tripolitanian hills (cave dwelling) and the experience of moshav life (citrus growing) were both salient features of the history of

these people but how they were to be linked was a story the villagers ought to tell, and not have it imposed on them from outside. All these musings were packed in the shift from a word of two letters to one of three, and I wondered at times whether anyone paid attention to this.

I soon learned that not only did people *not* perceive the planned innuendo, but they also misread the title. That time (1972) was also the period during which I came to live and work in Israel. As a newcomer, I was invited several times during the year to lecture about my work. On two separate occasions I was introduced as author of the book *From Cave-Dwellers to Citrus Growers* (sic!). It was striking that the same (mis)perception of the volume's title appeared among two people whose work is quite different: one was Dov Noy (see above), who may be considered the founder of Jewish folklore studies in Israel, and the other anthropologist Henry Rosenfeld whose work on Arab villages communities pointed to the dislocation these groups underwent in the new Israeli state (cf. Al-Haj et al. 2006).

Upon recalling the period when I was finishing the book, I estimate that my substantive and positive attempt to provide a fuller and historically grounded portrait of the Jews in Gharian was written against two sets of notions. One was based in my perception that 'Israeli society' readily latched on to the notion of 'the cave-dwellers of Tripolitania'. As I had noted, 'In some ways the wider society has shown a greater interest in the Gharian Jews than the latter have in Israeli society' (Goldberg 1972: 85). Perhaps, this was a way for the society to celebrate its own heroic programme of 'the ingathering of the exiles', while ignoring the price paid by those ratcheted into the 'cave-dwellers' image. The second object of my pointed ethnographic depiction was the academic community concerned with immigration to Israel. This was dominated by sociological perspectives which in the 1950s–60s began with a model of 'the absorption of immigrants', into which was later poured a modernization paradigm. I always have viewed 'modernization' as a latter-day version of evolutionism critiqued by American and British anthropology early in the twentieth century, and therefore have been cool to it. But, as the misreading of my colleagues testifies, the minor way in which a critique was embedded in the wording of my title, had little chance of dislodging discursive structures still in place. These were even evident among people who, when pushed to reflect more explicitly on the language in which they were enmeshed, might express reservations about those structures.

I now also realize that a certain tension existed between the stances I assumed vis-à-vis the two set of assumptions that I sought to challenge. With regard to popular images of the 'primitive' past of the immigrants from the Gharian (see Zenner 1963), I wanted to demonstrate that this past could be understood in 'rational' terms comparable to the history of any immigrant group. In respect to the sociological paradigms, which focused on what the immigrants would (should?) become in their new society, I wished to insist that attention to and acceptance of a group's past was an important aspect of making them full partners in the present. What was missing in both these implicit 'conversations' was an attempt to directly address the people who had been the subject

of my research about these issues. After settling in Israel, I visited Porat now and then, and gave a copy of the book to my closest informant, Disi Hajjaj (see Goldberg 1972: picture 5, 144–5). But the book and most of my writing was in English, so Disi could not relate to its contents, but only to the fact that a book existed and also to the pictures inside it. This situation soon changed, however, with reference to Jews from Libya more generally, and the projects that I undertook subsequent to my study of a single village community.

The Varieties of Heritage Politics

At the time that I was working on *Cave Dwellers*, issues of ethnicity relating to Jews from the Middle East were assuming new forms in Israeli society. An explicit link of cultural issues to politics began to gain strength. One clear expression of this was the founding, in 1976, of the 'Centre for the Integration of the Oriental Jewish Heritage' in the Ministry of Education, which provided budgets to advance projects in this direction that ranged from primary education to research in universities (see, for example, Shavit 2003). Within this atmosphere, I embarked upon historically oriented projects concerning Jews in Libya.

As indicated, my fieldwork in Porat made me extremely curious about the villager's background. After my doctorate, I wished to further investigate the lives of Jews from Libya in a historic-ethnographic fashion and returned to Israel in 1968/9 to undertake a pilot study in that direction. (Goldberg 1974). That year, several people mentioned a manuscript on the Jews of Libya found in the National Library in Jerusalem. It turned out to be an unpublished book written by Mordecai Ha-Cohen, who lived from 1856 to the late 1920s, and was born in Tripoli to a family of Italian origin from early in the nineteenth century. The book, which the author called *Higgid Mordecaï* ('Mordecai Narrated' – taken from the Book of Esther 6:2), was written in a Hebrew script popularly called 'Rashi script', and was a mine of information. In producing it, Ha-Cohen engaged in a wealth of activities. He copied extant manuscripts from the end of the eighteenth century, talked to old people about past times, poked about in cemeteries for inscriptions, provided lists of words in a local Jewish argot, described many of the political and cultural events of his own day, extracted discussions of customs from the books of rabbis active in Tripoli, and devoted a special division to the life of the Jews in the small towns of the Tripolitanian countryside. This division included 21 of the 114 sections that constituted the book (Ha-Cohen 1978; 1980).

This latter feature of the manuscript was especially important to me for two reasons. It showed how important Ha-Cohen was, intellectually, in the joint trip he took with Slouschz to the Tripolitanian interior (Ha-Cohen 1980; Goldberg 2004). What excited me most at the time was what would be seen as a bonanza to many field anthropologists, particularly to one who had no access to the original setting of 'his people', as was the case of anthropologists working among Middle Eastern immigrants

in Israel. Here was a fairly detailed description, from the early twentieth century, of the precise community I had studied in the mid-1960s. Two of the sections focused on the Gharian, and the twelve preceding sections described Jews in the neighbouring region, Jebel Nefusa, an area where Jews lived in close proximity to Berber-speaking Muslims. As my familiarity with the Tripolitanian communities in Israel deepened, I learned that the people on moshav Shalva, where I first encountered Jews from this background in 1959, came precisely from the Jebel Nefusa. This seemed to be a golden opportunity to combine ethnography with historical research, a step that was being urged by many anthropologists at the time.

My first idea was to work toward a book (in English) that would include a translation of the division of *Higgid Mordecaï* that focused on the rural communities, along with a commentary enriched by my field experiences in the 'same' communities as they continued to exist in contemporary Israel (about half of the fourteen moshavim of Jews from Libya could be viewed as 'transplanted communities'). I approached Machon Ben-Zvi, a research institute, linked to the Hebrew University, that was devoted to the study of Jews from Eastern Communities. The director of the institute encouraged me, but said that, if I were to delve into the manuscript, it made sense to publish the whole work which still had not seen 'the light of day', and in the original Hebrew. This task would partially draw upon my training as an anthropologist, but also required competencies more common among historical and textual scholars. I felt the need to consult many people about this matter, particularly as I learned that there had been previous attempts to publish the manuscript that did not reach fruition.

Among those I consulted was historian H.Z. Hirschberg, one of the few scholars in Israel studying Jews in Muslim lands who also took an interest in the modern period. Hirschberg was familiar with the manuscript, and drew upon it in his 1965 two-volume work: *Toldot Ha-Yehudim be-Afrika Ha-Tsfonit* (for a translation see Hirschberg 1974–81). He had encouraged an MA student, Yehudah Kahalon from a Tripolitanian family, to take on the editing of the book (Kahalon 1972 represents his MA study), but health problems prevented Kahalon from undertaking this work.

Hirschberg also indicated that I should meet Rabbi Frija Zuaretz, a key figure among Jews from Libya in Israel. Zuaretz (1907–93) was born in Tripoli, and served as the rabbi in the coastal community of Khoms. In Israel, he led the Committee of Libyan Communities, an ethnic organization operating under the auspices of the National Religious Party. In 1955, Zuaretz was drafted by that party to their Knesset list, and served as a Member of Knesset for two terms. I had met Zuaretz during my first field trip (1963–5), and he always had encouraged my research. In the case of the *Higgid Mordecaï*, he provided some background of his own connection to the manuscript.

In addition to the manuscript housed in the National Library (that reached Jerusalem upon the advice of the Chaplain Ephraim Elimelekh Urbach working with the Jewish Palestinian Brigade that entered Tripoli in January 1943; see Urbach 2008: 184–5), another copy was in the possession of the family arriving in Israel after 1949. One of Ha-Cohen's surviving sons (he had nine daughters and four sons), Yehudah

Ha-Cohen, held this manuscript, and wanted it to appear in book form. At one time Yehudah was in contact with Zuaretz, who sought to promote its publication. Some misunderstanding developed between him and Ha-Cohen, and this plan did not move forward. Yehudah Ha-Cohen passed away and his younger sister, Fortuna (Toni) Mosseiri, inherited the written material. The remaining Ha-Cohen descendants made an effort to publish the manuscript by collecting funds among themselves, and paying an acquaintance to type it up during his spare time. He finished about one third of the project when I approached the family with my interest in bringing about its publication under the auspices of Machon Ben-Zvi.

Eventually, I turned into a successful broker. The institute provided the professional skills necessary to turn the manuscript into a book, and the HaCohen family contributed some funds to underwrite publication costs. This took place several years before the Ministry of Education established its 'Centre for Integration', at which time budgets for such projects became readily available. This successful 'marriage' notwithstanding, tension developed when it took much longer for the book to appear than Mrs Mosseiri expected. Her work, as a cleaner in the Jerusalem court, placed her in contact with legal personnel, and she consulted one of the judges whether she might take legal action against Machon Ben-Zvi. The judge, acquainted with her for many years, suggested that she express her impatience in verse, as he knew she was accustomed to penning poems about important events. In a book of her own poems that she printed in the 1980s, one poem, entitled 'A Complaint in Rhyme' expresses her annoyance at Shaul Shaked, Professor of Iranian Studies and at that time director of the Ben-Zvi Institute, but ends with the hope for a successful completion of the project (Mosseiri n.d.: 53). This indeed took place in 1978. At a symposium on Jews in Libya later organized by Machon Ben-Zvi, Mrs Mosseiri and her family were present, and she, who had no children, affectionately referred to me as her father's grandson.

The family did not pay close attention to the substance of the work. Thus, it was not at all a problem that I often turned to Frija Zuaretz for assistance in understanding passages of the manuscript or related literature. I, of course, acknowledged his help when the book appeared, and felt that the way we had worked together exemplified constructive cooperation between 'a young American' and Jews from Libya in fostering part of their heritage (which, inspired by Ha-Cohen, I also viewed as a universal heritage), that had been neglected while the book lay unpublished. Elsewhere, I have discussed other examples of such synergetic efforts (Goldberg 1992), but after *Higgid Mordecaï* appeared there were also those who were not pleased at the way the effort had been carried out.

Two critical statements regarding the publication of *Higgid Mordecaï* appeared in print by people of Libyan background: one on the pages of a daily newspaper, and the second, eight years later, in a form that reached a limited audience. Each had a different emphasis. The first was written by Rabbi Yitzhak Barda, who for a period was the Sephardi Chief Rabbi of Ramat Gan. Barda was born to a family who were among a small number of immigrants from Tripoli to Mandate Palestine in the mid-

1930s. In a letter to the daily *Yedioth Aharonoth* from 11 February 1979, he provided four examples of recent work that cultivated the culture and heritage of *edot ha-mizrah* (Middle Eastern communities), being placed in the hands of 'Ashkenazi' Israelis. One instance was as follows:

> Machon Ben-Zvi recently published the book of Rabbi Mordecai Ha-Cohen of Tripoli, Libya. Despite the request of members of the *edah* to edit and publish the book, the work was placed in the hands of Mr Harvey Goldberg.
>
> Even more ironically, Mr Goldberg is an anthropologist, as if the one hundred thousand Jews of Libyan origin living in the country (this was the very rough estimation at the time, HEG) are only an anthropological exhibit ...

I had on occasion met Barda at events promoting Libyan Jewish heritage, but only at the level of 'shaking hands'; I never had carried out a conversation with him. I felt his letter called for some response.

I therefore turned to Frija Zuaretz, showing him the newspaper item. He expressed regret and said he would write to Barda. He sent a letter, with a copy to me, on 3 Adar 5739 (2 March 1979). In the letter, translated in the Appendix below, Zuaretz described the background to the effort, including the cooperation between us. He even took upon himself 'the blame' for the misunderstanding, stating 'it was a mistake on my part not to have brought this information beforehand to Your Honour and other personages in the *edah*.' I never heard directly from Barda, but may have received a roundabout apology from him.

This was at a meeting, in Netanya, devoted to attracting wider interest in the heritage of Jews from Libya. Barda spoke, and claimed that it was important to have consistent activity, rather than waves of enthusiasm. In his speech, he inserted a remark that 'sometimes we show too much enthusiasm', which I surmised to be a veiled statement that he had gone too far in his letter to the newspaper. Barda did not approach me personally (nor did I approach him), but I am certain he was aware of my presence. His claim had nothing to do with the content of *Higgid Mordecai*, but was a pure case of identity politics. Years later, a second instance of criticism of my work on the book, relating to matters of content, was expressed in writing.

This response was by Rabbi Levi Nahum, who was born in Tripoli but educated in *yeshivot* in Israel. Nahum has taken it upon himself to reissue, or bring to publication for the first time, books written by rabbis in Tripoli. Most of these publications feature the work of Rabbi Ya'aaqov Raccah, an important rabbinic figure in nineteenth-century Tripoli, and also an ancestor of Nahum. In one introduction, Nahum took the opportunity to criticize statements by the editor of *Higgid Mordecaï* (L. Nahum 1987).

My introduction to that book included the following statement: 'the community lacked rabbis of outstanding stature (*ba'alei she'ur qomah*), [and] most of the local rabbis [*hakhamim*] devoted themselves to the study of kabbalah' (Ha-Cohen 1978: 16). This account was an unsuccessful reworking of a sentence found in Hirschberg's

historical discussion of Jews in Tripolitania (1965, vol. 2: 201). Nahum forcefully made two claims. He argued (correctly) that in Jewish tradition, one is not supposed to study kabbalah unless he is already well grounded in the standard realms of Talmudic and rabbinic literature. Secondly, he showed that Raccah was a figure known outside of Tripoli. Nahum brought sources demonstrating that the publishers of prayer books in Leghorn, Italy, gave weight to some of Raccah's rabbinic decisions on liturgical matters in determining the details of some liturgical texts in these books.

There is no doubt that in writing the sentence in the introduction, my knowledge of rabbinic literature from Tripoli was meagre. Upon re-reading Hirschberg's chapter which I assumed as a base, I now perceive the caution built into his phrasing. Without assuming a defensive attitude, I note that the whole field of rabbinic literary production in North Africa is understudied and underappreciated, except for a few scholars and selected emphases (examples are Deshen 1989 and Zohar 2001). Our knowledge is limited not only with regard to the content of rabbinic writings, but we know little about the institutional side of rabbinic culture in the region and its networks beyond North Africa. I would not repeat precisely my simplified generalization about the local rabbis today, but I would try to examine Rabbi Raccah's contribution in terms of whether he mainly focused creative energies on ritual issues or whether he forged an approach to the broad and new challenges facing Jewish societies in the latter half of the nineteenth century. As to the main point of this discussion, I wish to underline the very different level of discourse at which Levi Nahum critiqued the work on *Higgid Mordecaï*, in comparison to Barda. I emphasize this, even while knowing that a 'politics of identity' was not absent from L. Nahum's concerns. To further stress variety in the ways Jews from Tripoli have reacted to being the 'objects' of anthropological research, I bring one more example.

In my ethnological work carried out in 1968/9 (Goldberg 1974), I met Raphael Nahum from moshav Hatzav, composed of people from Tripoli who received classic Zionist socialization before emigration, including the value of 'settling the Land'. R. Nahum, like most immigrants from Libya, had experienced the tribulations of living in a *ma'abara* (a 'transit camp' that eventually became the town of *Qiryat Malakhi*), but by the time I met him was settled in a comfortable home and held the position of secretary of his cooperative village. During one of my subsequent visits, he gave me a book of short stories that he had written and published on his own (R. Nahum 1972). The first story was situated in North Africa, while the second reflected, with gentle irony, tribulations of *ma'abara* existence and the struggle to establish new agricultural villages. His combination of light humour and irony touched upon both the immigrants in the stories and those charged with 'absorbing' them.

The second story was called *Beqesher*, presented as the nickname of an immigrant who enjoyed some success in the settlement process, and was now involved in a project seeking to raise the spirits of those still facing difficulties. A budget became available enabling *Beqesher* to arrange an evening during which immigrants with experience as entertainers in their original country would perform before the *ma'abara* residents,

and also earn pay for their efforts. This entailed much improvisation, such as putting together a stage from orange crates. Among the mishaps of the evening was that a young woman, *Titoqa*, selected to sing and dance while accompanied by musicians, trapped her foot in one of the crates, so that she could sing but was unable to move her body. This provides the setting for R. Nahum to 'return the gaze' onto those located on the 'absorber' side of immigrant-veteran interaction:

> In the audience were two students of folklore. They were deeply impressed by the mysticism of the ritual and one of them said: 'There is something special about this Jewry; the foot stuck in the crate, and the songs of longing, express Messianic yearning.' However, during the first intermission, when *Beqesher* mounted the stage and apologized to the audience for the technical problem, expressing hope that by the next performance the village would have a proper stage and *Titoqa's* feet would not get stuck in a crate, the faces of the students soured with anger, like a professor whose archaeological discovery was invalidated (60).

His wry portrayal of how researchers 'observed' the new immigrants notwithstanding, R. Nahum willingly agreed to my request for several ethnographic interviews. On one occasion, I was accompanied in the field by the late Haim Blanc, whose expertise in Judeo-Arab dialects (Blanc 1964) lent a new and detailed level to the content of the interviews that was stimulating to all three of us.

Raphael Nahum's diverse responses, placed alongside the two rabbinic ones regarding *Higgid Mordecaï*, show that an anthropologist writing in a manner accessible to the people he has studied may encounter a range of attitudes and responses. Assuming the existence of homogeneous perceptions within 'ethnic groups', hardly does justice to the topic of the implications of anthropological research for those who have been objects of it. People who are subjects of research may vary in attitudes as much as anthropologists themselves.

Conclusion

Fifty years ago, the book *In the Company of Man* (Casagrande 1960) presented intimate portraits of the key informants of twenty anthropologists, and the complex give-and-take that emerged in their work together over the years. The collection represented the growing feeling that 'scientific' publications did not adequately reveal the significance of personal experiences and interactions that lay at the heart of fieldwork in other societies. Since then, the emphasis on personal links between a researcher and the 'researched' has been dwarfed by other perspectives. Anthropological work has been placed in the context of colonialism and orientalism; the process of anthropological writing has been 'deconstructed'; and anthropologists have at times been

pictured as assuming an exploitative relationship to the groups among whom they have conducted research.

Anthropology in Israel began to take shape only from about the same period of time (late 1950s), and the smallness of the country, threaded with intensive media (written and later electronic), has meant that anthropological work there has long been only one mode through which groups and institutions have been represented to the wider society.

The ethnographic anecdotes assembled here, selected from the ongoing sojourn of one American-born anthropologist among Jews from Libya, show that no single theory can exhaust this multifaceted experience. Simplified views about the nature of anthropological work, along with presuppositions about the interaction between anthropologists and the people among whom they work, misrepresent the variety of situations that have emerged, and the personal relationships developing within them. Some groups are indeed enmeshed in romantic images that dissipate with difficulty, and 'anthropologists' too have not easily shed the popular images adhering to their discipline. Perhaps these are 'destined' to remain intertwined.

So long as social life and politics produce romantic images of under-empowered groups, there will be 'culprits' accused of perpetuating them. Anthropologists obviously should be aware of this, but need not take upon themselves the 'original guilt' for processes, to which occasionally they may have been accessories, but which they usually did not create.

Appendix

Translation of Frija Zuaretz's letter to Rabbi Barda

3 Adar 5739

My friend, Prof. Goldberg, who has prepared for publication the book *Higgid Mordecaï*, called my attention to Your Honour's letter in *Yedioth Aharonoth*, and I feel that I must bring the following to Your Honour's attention:

At one time, I and a few other members of the [our] *edah* undertook an initiative to edit the book and publish it under the auspices of The Committee of Libyan Communities in Israel. We held several meetings and even divided tasks among ourselves regarding the project. To my regret, the project was not brought to fruition for reasons over which we had no control. I returned the photocopy of the manuscript in our hands to the late Yehudah Ha-Cohen, of blessed memory, and that brought the matter to an end. After some time, when it became known to me that Machon Ben-Zvi was planning to publish the book, and the editor would be a professor of the social sciences at the Hebrew University, I was pleased and thought –

132

as I continue to think today – that this brings honour to the topic, to the author of the book, and to the *edah* generally. Professor Goldberg was in constant contact with me during the whole time that he worked on preparing the book for publication; we met, upon his request, five-six times to clarify points that were not clear enough to him (he mentions this several times in the book), and we thus have merited an important publication. Now, after the fact, I believe that it was a mistake on my part not to have brought this information before hand to Your Honour and other personages in the *edah*.

In this manner I might have prevented misunderstandings with regard to this important topic, and 'who can fathom mistakes?' [Psalms 19: 13]. I am truly sorry for this lapse on my part and request Your Honour's pardon ....

References

Al-Haj, Majid, Saltman, Michael and Sobel, Zvi (eds.) 2006. *Social Critique and Commitment: Essays in Honor of Henry Rosenfeld.* Lanham, MD: University Press of America.

Blanc, Haim. 1964. *Communal Dialects in Baghdad.* Cambridge, MA: Harvard University Center for Middle Eastern Studies.

Casagrande, Joseph B. 1960. *In the Company of Man: Twenty Portraits by Anthropologists.* New York: Harper.

Deshen, Shlomo. 1989. *The Mellah Society: Jewish Community Life in Sherifian Morocco.* Chicago: University of Chicago Press.

Fabian, Johannes. 1983. *Time and the Other: How Anthropology Makes its Object.* New York: Columbia University Press.

Fenson, Mel. 1951. Caves to Cottages. *Zionist Review*, 11(25): 11–13.

Goldberg, Harvey E. 1972. *Cave-dwellers and Citrus-growers: A Jewish Community in Libya and Israel.* Cambridge: Cambridge University Press.

———. 1974. Tripolitanian Jewish Communities: Cultural Boundaries and Hypothesis Testing. *American Ethnologist*, 1: 619–34.

———. 1992. History and Experience: An Anthropologist among the Jews of Libya, in 'Going Home' edited by J. Kugelmass, *YIVO Annual*, 21: 241–72.

———. 2004. The Oriental and the Orientalist: The Meeting of Mordecai HaCohen and Nahum Slouschz. *Jewish Culture and History*, 7: 1–30.

———. 2010 [1973]. Culture Change in an Israeli Immigrant Village: The Twist Dance in Moshav Porat, in *Perspectives on Israeli Anthropology*, edited by E. Hertzog et al. Detroit: Wayne State University Press, 31–9.

Ha-Cohen, Mordecai. 1978. *Higgid Mordecaï: Histoire de la Libye et de ses Juifs, lieux d'habitation et coutumes*, édité et annoté par H.E. Goldberg. Jerusalem: Machon Ben-Zvi (Hebrew).

————. 1980. *The Book of Mordechai: A Study of the Jews of Libya*, by Mordecai Ha-Cohen translated and edited by H.E. Goldberg. Philadelphia: Institute for the Study of Human Issues (reprinted: London, Darf Publishers, 1993).

Hirschberg, Haim Z. 1974–81. *A History of the Jews in North Africa* (2 vols). Leiden: Brill.

Kahalon, Yehudah. 1972. La lutte pour l'image spirituelle de la communauté de Libye au XIXe siècle, in *Zakhor Le-abraham: melanges Abraham Elmaleh*, edited by H.Z. Hirschberg. Jerusalem: Comité de la communauté marocaine, 79–122 (Hebrew).

Khazanov, Anatoly. 1984. *Nomads and the Outside World*. Cambridge: Cambridge University Press.

Marx, Emanuel. 1967. *Bedouin of the Negev*. Manchester: Manchester University Press.

Mosseiri, Toni n.d. *Le-hallel Halleluyah: Shirei Toni Mosseiri* [poems by Toni Mosseiri]. Jerusalem: no publisher (Hebrew).

Nahum, Levi. 1987. Introduction, in *Shulhan Lehem Ha-Panim: Hilkhot Shabbat, by Yaaqov Raccah*, edited by L. Nahum (ed.) Jerusalem: Machon Hai Hai (Hebrew).

Nahum, Raphael. 1972. *Mi-kan u-mi-sham* [From Here and From There], no publisher (Hebrew).

Ortner, Sherry. 1998. Generation X: Anthropology in a Media-Saturated World. *Cultural Anthropology*, 13: 414–40.

Rodrigue, Aron. 1993. *Images of Sephardi and Eastern Jewries in Transition: The Teachers of the Alliance Israélite Universelle*, 1860–1939. Seattle: University of Washington Press.

Said, Edward. 1978. *Orientalism.* New York: Vintage.

Schroeter, Daniel J. 2002. *The Sultan's Jew: Morocco and the Sephardi World.* Stanford: Stanford University Press.

Shavit, Zeev. 2003. The Center for the Integration of the Sephardi and Oriental Jewish Heritage as a Social Agent in Israel – A Sociological Perspective. *Pe'amim*, 94–5: 95–903 (Hebrew).

Shryock, Andrew. 1997. *Nationalism and the Genealogical Imagination: Oral History and Textual Authority in Tribal Jordan.* Berkeley: University of California Press.

Slouschz, Nahum. 1927. *Travels in North Africa.* Philadelphia: Jewish Publication Society of America.

Urbach, Ephraim E. 2008. *War Journals: Diary of a Jewish Chaplain from Eretz Israel in the British Army*, 1942–1944. Israel: Ministry of Defence Publishing House (Hebrew).

Zenner, Walter P. 1963. Ambivalence and Self-Image among Oriental Jews in Israel. *Jewish Journal of Sociology*, 5: 214–23.

Zohar, Zvi. 2001. *The Luminous Face of the East.* Tel Aviv: Hakibbutz Hameuchad (Hebrew).

Chapter 8: Moving Fieldwork: Ethnographic Experiences in the Israeli-Palestinian space

Cédric Parizot

Introduction

This chapter discusses the effect of emotional involvement in the field and the effect of movement between conflicted worlds on ethnographic research. I base my analysis on my own life experience in Israel and the Occupied Palestinian Territories, where I conducted fieldworks between 1994 and 1996 and where I have lived continuously from January 1998 to November 2004.

I engage in this analysis as a way to pursue a discussion I started with Emanuel Marx when I first met him in 1999. Emanuel had invited me to his place for dinner. While I was telling him about the elections and local politics in the Negev, he recommended that I look at people's actions and choices as highly emotional. This I did, and it led me to examine my own behaviour and wandering in the field as emotional. It took me time to make sense of my findings, and this chapter is a first attempt to articulate these thoughts.

Emotions and movements were narrowly related in my own experience in the Israeli-Palestinian space. Between 1994 and 2004, I constantly moved from place to place. Each movement represented a displacement away from a universe in which I had established long-term emotional relations with people. Between 1994 and 1999 I stayed in the Negev where I lived mostly among my Bedouin hosts in the villages of 'Abde (Avdat in Hebrew) and the 'Arab al-'Ugbi as well as in the townships of Hûra and Rahat. The rest of the time, I remained in Sde Boqer Campus (Ben Gurion University), living in caravan accommodation while making regular visits to the Occupied Palestinian Territories (OPT). In autumn 1999, I moved to Tel Aviv for three years where I lived with my girlfriend. At that time, I kept on visiting my friends in the Negev and the OPT. In 2002 I moved back to the Negev to work as the head of the French Cultural Center in Beer Sheva. There my girlfriend and I parted. In 2003, I met Noa. We left Israel to go to France in 2004, and married there a year later.

Being caught up in emotional relations is a common experience for anthropologists doing long-term fieldwork and, as such, it has been broadly discussed in the literature (Matsunaga 2000). Scholars also discussed how ethnographic observation, description and understanding may be deepened by the sharing of emotional intensity (Favret-Saada 1990) and even by taking sides with the hosting community (Swedenburg 1995).

Following these approaches, I will demonstrate that, being both emotionally involved in the field and moving between conflicted realms, such as those prevailing in the Israeli-Palestinian space, can bring a privileged ethnographic perspective. I will show that, while I was attempting to overcome the many difficulties and obstacles such involvement and moves brought to me, I have acquired the practical tools and knowledge. What at first I regarded, as a sign of failure in my anthropological research, now appears as a methodological tool that allowed me to understand part of the mundane processes structuring boundaries and gaps in the intensely conflicted Israeli-Palestinian space.

In discussing my own experiences I do not intend to confess the mistakes I made during this period; these I prefer to keep them to myself. Rather, I see it as an exercise in ethnography; viewing the anthropologist as an object. Like Roger Goodman (2000), I strongly believe that looking at how a society affects the behaviour of the anthropologist can teach us a lot about that society and some of its internal dynamics.

I will discuss my travels from emotions to emotions during my stay in the Israeli-Palestinian space in three stages. First, I will describe the conditions in which I became completely caught up in emotional and power relations with my Bedouin hosts and friends in the Negev, to the extent that I lost any distance and critical understanding of what I was observing. Second, I will describe how these extremely situated perceptions strongly shaped my interactions in Jewish Israeli society when I moved to Tel Aviv. Yet, as I will explain in the third part, these apparent shortcomings were not marks of failure in my ethnographic project. On the contrary, after a long, hard and complex struggle I believe that I deepened my ethnographic insight. This process did not merely provide me with a privileged access to the local system of signifying practices based on the politics of emotions, but it also gave me practical experience of how emotions can mark out boundaries in spaces, and provoke their embodiment. In other words this process showed me that drowning in emotions, and the difficulties of movement it entails between Israeli and Palestinian, Jewish and Arab conflicted worlds, should have been set as a methodological target from the very beginning of my fieldwork.

Caught up in the Field

My successive fieldworks in the Negev between 1996 and 1999 were poorly prepared. From the subject I chose to the location of my fieldwork, I seized the opportunities that were presented to me. This poor preparation, together with my limited knowledge of the region and my status as a foreigner, placed me in a relation of extreme

dependency toward my hosts. Instead of just observing and collecting interviews, my ethnographic experience forced me to manage in challenging social and affective environments. I could not remain distant, discrete and neutral while observing my hosts. On the contrary, I had to earn their sympathy and their recognition. I had to enter emotional and long-term relationships, I had to let myself be caught up by the field and to be deeply affected by it.

Finding a Subject and a Place to Live

In January 1996, planning to immerse myself in the Bedouin population in the Negev, I looked for a place to live either in one of the planned townships or in the slums scattered on the periphery of Beer Sheva.[1] My original aim was to study patterns of urbanization in this population. Yet new general elections were due to be held on May 29, 1996 to choose the new Israeli Prime Minister and the Members of the Knesset (the Israeli Parliament). Gideon Kressel, who hosted me in Sde Boqer Campus (Ben Gurion University), and supervised the beginning of my fieldwork, advised me to seize the opportunity of the election campaign. I did not know much about electoral politics but the idea was exciting.

At the end of January Gideon put me in contact with Nûri al-'Ugbi (Abu Salâh) the head of a human rights NGO 'The Association for Defence of Bedouin Rights in Israel'. When I met Abu Salâh I explained that I was looking for a place to live in a Bedouin township, in order to learn Arabic and observe the electoral campaign. He offered to welcome me into his home provided that I helped in the association every week. I agreed to his generous offer, and followed him that same day to his village close to Hûra. When we arrived at his home, he introduced me to his brothers and to the other members of his family; I stayed with Abu Salâh for a few days. Subsequently, I was invited to be the guest of his brother, Abu Sâmi, who eventually became my permanent host. Abu Salâh, Abu Sâmi and their respective families, as well as the other members of the village, displayed great generosity. They offered me their homes and took me into their confidence for the next six months, following which I returned to France. When I came back to the Negev, in January 1998, they invited me to stay in their homes for another six months. In November, 1998 I moved to Rahat, in order to observe the municipal election campaign, and later the general elections of May 1999.

---

[1] By the mid-1990's, the 120,000 Bedouin living in the Negev constituted an urban proletariat settled on the periphery of Beer Sheva. Half of them resided in the planned townships that lacked basic infrastructure, basic health services, as well as industrial and commercial activities. The remainder lived in small villages that were built during the 1960's on the sites of what had become permanent encampments. Because these villages were left outside of the zoning of residential areas established by the state of Israel, they were condemned to illegality; their inhabitants cannot obtain permits to build houses. It is for this reason that these villages are called 'unrecognized villages'.

Earning Recognition

It was not easy to gain legitimacy and sufficient respectability among Abu Sâmi's and Abu Salâh's families. In 1996 when I started my fieldwork as a graduate student, I was 24, much younger than an average Israeli graduate student. My level of Arabic was that of a beginner, my Hebrew non-existent, and I had little knowledge of Israeli and Palestinian societies. These drawbacks prevented me from claiming any credibility.

My position was absurd. I had to find ways to redefine it. I had to acquire a better level of Arabic as well as to learn rules of proprieties and, of course, to get familiarized with the basic information concerning local and national politics. Thanks to the unlimited patience and generosity of my hosts, I managed to take my place among them and acquire sufficient skills to enable me to carry out my fieldwork investigation.

I never managed however to change the first impressions I made during my first stays: that of a young exotic curiosity constantly asking questions, whose relevance was to be determined. Nevertheless, later in November 1998 I did partially manage to change my status when I moved to Rahat and settled at Abu 'Isâm's house. This time I started my investigation with better knowledge and position. I was already quite fluent in Arabic, I was more familiar with local politics and I was no longer a graduate but a PhD student.

Yet, even in Rahat, I evoked amusement and even contempt among the local elites. Sometimes, I felt like the (Inspector) Colombo character! Although my knowledge would increase progressively to the point of mastering nuances of local politics, people still regarded me as stupid. I still remember interviews held in the city hall of Rahat which eventually turned into sessions in which I was totally overwhelmed and patronized by my interlocutors. My interviewees became the interviewers while I became a funny attraction, subjected to mockery and jokes. These experiences made me realize that if anthropological investigations can involve deep asymmetric relations, it is not always to the disadvantage of the people observed by the anthropologist. Contrary to what is often assumed (see, among others, Bourdieu 1998[1993]), symbolic violence does not always exercise itself on the interviewee but, in certain circumstances, it can also exercise itself on the interviewer (Chamboredon et al. 1994).

All in all, I was not only caught up in power relations, but the field started to gain control of my unfolding research. This tendency persisted because the very conditions of my investigation gave me no way to escape it.

Affective Relations, Affected Perceptions

Emotions became central to my relations with people; not only did I develop long term relations with them but I also compensated for my feeling of isolation from my country, family and friends by directly re-constructing affective relations in the Negev. Abu Sâmi, Abu Salâh and Abu 'Isâm, who were my elders, the ones who introduced

me into their world, and the ones on whom I was relying economically and socially, progressively became my mentors and their respective families my shelter.

Given such deep affective links with my hosts, I internalized their narratives with little criticism and these became the sole means by which I could make sense of the surrounding reality. In 1996, at the very beginning of my field work, not knowing much about anything, I grasped any story and narrative as I learned words to build up my Arabic vocabulary. I added these discourses to a more or less coherent patchwork that I was mentally sewing to make sense of the unknown surroundings. It is on such grounds that I tried to understand the position of the family of my hosts in relation to their neighbours, and to understand the content of the relations between my Bedouin hosts and the dominant Jewish Israeli society. These stories did not just make sense because they were enunciated by people I respected, and in whom I had total confidence, but also because they were loaded with emotion. Finally, reproducing such narratives was a way to unconsciously acknowledge the new affective yardsticks I acquired in the Negev. It was also a way to publicly claim my belonging to this new social and cultural environment whose recognition I was seeking.

I developed extremely situated perceptions. Locally I was not integrating myself into the Bedouin society of the Negev but into the specific families and networks of my hosts; I made their points of view my own. In November 1998, during the municipal elections of Rahat, I actively identified with the electoral struggle of the party led by the cousin of my host.

On a broader level, I sided with my hosts and the Palestinians in their conflict with Israel. Living in the village of Abu Salâh, in the slum of 'Abde and in the planned township of Rahat, I shared their difficult material conditions of life, I witnessed and even found myself directly exposed to the violence of the Israeli police. My regular trips to the OPT also made me see the violence of the Israeli occupation. These experiences provided me with many incidents to illustrate the discourse of my Bedouin and Palestinian hosts about the toughness of their relations with the dominant Israeli society. Each incident contributed to better ground my narratives and perceptions of the general situation. Finally, as it happened to Clifford Geertz and his wife (Geertz 1973) when they run away from the Balinese police with the local inhabitant during cock fight, I found out that sharing experiences of violence helped me develop mutual feelings of closeness with my hosts in Abu Salâh's village, in 'Abde and in Rahat. It took me a long time to realize this, and to adopt a critical attitude toward these perceptions.

Displacements in Conflicting Worlds

Living in Tel Aviv did not take me through a process of detachment from the field, but through the confrontation between the perceptions I had acquired during my fieldwork and those I developed in my new environment.

Surprises

For a little while, I really thought that living in Tel Aviv would free me from the emotional and social challenges that I experienced in the field. I was planning to write my PhD and to start a new life. The atmosphere of the city, the rhythm of my new existence, as well as my daily activities made me feel like I was back in Paris. Moreover, living with my girlfriend whom I had met a few months earlier, as well as developing long-term relations with her family and friends, introduced me to a whole new universe of relations, emotions and perceptions. I had put an end to my fieldwork, and had stopped trying to observe and decipher the reality around me.

Yet, I could not forget what I had seen and experienced in the Negev and in the OPT. These past experiences made it extremely difficult to relate to the part of the Jewish Israeli society I had met without thinking of the Israeli-Palestinian conflict and the Jewish-Arab relations inside Israel. I became obsessed with the gaps I noticed between the life I was now enjoying and the one I had experienced; I even developed resentment towards the dominant Jewish society. Significantly, despite going regularly to the Ulpan[2] as well as being surrounded by the language, for a long time I did not manage to speak Hebrew.

This resentment intensified in the face of the gaps of knowledge separating Israeli Jews from the Palestinians. Apart from my girlfriend and some colleagues who where themselves involved with Palestinians through their work, most of my Jewish Israeli friends had absolutely no idea of what was happening in what they called the 'shtakhim'[3] and even in places populated by Israeli Palestinians. It seemed to me that they were not even aware of their Palestinian neighbours. Their lack of awareness was proportional to their surprise in September 2000, when the second Intifada broke out. Many of my Jewish friends thought that the period of the implementation of the Oslo Agreements (1993–2000) had brought dynamics of peace and prosperity. They did not realize that during that same period Israeli policies of control concretely imposed on Palestinians more obstacles of movement, a harsh economic crisis, and the constant shrinking of space.

Surprisingly, I noticed even more ignorance among Israeli Jews concerning the Bedouin in the Negev; not a single friend of mine was aware of what was happening in the south. Some had more knowledge about the Palestinian Territories than about the Bedouin. Actually, none had ever entered a Bedouin township. This explained the odd questions they sometimes asked, such as how hard it was to sleep in tents, and whether the 'traditional' Bedouin coffee they heard so much about was really that good!

---

[2] Hebrew language courses organized for new immigrants.

[3] Literally, this term means 'The territories'. Originally it was used to describe the West Bank and Gaza occupied by Israel after 1967. Yet since the implementation of the Oslo Agreements (1993) the multiplication of checkpoints, the building of bypass roads and the regular closures of Palestinian enclaves in these territories, the word 'shtakhim' tends to designate solely the shrinking Palestinian enclaves located beyond the checkpoints, trenches and walls build by Israel around the Gaza Strip and the West Bank.

Given the degree of interconnectedness between the populations and the short distances between populated areas, it was very difficult for me to understand the lack of knowledge Israeli Jews had about their Palestinian neighbours in the OPT or within Israel.

Painful Bridging

Yet, I soon experienced myself the difficulty of moving from one world to another. During this period, I encountered difficulties in moving back and forth between Tel Aviv and the Negev and between Tel Aviv and the OPT. It confronted me with the extent of ethnic inequalities and discrimination prevailing in Israel, as well as with the increasing damage inflicted by the Israeli repression of the Second Intifada and created unease. It was all the more difficult to get rid of this unease as I could hardly share it with my close Jewish Israeli friends. And for good reason, in an antagonistic situation nobody can address affects or issues related to the conflict in a neutral way. Claiming 'neutrality' or even worse 'objectivity' as do many westerners coming to Israel/Palestine creates more irritation than it appeases interlocutors. Not taking side is often perceived as being against. Jeanne FavretSaada (1985) explains that antagonistic situations allow no margin for pretending to be a neutral observer. In situations of conflict where people are personally and deeply affected, refusing to take sides can be considered as questioning the whole seriousness and consistency of their suffering. Thus, in Tel Aviv voicing concerns for the 'other side' was often perceived as criticism or, worse, confrontation.

I became aware of this difficulty in communication through a series of violent exchanges following the outbreak of the second Palestinian uprising in September 2000. One such incident happened about a month into the uprising. I remember I was sitting in a café in Tel Aviv with a couple of close friends and they started talking about a clash in the region of Nablus between the Israeli army and Palestinians. The TV news had reported many Palestinians dead after the soldiers opened fire on a crowd trying to capture an army post. We were all deeply worried about the general escalation of violence. Considering the amount of passion, anxiety and revolt that we all felt about what was going on, the discussion quickly flared up. My friends voiced an extremely harsh and hateful discourse against Palestinians. Similarly moved by the debate, I protested and strongly criticized the attitude of the Israeli army. Eventually, I stopped arguing before the discussion became too violent. Feeling that I could not confront them without risking falling out with all of them, I withdrew from my positions and excused myself, admitting that I went too far. Yet, my friends were already shocked by my words. A few days later one of them told me that he was profoundly disappointed by the fact that I systematically pleaded the cause of the Palestinians every time we met. He added that as a foreigner, I had no legitimacy to do so whatsoever. I could not tell him that I had been myself hurt by his words. If I wanted to keep their friendship, I had to surrender to their terms. Needless to say, these incidents were terribly humili-

ating for me. They questioned the very affective relations I was trying to develop with my Jewish Israeli friends. Such difficulty in speaking out created a permanent unease during my stay there. Indeed, but for a few exceptions, I found myself in a situation where I could not voice my frustrations caused by the conflict with the very people I regarded as potential confidants.

During the same period, interactions with my Palestinian friends, in the OPT as well as in Israel also became increasingly difficult. Being with a Jewish Israeli girlfriend changed the nature of my relationship with many friends. While it was hard to tell exactly what these people felt about it, it obviously modified in their eyes my position in relation to them and to the dominant Jewish Israeli society. I felt it in the half-joking remarks my Palestinians friends would make, such as 'so you finally decided to join the enemy?' Others would indirectly ask me to explain what they considered a weird choice on my part.

I also experienced some difficulties exchanging points of view on the ongoing conflict with Palestinian friends. Having myself experienced or witnessed to some extent the violence my friends were submitted to, I could definitely understand their anger and hate towards Israel. However, as I was now living in the heart of Jewish Israeli society, I could not relate to these expressions of anger and hate as I did before. First, their anger targeted what I no longer saw as a monolithic, dominant society alien to myself. Second, the anger and the hate of my Palestinian friends were aimed at people that I could now identify with faces, and among whom I also counted friends that I cherished and loved. Yet, fearing confrontations, I could not voice my reservations.

Once I started living in Tel Aviv, dealing with the issue of the conflict or its ramifications became increasingly painful. I felt in permanent discomfort for I could not remain neutral, but at the same time I could not take sides.

I started to envy my friends who were working on their research exclusively with Palestinians in the OPT or with Jews inside Israel. They could take sides, a comfortable option I no longer had. In all, I could neither bridge the two worlds nor remain exclusively in one of them.

Selecting Encounters, Avoiding Conflicts

Facing such conflicted realities and worlds, I unconsciously selected my encounters and restricted my movements. Between the years 2000–2002, I reduced the number of visits to Ramallah and the Negev, and instead I stayed almost exclusively in Tel Aviv. At the beginning I attributed this attitude both to the outbreak of the second Palestinian uprising and to the stability I felt I needed in order to write my PhD. In point of fact, I limited both my movement and my encounters in order to avoid any direct or indirect confrontations between opposing realms.

I became aware of this attitude much later after moving back to the Negev in 2002. In September of that year, I rented a house in Beer Sheva where I was to start working as head of the French Cultural Center. I looked forward to meeting my old friends

as my move to the Negev corresponded to sharp ruptures in my life, not the least of which was my recent break- up with my girlfriend. Losing her and her family, I had to reconstitute a new affective network. At that point I realized the extent of the rupture I had made with my friends in the Negev and in the OPT during the years I lived in Tel Aviv; it seemed that I had lost many of my friends. Luckily the few I had retained welcomed me back with warmth that enabled me to get back onto my feet.

Emotions, Movements and the Grasping of Limits

The successive moves I made between 1996 and 2004 brought me through different and conflicting worlds of relations and emotions. I lost my detachment and forgot the posture of 'The Researcher'. Yet, such a process did not mean that I had failed in my ethnographic research. On the contrary, it provided me with a certain depth of understanding that I could not have reached if I had maintained controlled detachment. First, sharing emotions in the field opens up a unique access to local systems of signifying practices. Second, the confrontations within conflicted realms are a privileged way to fulfil the process of detachment from the field, as well as to understand mundane and daily mechanisms of boundary markings.

Sharing Emotions, Deepening Ethnographic Sight

Caught up in long term emotional and power relations, my daily experience became increasingly mediated by the sharing of emotions with my friends. With time and with accumulation of experience, I developed a repertoire of common signifying practices with my hosts that allowed me a better understanding of local politics of emotions.

The emotional mediation of my daily encounters started with the sharing of unrepresented emotions. According to Jeanne Favret-Saada (1990: 6), sharing unrepresented emotions constitutes a mean of communication that goes beyond empathy. She suggests that experiencing empathy is to vicariously experience the feelings, thoughts and perceptions of another. Such definition assumes a distance. A second definition she considers involves the idea of a greater closeness, and identification with the subject experiencing the feelings. Yet in both definitions of empathy the anthropologist does not share or directly experience these feelings. He can only project himself and his categories onto the feelings of the other. In contrast, according to Favret-Saada, being affected involves another kind of communication. What are exchanged and communicated are not the feelings and the images attached to them; rather, it is the intensity of the feelings provoked by being in a same place, a same position and a same situation.

I would like to elaborate on this last point based on an experience I shared in 1996. One morning, I intended to take a bus in the Tel Aviv central bus station to go back to Beer Sheva with Hâj 'Awûde Abu Srîhân. Hâj 'Awude, was one of the people I most respected among my acquaintances and from whom I might have learned the most.

The sixty-year-old-man was a respected notable of the town of Tell as-Sabaʻ and one of the founders of the Association for the Defence of Bedouin Rights in Israel.

That morning, he offered to pay my bus fare to Beer Sheva and I protested vehemently. My protest was definitely a performance; how could I dare to seriously contest a respectable old man like him? I was half serious while walking around the Hâj who pretended to ignore me. Suddenly I felt a hand on my shoulder. I stopped in my steps and turned to discover three young security guards in their 20s who quickly surrounded us. One of them asked in Hebrew 'Is everything alright here?'; and before we could answer a word, he added with a severe tone in a broken Arabic tainted with a strong Israeli accent: 'Give ID!'. Still surprised we both complied. Then, in a patronizing and contemptuous tone, one of the guards told the Hâj: 'What are you doing here? You have nothing to do here in Tel Aviv! Go back to your place!' We took back our papers and moved away. In the bus, I still felt revolted by this guard who dared to publicly insult in an outrageous and insolent way a man that I regarded both as a reference and a shelter. It was hard for me to judge the extent to which the Hâj was affected. Yet, I could easily feel his embarrassment as he preferred not to speak about this incident any further.

On the one hand, I was not able to tell what images, words and other affects the Hâj paired with his specific emotional reaction. On the other hand, we did exchange a certain intensity of affect, even if this intensity was different considering our distinct position in relation to the guards, and to the context. These affects, although not represented by words as we both kept silent, were at the heart of the process of communication between Hâj ʻAwude and me. In my definition I do not regard emotions through an individualistic approach that would locate them inside the individual; rather, I apprehend them as generated in the very interactions taking place between people (Myers 1988: 603; Papataxiarchis 1994: 6). As Vincent Crapanzano (1994: 116) explains, the expression of emotions and their qualification can practically define the context of enunciation and the respective positions of the interlocutors.

The accumulation of shared unrepresented affects provoked by specific encounters and contexts helped me eventually to build a common repertoire of emotions with my hosts. I managed to progressively adjust my own repertoire of emotions to theirs. I strongly believe that the expression of emotions and their meanings can be adjusted, because I regard emotions as signifiers 'in a system of signifying practices [and their meaning as] produced through [their] use in social life and [their] relationship to other signs' (Myers 1988: 606–607). I do not mean here that I paired my repertoires of emotions to those of my hosts and that it allowed me to feel and experience situations as they did without entailing any projections drawn from my own life. Rather, I consider that our shared experiences provoked an adjustment of systems of signifying practices that allowed me to better express and decipher feelings as well as to better understand and deal with local 'politics of emotions' (Crapanzano 1994).

Moving from Affect to Effect, a Practical Experience of Boundaries

In order to profit from such an adjustment of emotional repertoires, I had to take a detached and reflective posture. Yet, given my degree of emotional involvement, I could not easily and quickly undergo this kind of a change. It is chimerical to think that anthropologists can shift instantly from the position of 'participant' to that of 'observer' and vice-versa. It was through a long, hard and complex process that I managed to adopt a reflective posture. Paradoxically, this process was mainly triggered by the emotional confrontations I experienced during my moves from one conflicted world to another. On the one hand, they enabled me to better grasp parts of the land marking practices that people develop; and on the other hand, they helped me apprehend the ways by which each side would distinctly interpret boundaries, practices and events. In order to avoid confrontations between friends, information and places, I had to develop my own defences and strategies. During the time I spent living in Tel Aviv and later in Beer Sheva, I spontaneously began to compartmentalize my life by selecting and ordering my encounters and movements. In other words, my experience of the boundaries and the gaps fragmenting the Israeli-Palestinian space was also very much practical. I had to make sense of them in order to better circumvent them and to avoid unpleasant confrontations.

This process of practical learning was deeply influenced by my friends, and by the encounters that I had unconsciously imitated or actively consulted. Recently, during one March evening in 2006, I was sitting in Beer Sheva in the house of two Israeli Palestinian friends. We discussed how difficult it was for us to express our thoughts with our Jewish Israeli friends. It was clear that their position of constant reservation when speaking with Israeli Jews, much like my own, was not motivated by any kind of hypocrisy. Rather their aim, as well as mine, was above all to maintain the equilibrium of our affective surroundings.

Moreover, during the second Intifada I discovered that I was not the only one to compartmentalize my life to the point of reducing the scope of my movements. Some of my friends living in the Negev had considerably reduced their visits to their family in the West Bank. This was less due to the obstacles of movements created by the tightening of the closure policy, and more because paying such visits would involve uncomfortable confrontations. Some Bedouin felt as if they were increasingly questioned about the content of their relations with the Israelis by their Palestinian kin and friends. Others preferred to avoid witnessing the effect of Israeli repression on the life of their kin and neighbours. Finally, the reinforcement of closures, the economic crisis it provoked in the OPT, and the border economy (Parizot 2006) that developed between the Northern Negev and the Southern West Bank, rendered the content of these relations between local Bedouin and West Bank population gradually more problematic. The growing gaps in power relations between these two related

societies, that manifested themselves in economic and social exchanges, tainted the content of cross-border relations with an unpleasant and at times violent dimension.

Gaps have even broadened as some people started to refuse hearing about the other side. Once, in April 2005, I went to perform a short ethnographic enquiry in the border village of Samû' in the southern West Bank. I had never realized before this time to which extent small Palestinian villages were deeply affected by the Israeli repression and closure policies. It had nothing to be compared with Ramallah not even with the Bedouin slums of the periphery of Beer Sheva. Yet, when I left Samû' and went immediately to Hûra in order to visit friends, I encountered walls of misunderstanding. My friends refused to hear about the misery of their neighbours of Samû'. Their refusal to let me voice and share my frustration and revolt was actually not much different than some of my Jewish Israeli friends that I met few days after.

Learning how to practically manage and avoid confrontations eventually brought me to adopt a series of invisible and mundane mechanisms that were widely spread among my friends. I could also better sense the power of such land marking in maintaining boundaries between populations in the Israeli-Palestinian space. Selecting friends and acquaintances in my networks, orientating my trajectories in spaces, in order to avoid unpleasant confrontations, my affects helped me organize the compartmentalization of my work and life. They create what could be called unrepresented 'land marking' (*bornage*) of boundaries (de Certeau 1990: 180–81).

These invisible emotional land marks can be all the more powerful and more airtight than any wall, for they do not solely set and maintain boundaries between groups of people; they also mark the passage of political-administrative borders deep inside the body. By pairing emotions with physical and social landscapes, as well as emotions with past experience associated with particular space, these land markings can provoke the embodiment of borders on a similar mode to that of the internalization of constraints. I witnessed this on my behaviour or that of my friends with whom I moved from Israel to the OPT or vice versa. People who happened to ride in collective Palestinian taxis along West Bank roads know how much getting closer or getting away from an Israeli checkpoint affect differently the whole behaviour and emotional state of the passengers. This land marking can also operate in areas where the border is hardly defined by specific points in the space. Rather, it is the architecture, the people, the sounds, the words or the accents that would notify the passage and provoke a specific emotional reaction.

All in all, the difficulty of movement that I experienced in the Israeli-Palestinian space cannot be associated with a failure of my ethnographic project. On the contrary, the constant confrontation of conflicting perceptions and affects that I developed in these conflicted worlds was actually the key to activating a process of extraction from the field, and to setting in motion a process of reflectivity. Moreover, by managing the difficult passages between these worlds and affects, I learnt in practical terms mundane and invisible mechanism of marking boundaries. Hence I think that in long-term

fieldwork in conflicted contexts, drowning in emotions and moving between opposing worlds could be set as a methodological tool.

Conclusion

The place that emotions have taken in my fieldwork experience was linked to the conditions in which I carried out my research. Yet, my experience was not exceptional: emotional relations are to be expected by any researcher performing a long term fieldwork.

On the one hand, emotional involvement in fieldwork has its flaws and limitations: emotions mediate and influence our perceptions of reality; they render the exit of the field difficult. It shows that 'participant observation' is an oxymoron built on the positivist illusion that the researcher can shift, instantly and as he wishes, from the position of a distant observer to that of an immersed participant and vice versa (Favret-Saada 1990).

On the other hand, emotional involvement brings many advantages. It is an excellent mean by which one can better one's ethnographic sight and ways of communication beyond the level of empathy. It helps coming closer to local signifying practices, such as those involving the politics of sentiments. Moreover, emotional attachment coupled with movements between conflicted worlds allows unique experiences of the extent of gaps and of the degree of confrontation between these very worlds, as well as of the difficulty to bridge them. However, our experiences as anthropologists are never exactly that of the members of the societies we study. Because of our own life experience, because of our own projections, and because we are entangled between opposing groups, we are forced to remain in constant movements between them.

This perpetual movement in between conflicted worlds prevents the anthropologist from 'turning native'. Instead, it places him slightly aside of the experiences of the people he works with, but close enough in order to grasp the point of view of these actors. Moreover, the position given by this specific movement provides a different angle of approach from that of objectivism. Objectivism constructs a specific perspective and relation to reality in which the researcher withdraws himself from action in order to observe it from afar and from above (Bourdieu 2000 [1972], 229). Yet, this distance is often a given position resulting from being a stranger and having little practice and knowledge of reality. According to Bourdieu (ibid.), it is often a bias turned into a methodological claim. Michel De Certeau (1990, 140–141) regards this belief of being an external seeing point as the fiction of knowledge. Furthermore, he shows to what extent this position has the disadvantage of cutting the observer from the actual experience of the actors. Throughout their mundane and everyday practices, actors do not see spaces from afar, but they experience them through their journeys and their routes. Their knowledge is not built on observations, but it is a knowledge as blind as a 'love clinch' (*corps à corps amoureux*) (De Certeau 1990, 141).

Consequently, I consider that both emotion and movement allow the anthropologist to develop a subtle kind of detachment from the practices of people: a detachment that, in conflicted realities, can provide a much better angle of observation than that produced by distant and illusory 'neutral' or 'objective' observation.

Acknowledgements

References

Bourdieu, Pierre. 1998. Comprendre, in *La misère du monde*, edited by P. Bourdieu. Paris: Seuil, Points: 1389–1447.

——. 2000 [1972]. *Esquisse d'une théorie de la pratique*. Paris: Seuil.

Certeau, Michel de. 1990. *L'invention du quotidien. 1. Arts de faire*. Paris: Gallimard Folio essais.

Chamboredon, Hélène, Pavis, Fabienne, Surchez, Muriel, and Willemez, Laurent. 1994. S'imposer aux imposants. *Genèses*, 16: 114–32.

Crapanzano, Vincent. 1994. Réflexion sur une anthropologie des émotions. *Terrain*, 22 (mars): 109–17.

Favret-Saada, Jeanne. 1985. *Les mots, la mort, les sorts*. Paris: Gallimard Folio essais.

——. 1990. Être Affectée. *Gradhiva*, 9: 3–9.

Geertz, Clifford. 1973. *The Interpretation of Culture*. New York: Basic Books.

Goodman, Roger. 2000. Fieldwork and Reflexivity: Thoughts from the Anthropology of Japan, in Anthropologists in a Wider World, in *Methodology and History in Anthropology*, vol. 7, edited by P. Dresch et al. New York, Oxford: Berghahn, 151–65.

Matsunaga, Louella. 2000. Recollections of Life Crisis: Distancing the Personal, in Anthropologists in a Wider World, in *Methodology and History in Anthropology*, vol. 7, edited by P. Dresch et al. New York, Oxford: Berghahn, 167–85.

Myers, Fred R. 1988. The Logic and Meaning of Anger among Pintupi Aborigenes. *Man*, 23: 589–610.

Papataxiarchis, Evthymios. 1994. Les émotions en Grèce égéenne. *Terrain*, 22: 5–20.

Parizot, Cédric. 2006. En attendant le mur: Gestions israéliennes des mobilités palestiniennes pendant la seconde Intifada (2000–2006). *Migrations Société*, 18 (107): 15–39.

Swedenburg, Ted. 1995. Prisoners of Love: With Genet in the Palestinian Field, in *Fieldwork under Fire: Contemporary Studies of Violence and Survival*, edited by C. Nordstrom and A.C.G.M. Robben. Berkely, Los Angeles, London: UCP, 25–40.

Part III: The Journey

Chapter 9: Spiritual Travels, Social Knowledge: Entering the Space of Tuareg Possession

Susan Rasmussen

Introduction: Bridging Self and Other, Knowledge and Belief during Spirit Possession in a Semi-Nomadic Community

Possession, Nomadism, and Anthropological Knowledge

In travel,—whether shamanic, nomadic, or anthropological—there are always surprises around the corner. During the ethnographic quest of anthropological travel, the power of the unexpected and unintentional in knowledge construction should not be underestimated. Some cultural "idioms"—in particular, spirit quests—are enigmatic because they are not usually experienced directly by the researcher in his/her home cultural setting (Crapanzano and Garrison 1978; Boddy 1987; Lambek 1981; Rasmussen 1995, 2001a, 2006). Spirit possession exorcism and other related medico-rituals are not easily accessible to the researcher's own analytic categories, except through the paradigms of anthropological theory— canons now questioned in some post-structural and postmodern efforts to deconstruct and recast concepts into terms more conducive to local knowledge constructions (Good 1994; Moore 1996; Stoller 1989). This essay discusses additional insights into possession gained from an instance of unexpected and unintended catapulting of this researcher into the role of patient, analyzing its dual significance for both local residents and participating researcher, through the histories they brought to that experience. The essay also explores parallels between spirit travel, ethnographic interpretation, and local semi-nomadism in Tamajaq-speaking Tuareg communities of northern Niger.[1]

---

[1] The Tuareg (Kel Tamajaq) speak Tamajaq, a Berber (Amazigh) language. See Ag Soliman (1996) and Ag Erless (1999). Until French colonization in the early twentieth century, these noble descent groups, predominantly nomadic, enjoyed military and economic dominance over subordinate tributary, client, and servile groups, some nomadic, some more sedentarized on oases. Social status was based

Possession involves spiritual travel, and the study of possession, similarly, involves (indeed, requires) a kind of intellectual nomadism on the part of the anthropologist: one must resist the temptation of making a false opposition between the local belief or idiom and the assumed "knowledge/truth" of anthropological theory (Good 1994). Possession adepts and anthropologists need to become, at least to some extent, "kindred spirits" (Boddy 1987). In spirit possession, there is travel to the spirit world. In nomadic transhumance and ethnographic field research, there is also travel to an often distant place. This essay probes intersections among these experiences, in mutual constructions of knowledge, through examining a case of possession arranged by this researcher, which was contrary to the researcher's usual practice, of waiting for such rituals to occur in context. My purpose in arranging this ritual on that occasion was to video-tape it, the trance dance motion in particular. Local residents, however, had a different purpose: to heal me (to exorcise me of spirits of the wild)—that they believed to actually possess me. Yet as this essay shows, also important here was both my own self-psycho-analysis and their healing of not solely myself, but also themselves—from stresses and conflicts which many Tuareg believe provoke wandering, nomad-like, following the calls of spirits.

In this ritual, local participants—most long-term adepts at possession, were well-known to the researcher as friends as well as consultants, assistants, and sometime hosts over a period of approximately twenty-five years of field research in a predominantly semi-nomadic, rural Tuareg community in northern Niger. We all traveled to a space called "the wild" (*essuf*, also denoting solitude or nostalgia).[2] Although my own participant-observation had certain limits, discussed presently, nonetheless new insights into the world of possession emerged from my unexpected and unintended role as patient. The present essay reflects upon what knowledge is gained, and what are its limits, in this perspective.

The goal here is neither the old confessional ethnographic narrative nor a literally-dyadic conception of dialogic critique. I examine not solely my own, but also local residents' perspectives and predicaments, the ritual process and its sung poetry, and the wider historical, social, economic, and political contexts. I analyze what this case reveals about the Tuareg spirit cosmos in relation to their nomadic traditions, as well as how it enriches the researcher's understanding of spirit possession over many years of return visits to the field. Tuareg spirit travel is constructed metaphorically upon an important cosmological concept: a geographic, emotional, and symbolic space locally called the wild, solitude, or nostalgia—in the Tamajaq language, *essuf*, which extends beyond the tent and camp. This space is dangerous, but also a space of creativity and growth.

upon descent, and roughly corresponded to occupational specialization. But nowadays, increasingly, there is lack of coincidence between prestigious descent and socioeconomic prosperity. See Nicolaisen (1961, 1997), Keenan (1978), Bernus (1981), Claudot-Hawad (1996), and Boilley (1999).

[2] The concept of *essuf* refers to both a physical place and a more abstract state of mind, as in psychological nomadizing in poetry (Penel 1991) and metaphorical travels (Claudot-Hawad 2002).

Nomadic "travel" to the spirit world is often considered metaphorical by outsiders, but is taken quite literally by local residents. Despite their varying theoretical frameworks, many rich works on shamanism, possession, and mediumship have long noted the importance of journey and enlightenment (Siikala and Hoppal 1992; Boddy 1987; Lambek 1981; Rasmussen 1995; Stoller 1989; Winkelman 1999, 2003), as have studies of "sorcery" and healing apprenticeships, with researcher participating as a healer (Peters 1981; Olkes and Stoller 1987; Turner 1997). Less explored has been the idea of researcher as the subject of study by local residents, for example, in aging (Rasmussen 1997), and as the patient of local specialists, such as herbalists and diviners (Rasmussen 2001a, 2006). Also needed are juxtapositions of local residents' and researcher's images and meanings of spirit travels, analyzing where these alternatively converge, intersect, and diverge.

Travel to the possession space by the researcher encourages processes I term spiritual and intellectual nomadism, but this does not necessarily provide the definitive meaning of possession. Rather, I argue, the contrasting and similar meanings of becoming possessed here act pluralistically as alternate registers of the spirit possession idiom, and, intertwined, enhance understanding of possession. More broadly, I propose this more "nomadic" or transhumant travelling between meanings in alternation as a middle-ground solution to wider theoretical debates in medical anthropology, anthropology of religion, and general cultural theory concerning the problem of "truth" vs. belief and authoritative knowledge systems (Good 1994).[3] Hopefully, this analysis balances hierarchical relationships between researcher's and local residents' idioms, "truth," and belief, and contributes to efforts to mediate opposition of rationality and emotions (Rosaldo 1989; Lutz and Abu-Lughod 1990), and to studies of ethnographic intersubjectivity (ReedDanahay 1997).

Ethnographic and Theoretical Connections

The Tuareg ritual exorcism context offers a rich perspective from which to explore anthropologist's and local residents' symbolic as well as literal nomadism; for geographic and spiritual nomadism are explicitly connected in local cultural symbolic parallels between nomadic wandering in the desert, emotional transformation in healing rituals, and transformations in poetic creativity and life course transitions.

Most Tuareg adhere to Islam, are socially-stratified, and live by semi-nomadic livestock herding, oasis gardening, caravan and other itinerant trade, artisan work, and la-

---

[3] Here, purpose is to probe the problem articulated by Byron Good (1994), of how in western traditions of rationality, anthropologists and scientists more generally have separated "belief" from "truth." In spirit possession and shamanism, this assumption has often meant that occidental theories and paradigms tend to crowd out other locally-grounded epistemological grounds of possession (Turner 1997; Stoller 1989). I favor emphasizing mutual constructions of knowledge in a "middle-ground" here. My strategy is to bridge, as well as compare or contrast.

bor migration.[4] Herding and caravans have declined, though not ceased, from droughts, wars, and colonial and post-colonial policies favoring sedentarization, farming, and rigid state boundaries. Gathering, once a food source, remains a significant source of medicinal herbs for many healers who gather them in *essuf*, wilderness locations outside villages and camps believed inhabited by spirits. Certain conditions, such as loneliness, depression, and nostalgia, make persons more vulnerable to the call of these spirits of the wild, called Kel Essuf.

The Tuareg or Kel Tamajaq are marginalized, far from the centers of official decision-making, in those nations where they reside: Niger, Mali, Algeria, Libya, and Burkina Faso. Under French colonialism, there was uneven economic development of the different regions of these countries (Charlick 1990). Until recently, the Tamajaq language and the Tuareg peoples were underrepresented in higher education, jobs, and governments of their national infrastructures. Schools and clinics were initially feared, associated with censuses, taxation, and forced sedentarization (Keenan 1978, Bernus 1981; Claudot-Hawad 1996; Rasmussen 1997, 2001a). Economic aid programs ignored traditional nomadic ecological techniques and cultural values (Childs and Chelala 1994).

The leaders of the 1990–96 Tuareg armed separatist rebellion against the central governments of Mali and Niger made a number of demands: compensation for property loss and political violence; regional and cultural semi-autonomy; and greater integration of Tamajaq-speakers into the army and the university. Since the 1995 and 1996 peace accords, governments have attempted to implement these goals, but with limited national budgets. Sporadic resurgences of conflict continue in some regions. The United Nations and international NGO agencies are compensating nomads for lost livestock, and providing training in alternative occupations (Boilley 1999; Rasmussen 2006). Tamajaq-speaking intellectuals and politicians have urged all Tuareg to broaden allegiances beyond the traditional social hierarchy of nobles, tributaries, smiths, and (former) slaves, and embrace a wider identity based on their common language (Keenan 1978; Bernus 1981; Nicolaisen 1997).[5]

There are now accelerating, ever-expanding cultural encounters in exile, labor migration, and itinerant trading of art objects. Yet much rural life in small villages and camps, while impacted by these processes, remains focused around kin, social stratum groupings, spirits, and ancestors. As among many other peoples (Siikala and Hoppal 1992; Turner 1997), among the Tuareg, the soul (*iman*) is believed to wander and travel over domains during altered states such as possession, dreaming, and even ordinary travel, and respond to calls of the Kel Essuf spirits. Essuf, a place of solitude (desolation), outside the community of social mores and ties, is where the soul of a possessed person travels. It is also a literal remote space, "the wild," and more recently, also has

---

[4] In the past, most occupations were inherited. Nowadays, many persons of diverse origins practice mixed subsistence patterns. Slaves were manumitted by the French early in the twentieth century; slavery was officially abolished in Mali and Niger at mid-century upon these countries' independence.

[5] There was some mobility for servile persons in fictive kinship with owners. See Nicolaisen (1997).

the sense of "nostalgia" (in French, *la nostalgie*) in some modern poems (Hawad 1979, 1985, 1987).

The Tende n Goumaten Spirit Possession Ritual

Possession involves psychic spiritual wandering (Hawad 1979; Rasmussen 1995, 2001b). The public possession ceremonies are called *tende n goumaten*. They are held for persons with afflictions that do not respond to Qur'anic verses, although this ritual is often preceded and followed by more Qur'anic verse treatments and seclusion with an Islamic scholar (*aneslem* or marabout). Those who undergo the *tende n goumaten* are not usually mediumistic diviner/healers; the latter have a long-term contract with a tutelary spirit. Only a few possessed persons ever achieve such a position, since this requires regular sacrifices to these spirits and attraction of a large, trusting human clientele (Nicolaisen 1961; Rasmussen 2001a, 2006).[6] Trance in the *tende n goumaten* rituals is described as "turning around of the head," expressed in the possessed person's trance dance, called the "head dance," which is performed once the possessed has risen from a prone position beneath a blanket to a seated position. Regardless of whom this ritual is initially organized for, other participants may also begin this dance spontaneously during the ritual, and may also become possessed. In the *tende n goumaten*, the goal is exorcism, and the cure consists of music (drumming with female solo and choral singing) believed to please the spirits of the wild or solitude. Audience members include both sexes, who participate through conversation, joking, and social commentary in group therapy.

Many possessed persons (some singers/curers are adepts who become recurrently possessed) inherit their spirits from their mothers' milk. Predominantly women undergo this ritual. Despite some gender-related conflicts, nonetheless this is not a clear-cut case of "female deprivation" or "sex warfare" (Rasmussen 1995). For most Tuareg women enjoy high social prestige and independent propertyownership (Worley 1992). These resources are threatened, however, by ecological, economic, and political crises and pressures to sedentarize (Oxby 1990).

The possessed person, called *gouma*, is said to be "in solitude (or the wild)" or, alternatively, "the wild or solitude is in him/her." In contrast to some other African spirit pantheons, Tuareg spirits are not highly individualized. However, there are hints that spirits possess humans of the opposite sex; for example, spirits possessing the women who undergo the public possession ritual exorcism are widely described as masculine: "They like women," local women and men explain, "in the (same) way that men like women in human society: for their perfume, beauty, fine clothes, etc." (Rasmussen 1995: 129). Irrespective of precise gender and sexuality, many spirits are matrilineal, for there are maternal images throughout many song verses: for example, allusions to

---

[6] There are several types of mediumship/counseling and specialists. Islamic scholars' divination is based upon the Qur'an. Diviners locally-defined as non-Qur'anic must accommodate their activities to Islam. See Rasmussen 2001a, 2006.

medicinal tree spirits, female-owned tents, and twins (Rasmussen 2001b), and spirit inheritance through one's mother's milk. There are also nuptial images: in the ritual seclusion of the possessed person, and in her initially- prone position beneath a blanket, both also actions of the bride at the wedding. Many Tuareg indicate that women's and men's spirits are not the same, explaining, "the former 'illnesses of the heart and soul' prefer noisy (musical) rituals as medicine, the latter 'illnesses of God' prefer the Qur'an" (Rasmussen 1995:11). Depression (*tamazai*), disruptions in usual routines or relationships (*anoughou*), and unrequited love (*tarama*) bring spirit afflictions into the open, when the soul (*iman*) wonders.

Despite its desolation, *essuf* is also a space for creative reflection and understanding. These spirit spaces are not always diametrically opposed to social spaces of the tent, camp, and village. Others besides the possessed may also venture into this space: in nomadism and other travel to remote places, during transitions, and while composing poetry and song—though not without risk of falling into an abyss of emotional desolation and susceptibility to permanent spirit affliction. Both the very young and very old are especially close to spirits (Rasmussen 1997). Babies are in danger of being pulled back into the spirit world (Casajus 1987). In order to mature into adults, adolescents must explore and master the unknown space beyond home (Claudot-Hawad 2002: 12), but ideally return home again. Later in the life course, elderly persons enter a kind of wild space, and become spiritually "pre-ancestral" (Rasmussen 1997).

Turning the Ethnographic Tables: Researcher's Entry into the Spirit

A Decidedly Un-exotic "Staged" Exorcism Ritual

How exactly was this anthropologist/ethnographer drawn into these travels of the soul toward the "wild" space of possession, and what was learned? I experienced an approximation of *essuf*, but only upon revisiting it, much later in my research, in 2002. Until then, I had only witnessed *tende n goumaten* rituals held for others—never for myself, the researcher. When I returned in 2002 to the rural Air Mountain community in northern Niger of my long-term field research, I brought with me for the first time a video camcorder. [Prior to this visit, I had photographed this ritual with a standard camera in "still" photos, and recorded its songs on audio-tape.]

I felt excited at the prospect of vivid documentation of the dynamic motions of the trance dance with my new equipment. Friends, adepts, and assistants/consultants graciously gave me permission to do so. As we awaited a ritual, however, none occurred. Some men insisted, "the *tende n goumaten* is on the decline because of the new clinic constructed nearby." Other residents—women and men— speculated, "it is because there are so many Islamic scholars (marabouts) who have migrated to sedentarized

villages, and built new mosques and more Qur'anic schools."[7] Others related how, in one town, a session had led to divination which identified a thief, who turned out to be a member of a town government, and the ritual there was subsequently discouraged.

Yet female adepts insisted, "Yes, these rituals still take place!" and listed a number of local women active in the ritual, predominantly matrilineal kin of other frequently-possessed women I had known long term.

After about a week's delay, I reluctantly requested that a ritual be staged. Previously, I had shunned arranging performances and rituals of any kind, and had always waited for them to occur in context, initiated and organized locally for stated local purposes. I feared the danger, in my view, that arranged rituals would be locally-perceived as entertainment, and hence not "genuine" healing processes. In hindsight, I now realize the problems of assuming any "objective" participantobservation or "naturally-occurring" ritual or performance. Of course, since my earlier study of possession, anthropology has changed, and I have changed as well. Many studies in folkloristics and anthropology of religion recognize the value of more intertextual and reflexive approaches (Cole 2001; Stoller 1995, 2004). In keeping with such critiques, I am not arguing here that this arranged performance was any less "authentic" than a spontaneously-occurring one.

Rather, I seek to make sense of this ritual in terms of local residents' and my mutual construction of knowledge, in our joint healing. It became apparent that, despite my stated reason for arranging this ritual (which I considered more a "performance," in my own cultural categories), my friends and adepts believed that I, too, suffered from spirits of the wild, and regarded this event as like any other local possession ritual. Furthermore, they also brought to this ritual their own preoccupations, predicaments, and memories; these, too, were reflected in the ritual process. But first, why did my friends and consultants believe I, as well as they, could suffer from the calls of the Kel Essuf?

The female adepts assumed that, since I had asked them to organize a healing ritual for myself, I suffered from *essuf* affliction, despite my stated reason of wishing to video-tape the ritual. Tuareg noble cultural values emphasize indirect speech by allusion (*tangalt*), rather than directly expressing one's real sentiments. The women, in particular, had additional reasons to believe that I had a spiritrelated depression, an "illness of the heart and soul," and that I was in *essuf*.

I had in fact, been anxious and depressed lately, from several causes. Of course, I missed my loved ones back home in the United States. The Tamajaq term *tamazai* denotes approximately a depression (*tamazai*) from "lacking" something (*anoughou*), a

---

[7] Recently, outside sects, including some outside "Islamist" reformists, have established mosques and Qur'anic schools in some parts of northern Mali and Niger. These sects should not, I emphasize here, be confused with militant groups. Rather, they advocate imposing religious Shar'ia laws upon state laws, in a manner similar to some fundamentalist Christian groups in parts of the United States. The Islamist reformists have received a mixed reception among the Tuareg, who vary considerably in religious devotion.

change, or a hidden sadness, which is believed to leave the person vulnerable to the spirits. Another possible cause, in their viewpoint, was *tarama*, either unrequited or unfulfilled (whether temporarily or more long term) love. Rural Tuareg women own property independently, may initiate divorce, travel long distances in nomadism, and do not need husbands' permission to visit or receive visitors. Yet women are also firmly associated with the tent and home— for example, they transmit folk tales, the Tifinagh script, and moral education to children (Nicolaisen 1997; Ag Soliman 1996; Ag Erless 1999). Many men and women explained to me that "often, women become possessed from a hidden love sadness, or illness of the heart and soul, and life without husbands can be difficult economically and emotionally." Many women nowadays are left to fend for themselves in husbands' labor migration.

Another problem local women face, in their own viewpoint, and which my female friends believed might plague me as well, is that some more prosperous men are contracting polygynous marriages, contrary to the traditional prevalence of monogamy. Most Tuareg women oppose polygyny, but many men return home from abroad with different ideas of gender relations, and justify it by stating that oasis gardens require more workers, and that men on labor migration need homes in different places.

The point here is that these tensions local women face shaped their interpretation of my own request for a possession ritual. This was revealed in conversations. One person somewhat reproachfully asked me, "How can you neglect your family?" Many people also asked me why I was childless, and one even commented, "How can one ever have a child, if one travels so far from home?" A few women asked me slyly, "Don't you worry about your husband marrying a second wife while you are gone?"

Hints that my friends suspected that I suffered from *essuf* myself were also conveyed in various actions performed before the *tende n goumaten*. A prominent Islamic scholar/ marabout, arrived just before the ceremony and made protective amulets, as he did frequently for possessed persons in consultations prior to and following the ritual. Also, a few days earlier, an herbalist-diviner giving me a massage had diagnosed *togerchet* (evil mouth or gossip-related affliction, frequently treated in combination with spirit possession afflictions.)

What, I wondered, was the source of this evil mouth/eye? In fact, I had recently experienced a social conflict and dilemma. Perhaps my friends—healers, adepts, and host family—had noticed them. Several years earlier, I had on two occasions, hosted in my home an artisan from their community who was travelling in the United States selling his silver jewellery. I hoped that such assistance would in a modest fashion, benefit him, his family, and perhaps—indirectly, his village. Later, however, I discovered a social conflict between the artisan's family and my host family in the field, which I had been unaware of earlier. Rural nobles and smith/artisans traditionally share mutual rights and obligations in a client-patron relationship. In return for smiths' work, nobles are supposed to support smiths, but nowadays many nobles are impoverished, and many smiths have become prosperous in working increasingly for the tourist trade as jewelers. There are consequently many tensions between them (Rasmussen 1998,

2001a). Hence I was drawn—albeit unwittingly and reluctantly—into local politics. To my relief, these fences were mended upon my reassuring my hosts they, too, would be welcome in my home if they ever travelled to the United States. A marabout reassured me of this conflict resolution when he divined through his dream that I had "vanquished my enemies."

These medico-ritual treatments leading to the possession ritual inspired me to carefully reflect upon reasons why, in local terms, my soul approached *essuf*.

There was also, finally, another situation causing me sadness. Upon my return to the community of my long term research, I noticed that the aged father of a research assistant, in my host family, was now enfeebled, perhaps terminally ill. He occupied a room in a compound that had been formerly home to his extended household, where I, too, had stayed as guest on several project visits. But its central compound was now abandoned: there were no more tents (of married women) there.[8] I wistfully recalled how, some fifteen years earlier, when my research assistants (now a married couple) had planned to marry, there were lively, joyful preparations there for their wedding. At that time, the assistant's father and mother had both been vigorous. Their compound had been a dynamic social center, and the older couple had enthusiastically recounted folk tales and legends to me.

Now, my assistant's father was widowed, and had become an invalid, emaciated and fragile. He recognized me, but sat rather dejectedly upon his mat inside the crumbling edifice of what had once been a well-maintained grass building used for guests. Intermittently, he slept. Fewer and fewer people came by. Although his daughters continued to cook for him, they did not linger, and sent small grandchildren to deliver his food and tend the fire.[9]

Thus that compound, once lively years before in my own and others' memories, now stood nearly empty; and the old man, while cared for and loved, was no longer a vital social presence. Here, I keenly felt the essence of *essuf*—a space once full of people and social life, and now a space of nostalgia and the wild, which also prompted me to think of my own parents' declining health. Many Tuareg warned me that "spirits fill places (that were) once full of people and (are now) deserted," for example, abandoned campsites and empty markets.

In several ways, therefore, my own and this family's experiences and sentiments intersected. We all had to move periodically, in physical travel as well as emotional and symbolic nomadizing. We all coped with loss and distance. I could, so to speak, meet them half-way in meaning construction at certain points in my own experience. While the specifics differed, we all suffered generally from personal loss, disruption in social relations, and anxieties over change and distance. These predicaments induced feelings

---

[8] In nomadic communities, the married woman brings her tent (*ehan*) as dowry, which she owns, to the household upon her wedding, and maintains it even upon divorce.

[9] Aging among the Tuareg is based upon social and ritual criteria: one is "old" when one's children are married, and one remains active in social and ritual events until frailty prevents this (Rasmussen 1997).

of *essuf*—equally genuine on everyone's part. This intertwining of some (though not all) of our experiences, and our mutual empathy and engagement connected us on some levels, and enhanced my understanding of possession. The hermeneutic effect here of what I call "spiritual travel" was to create some degree of "intellectual nomadism." Hence the value of the knowledge revealed to me through this incident, of what locals believed to be my own possession.

Yet in other respects, our experiences diverged. The experiences of this family and others in their community also included much socioeconomic deprivation and political violence. Hence the necessity here to move beyond the level of my personal sentiments and the immediate social interactions between myself and local residents. While that level is analytically instructive, more is required here. Both perspectives are relevant, and play roles in meaning-construction. The intersection between researcher's and local resident's own experiences to some extent bridged the gap between assumptions of "rationality" or "truth" on the one hand, and subjectivity and emotions, or "belief" on the other. It was only upon my being treated as a possessed person in the wild/solitude/nostalgia, that I began to reflect more carefully upon the inward subjective sentiments, as well as intimate social dynamics, involved in entering the space where the possessed "travels." My literal travel to the field over many years was necessary, but not sufficient, to produce these insights. Only as presumed patient, in their view, did I finally begin to consciously enter and glimpse into the possession space myself, through the local lens or idiom. In other words, those therapies caused me to nomadize spiritually and reflect more carefully about the possibility of my own *essuf*, first stimulated by my missing of distant loved ones, next by my encounter with social conflict and my regrets over my resulting dilemma, and then by my nostalgia at seeing the ailing elder, who was now also, effectively, in *essuf*. [Later, after my return to the United States, I received the sad news of his death.]

Yet is it not also the objective of anthropology to transcend one's own epistemological system, and gain insight into that of others? The researcher's own subjective experience of the possession exorcism ritual should illuminate, not obscure or "crowd out," that of local residents. Next, therefore, I examine more closely this ritual itself, and contextualize it by analyzing the ritual's embedding within wider historical, political, and socio-economic forces.

The Ritual Process and Beyond

The Battle against the Wild
The foregoing evidence suggests that local residents were concerned about my welfare. But what else were they saying? The meanings of possession are not unitary, but multiple; and contradictions can coexist within a single ritual event. What did local residents choose to reveal to me about themselves through their possession idiom?

On the evening of Saturday, January 12, 2002, when my requested spirit possession ritual was held, a small group of kinspersons of the adepts were present with their babies and children. The ritual was held inside the house of a woman in a family of long-term adepts—daughters who had inherited their spirits from their late mother, whom I had known since the 1980s. One sister sang solos and, for percussion, substituted a bucket for the usual *tende* drum, struck with the hands. The other sister substituted a water-can for the usual *asakalabo* calabash, struck with a long stick.

Several women there asked me whether I would perform the head-dance. I wished to devote most of my energies to video-taping this dance, as performed by another adept known for her recurrent possession, sometimes spontaneously, during rituals arranged in advance for others. Thus the question arises, whether I could refrain from this trance dance, and still be considered possessed? Local residents indicated that, while possessed persons frequently perform this headdance, sometimes, possession occurs without it, though spirits are "most pleased" if someone at the ritual performs this dance. So I left most of this dance to the expert, and attended to my video, although intermittently I briefly rocked my head to the songs, and this won approval. The ritual lasted until around 3 a.m.

The soloist and chorus performed a repertoire of sung poetry accompanied by drum patterns that Tuareg explicitly associate with the *tende n goumaten*. The soloist had composed some songs herself, and had learned others from other *tende n goumaten* singers. These songs were very moving. Some song themes and images were subtle, and thus open to a variety of interpretations. Others made more direct references to events and predicaments of participants there.

The first song performed was entitled Yelwa (A Great Care, Worry, or Concern) (drum pattern: *talawankan*, a slow rhythm, most associated with the spirit possession ritual): "My soul, oh my soul/My soul, up high, up high, applauding/ Oh my soul Yesterday evening I was asleep in the middle of the night/A message came to me/It was the greeting of a lover/Oh, I address the young girls of my village, if you know God/Assist the song well and applaud that is going /The young girls have left/It is the care (worry, concern) that has left from them (i.e. their spirit is impacted by the care)/The care of young people who are in distant countries/Oh, my soul has aged, grown old (i.e. in local exegesis, she does not see the young male dancers)/Oh, my soul has aged because it does not hear thanking, appreciation (i.e. the dancers and the applauding audience) ."

The next song was entitled Azel n'Azori (Song of Azori) (drum pattern: *elengue*, a drum pattern associated with a bent-over dance-style): "The young people of my village/My soul addresses the young girls, applaud the youths/The festivities have become orphaned because they lack Siliman (pseudonym, a man in a nearby oasis)/ Siliman represents onyx (a metal that does not tarnish; very noble metal) that one bought from Abalawlaw (a merchant in the iron market in Agadez)/My young people of my village, I address Salifou (pseudonym, an exrebel now in the national army)/He fires the Kalashnikov (rifle) in the year that I cannot forget (i.e. during the rebellion)."

There followed a song called Beliwa (Unripe Dates) (i.e. in local exegesis, symbolizing a bitter soul): "Oh, my bitter soul Applaud/in the middle of the night, yesterday night,/some good news came to me/Get up, in order to make-up (the face), put on the blue eye-liner /Oh, my bitter soul, I address the young persons of my village /among them there is my older brother/This song it is for Moussana (a deputy in Niamey representing a regional political party)/that the courageous men have founded."

Following this, the next song's title was Lale douwe (i.e., a song that is uniquely dedicated to someone), its drum patterns mixed: "Young girls whom I address, you must applaud me/so that we do a song for Rhubda (pseudonym, son of soloist's sister on labor migration doing oasis garden work in Libya)/This piece is dedicated to Rhubda, my nephew who is in —— (village in the south of Libya)/His steps resemble drops of water/His smile resembles gold/I dedicated this song to Rhubda/Applaud, Oh, the *gouma* (woman performing head dance) is tired." [Becoming exhausted is a sign of the spirits, having mounted their adept, are pleased, and are leaving the possessed in peace, at least temporarily.] (Rasmussen 1995).

The next song was called Heme Helane (vocables) (drum pattern, *Jagadawe*, in dancing, one runs; in the possession ritual, the possessed extends and moves her arms): "Oh, young girls/ If you know God/you must help the festivities, the festivities have become orphaned/this song I composed it for the young people of before who have served in the rebellion (*atawra*)/who were born in Niger, those who have rained iron until dawn/who drew a 12–7 missile that roars like a lion, a Kalashnikov rifle drawn by youths, people I thank you for taking up arms on the shoulders/Me I give you my greetings, for you Mamane, Salihou, and Amoumoun, Tahirou, Oumarou, Ibrahim (pseudonyms, several former rebel fighters from soloist's village),/ I encourage Gannani (pseudonym), who saved and helped a thousand persons."

Discussion: Poetic Wandering in Possession

What kind of wanderings were occurring here? These songs, which I later transcribed and translated with exegesis from both assistants and singers, conformed to the usual pattern of *tende n goumaten* song progression noted by Hawad (1979) and Rasmussen (1995): they began with themes of distance and solitude from calls of the Kel Essuf, and evolved into more socially-conciliatory songs, for example, praise of heroes. I could identify more easily with the images in the first songs concerning distance, love, and nostalgia. There were very different reasons for our uprootings from home and separations from loved ones, however. In one song, the soloist praised the former rebel fighters, who were still viewed by many (though not all) persons in that region, at that time, as heroes and protectors. For there had been attacks on non-combatant villagers by some errant militia, even following the peace accords. A party deputy is also praised. Despite multiple political parties since the peace pacts, there had been some violence against a popular party in their region: during a rally in 1992, a soldier had thrown

a grenade, seriously wounding many. In effect, the verses' symbolic content conveyed a sympathetic or empathetic commiserating over different experiences we brought to this event.

In another song, the soloist also praised labor migrants. Since approximately the 1970s— following the devastating droughts of 1969–74 and 1984, unemployment, and de-valuation of currency in Niger and Mali following the World Bank and IMF structural adjustment and privatization programs in the 1980s—many Tuareg men have been compelled to find wage labor in Nigeria, Libya, Algeria, France, Italy, and Belgium. The singers' families and other villagers had lost herds in a devastating drought in 1984, there had been a serious malaria epidemic, and their mother had died. These calamities had uprooted family members.

Thus in one song, the soloist referred to her nephew, who was in Libya working. This singer's family also had worries over their young relative's safety, monetary gain, and when precisely he would return. According to rumors, some employers in Libya shoot Tuareg migrant laborers after the latter complete their gardening or herding, to avoid paying them. These actions are difficult to document, but the point is that these rumors, whether true or not, caused much stress among their relatives. This soloist, through her verses, hoped the spirits would guide and protect her nephew in his wanderings.

Somewhat like the incantations of the Cuna shaman analyzed by Levi-Strauss (1963), these metaphorical allusions were likely intended to have the soothing and calming effect so central to healing the body and soul. Not only I needed healing, but also, they did; for this family and their neighbors had experienced severe adversity over the years intervening between my earlier study of possession and now.

Essuf is therefore not solely an affliction, but also productive: a place of creativity, re-peopled by nomads, poets, diviner-mediums, and the *tende n goumaten* musical curers. Nomadic transhumance imagery pervades Tuareg and other Berber poetry. According to Drouin, the poet "weaves the real and the unreal, after having distanced them, and senses profoundly the sadness of *essuf*" (Drouin 1992:180). Hawad the poet likens his poetry-writing to wandering (Penel 1991).

Similarly, the singer-adepts and poets at my arranged *tende n goumaten* ritual attempted to re-people empty spaces—to bring dispersed persons back into the community and closer together. The singers-curers were prompted by my own presence and their concern for me to reflect further on all our predicaments. Thus local residents and researcher filled the empty space of *essuf* with poetic spiritual travel, in self and mutual reflections, thereby leading me through this intellectual nomadism toward a more nuanced understanding of possession.

Conclusions

In spiritual travel to the wild or solitude, as in nomadism and ethnographic field research, one is swept away from one's own social convention and community support, with contradictory and unpredictable outcomes, but often, later being drawn back into the familiar social community.

Anthropologists who venture into local spiritual domains achieve some reversal—however temporarily—of hierarchical relations between authoritative knowledge systems, thereby allowing some rapprochement between self and other, "truth" and belief, in knowledge construction: local residents respond to the researcher's presence and thereby comment, however obliquely and tactfully, on interpretation.

Acknowledgments

References

Ag Erless, Mohammed. 1999. *Il n'y a qu'un soleil sur terre: Contes, proverbes et devinettes des Touaregs Kel-Adagh.* Aix-en-Provence: Edisud.

Ag Soliman, Alhassane. 1996. *Bons et Mauvais Présages.* Paris: l'Harmattan.

Bernus, Edmond. 1981. *Touaregs Nigériens: Unité d'un people pasteur.* Paris: ORSTOM.

Boddy, Janice. 1987. *Wombs and Alien Spirits.* Madison: University of Wisconsin Press.

Boilley, Pierre. 1999. *Les Touareg Kel Adar (Mali).* Paris: Khartala.

Casajus, Dominique. 1987. *La Tente dans l'Essuf.* Cambridge and Paris: Cambridge University Press.

Charlick, Robert. 1990. *Niger: Personal Rule and Survival in the Sahel.* Boulder, CO: Westview Press.

Childs, Larry and Chelala, Selina. 1994. Drought, Rebellion, and Social Change in Northern Mali: The Challenges Facing Tamacheq Herders. *Cultural Survival Quarterly* Winter: 16–20.

Claudot-Hawad, Hélène. 1996. *Touaregs: Voix Solitaires sous l'Horizon Confisqué* Paris: Ethnies.

——. (ed.) 2002. *Voyager d'un point de vue nomade.* Paris: Editions ParisMéditerranée, IRMAM.

Cole, Catherine. 2001. *Ghana's Concert Party Theater*. Bloomington: Indiana University Press.

Crapanzano, Vincent and Garrison, Vivian (eds). 1978. *Case Studies in Spirit Possession*. New York: John Wiley.

Drouin, Jeannine. 1992. Amour, Poésie, et Onirisme chez les Touareg, in *Amour, Phantasmes et Sociétés en Afrique du Nord et au Sahara*, edited by T. Yacine. Paris: Harmattan-Awal, 173–88.

Good, Byron. 1994. *Medicine, Rationality, and Experience: An Anthropological Perspective*. Cambridge: Cambridge University Press.

Hawad, Mahmoudan. 1979. *Le Tagdudt*. Tisruraf: Groupe d'études berbères, Université de Paris VIII 3: 79–82.

———. 1985. *Caravane de la soif*. Aix-en-Provence: Edisud.

———. 1987. *Chants de la Soif et de l'égarement*. Aix-en-Provence: Edisud.

Keenan, Jeremy. 1978. *Tuareg: People of Ahaggar*. New York: St. Martin's Press.
Lambek, Michael. 1981. *Human Spirits*. Cambridge: Cambridge University Press.

Lévi-Strauss, Claude. 1963. *Structural Anthropology*. New York: Basic Books, Inc.

Lutz, Catherine and Abu-Lughod, Lila (eds.) 1990. *Language and the Politics of Emotion*. Cambridge: Cambridge University Press.

Moore, Henrietta. 1996. *The Future of Anthropological Knowledge*. Cambridge: Cambridge University Press.

Nicolaisen, Ida and Nicolaisen, Johannes. 1997. *The Pastoral Tuareg*. Copenhagen: Rhodos.

Nicolaisen, Johannes. 1961. Essai sur la Religion et la Magie Touaregues. *Folk*, 3: 113–60.

Oxby, Clare. 1990. The 'Living Milk' Runs Dry: The Decline of a Form of Joint Ownership and Matrilineal Inheritance among the Twareg (Niger), in *Property, Poverty, and People: Changing Rights in Property and Problems of Pastoral Development*, edited by P.T.W. Baxter and R. Hogg. Manchester: Manchester University Press, 222–8.

Penel, Jacques Dominique. 1991. Hawad, Marcheur, Poète, Calligraphe. *Notre Librairie* (Littérature Nigérienne) 107: 86–91.

Peters, Larry. 1981. *Ecstasy and Healing in Nepal*. Malibu, CA: Undena Publications.

Rasmussen, Susan. 1995. *Spirit Possession and Personhood among the Kel Ewey Tuareg*. Cambridge: Cambridge University Press.

———. 1997. *The Poetics and Politics of Tuareg Aging: Life Course and Personal Destiny in Niger*. DeKalb: Northern Illinois University Press.

———. 1998. Ritual Powers and Social Tensions as Moral Discourse among the Tuareg. *American Anthropologist*, 100(2): 458–68.

———. 2001. *Healing in Community: Medicine, Contested Terrains, and Cultural Encounters among the Tuareg*. Westport, CT: Bergin & Garvey Press.

———. 2001b. In the Shadow of Sheltering Songs (Trees). *Journal of American Semiotics*, 17(4): 43–92.

———. 2006. *Those who Touch: Tuareg Medicine Women in Anthropological Perspective*. DeKalb: Northern Illinois University Press.

Reed-Danahay, Deborah (ed.) 1997. *Auto/ethnography: Rewriting the Self and the Society*. Oxford and New York: Berg.

Rosaldo, Renato. 1989. *Culture and Truth*. Boston: Beacon Press.

Siikala, Anne-Leena, and Hoppal, Mihaly (eds). 1992. *Studies on Shamanism*. Helsinki: Finnish Anthropological Society.

Stoller, Paul. 1989. *The Taste of Ethnographic Things*. Philadelphia: University of Pennsylvania Press.

———. 1995. *Embodying Colonial Memories: Spirit Possesssion, Power, and the Hauka in West Africa*. New York: Routledge.

Stoller, Paul and Olkes, Cheryl. 1987. *In Sorcery's Shadow*. Chicago: University of Chicago Press.

Turner, Edith. 1997. *The Hands Feel It*. DeKalb: Northern Illinois University Press.

Winkelman, Michael. 1999. Altered States of Consciousness, in *Encyclopedia of Human Emotions*, edited by D. Levinson et al. New York: Macmillan, 32–8.

———. 2003. Shamanisms and Survival. *Cultural Survival Quarterly*, Summer: 12–16.

Worley, Barbara. 1992. Where All the Women are Strong. *Natural History*, 10(11): 54–64.

Chapter 10: Flying Witches, Embodied Memories, and the Wanderings of an Anthropologist

Raquel Romberg

Travel and travel writing as a trope have been suggested as vantage points for generating social critique and self-knowledge in the writings of Enlightenment philosophers such as Montesquieu, and of Romantic intellectuals such as Goethe.[1] They have contributed significantly not only to the 'spatial practices' of anthropological research but have also shaped its disciplinary boundaries and writing genres from its earliest nineteenth-century humanistic 'salvage' practices to its subsequent scientific, comparativist quests.[2] Travel to, and dwelling in, the 'field' (for various periods of time) have become cornerstones of anthropological knowledge and disciplinary boundaries no less than of the heroic habitus adopted by anthropologists and their academic exchange value.

But journeying is not limited to traversing space. As Proust (1982) and Deleuze (1987: 482) have suggested respectively, a particular object might transport us to other times, and some types of journey (e.g., temporal) might not require actual movement.[3] Indeed, my ethnographic excursions in Puerto Rico, particularly my encounter with Tonio, an old witch-healer (*brujo*), revealed that such forms of temporal wanderings had much to teach me. While the pedagogical purposes of the Aristotelian Peripatetic School assumed the epistemological value of spatial practices such as walking about the circular arcade (*peripatos*) of the Lyceum. I found comparable pedagogic/ethnographic

---

[1] See Montesquieu's *Persian Letters* (1964 [1721]), which includes the satirical insights of two fictional Persian brothers describing (in epistolary form) their travel impressions of France, in particular the 'barbaric' absurdities of French life; and Goethe's Italian Journey (1982 [1816–17]), where he describes the various places and people he encountered during his extended trip to Italy (1786–8) and how these experiences enhanced his self-knowledge as an artist through a process of defamiliarization: 'My purpose in making this wonderful journey is not to delude myself but to discover myself in the objects I see' (57).

[2] I draw on Michel de Certeau's notion of 'spatial practice' (1984), critically revisited by James Clifford (1997) to comment on fieldwork and other transcultural predicaments of the late twentieth century such as travel, diaspora, and border crossing.

[3] See Proust's *In Search of Lost Time*, and Deleuze's *A Thousand Plateaus*, p. 487.

value in Tonio's peripatetic embodied memories.[4] For they had not only surreptitiously traversed the moral chronotopes separating colonial and modern Puerto Rico as well as this and other invisible worlds, but also stirred my own imagination and temporal wanderings. This essay evokes these dialogic wanderings in an imperfect mimetic rendering three times removed: bridging Tonio's embodied spatiotemporal journeys with my own temporal journeys represented in the mimetic inscriptions that follow. In this sense, this is an invitation to reflect on such journeying, not simply as a way of commenting on anthropological research and writing practices, but more importantly of approaching the ways in which fieldwork might be envisioned as a dialogic temporal form of nomadism.

The first time I saw Tonio, in July of 1995, he was sitting in his wheelchair watching TV, dressed in a worn-out robe, his short-cut hair in a net fashioned from a piece of what looked like a woman's dark-skin-coloured panty hose. He was about 90 years old at the time. Someone at the library of Loíza had told me that he was one of the most renowned *brujos* (witch-healers) of Puerto Rico. His house was only a few minutes away from the local library. When I arrived, a man in his sixties – his son – opened quite hesitantly the outside iron-gate and invited me to wait for Tonio on the porch. I was gazing out across the road when Tonio wheeled himself onto the porch a moment later. He pointed with pride to the Cave of Loíza across the road from his house and told me about its significance for Puerto Ricans. A mystical place inscribed in the nationalist history of Puerto Rico, it is the place where a very powerful Taíno female cacique by the name of Loíza (probably Yuisa or Luysa originally, and baptized as Luisa) lived. Adding to the mysticism of the cave, a natural stream runs under Tonio's house, providing the 'blessed' water of the earth for drinking and irrigating. Everything around him – people and plants – 'grow' fast and strong, I later heard from other brujos.

I ask him about *brujería* and *espiritismo*. Without a word, he uncovers a small silver medal of the Virgin of Mercy carefully pinned to the underside of his pyjama-shirt collar. She was the one who appeared to him when he was 7 years old, promising him spiritual guidance and protection; her metal image has protected him ever since. He begins to pray in Latin, showing off his knowledge of Catholic prayers, which he uses to heal, he tells me. I gather that he is Catholic; Jesus and the saints protect him and direct his healing practices.

Tonio also speaks as a popular Spiritist medium in the tradition of Kardecian Scientific Spiritism. A fashionable secular-transcendental practice created in France during the second half of the nineteenth century, Spiritism was based on the belief in human communication with enlightened spirits and reincarnation. European Romantics believed that enlightened spirits could direct people in creating a progressive, morally sound world, and this is why Spiritism was embraced by nineteenth-century liberal, an-

---

[4] While not based on etymology, perhaps this explains the synonymic connection between 'peripatetic' and 'nomadic' – a curiosity that I take as one of the inspiring threads of this essay.

ticolonial and progressive elites as a counterforce to the hegemony of Spanish Catholic rule in Puerto Rico, and became through the decades a legitimate sphere for alternative transcendental practices (Romberg 2003). Tonio, as many other *brujos*, adopted the form and content of Spiritist practices such as holding night-time spiritual gatherings or *veladas*. Following a minimalist symbolism, Scientific Spiritists believe that at these types of gatherings the white tablecloth, the white candles, and the Bible they use attract spirits of light or enlightened spirits, who will eventually appear and give their messages.

Hundreds of novice mediums, who are now practicing *brujos*, developed their mediumship under the auspices of young Tonio, who used to be the head or *presidente* of these *veladas* for more than half a century. He also consulted some who needed special spiritual treatments in his private consultation room or altar. This is how Haydée, the *bruja* I got to know through Tonio, met him. As a child she used to accompany her father at dawn – as early as four or five o'clock in the morning – to Tonio's *altar* in order to assure they would be able to consult with him since, Haydée once told me, hundreds used to come from all over the island to see him. At his home I often heard Tonio speak of the good will, charity, and positive energies that inspired his work as a healer. As I would hear among other healers during my fieldwork, Tonio asserted his mission as a healer by uttering the Spiritist motto, '*Fé, esperanza y caridad*' ('Faith, hope and charity'), commonly used as a greeting among healers and in consultations.[5]

But I also remember people telling me that Tonio Lacén was a famous *brujo* or witch-healer, implying that he was an *espiritista* who was also knowledgeable in performing all sorts of magic works or *trabajos*, and especially in managing and controlling any evil forces that were, or might again be, involved in harming his clients. Why was Tonio covering up this aspect of his work, and highlighting only his Catholic devotion and Scientific Spiritist ethics? 'What are the African aspects of what you do?' [I hesitantly ask, in keeping with the academic questions I had proposed back at home for my investigation.] 'None.' I realize it was an inappropriate question. He immediately points to a young kid getting a haircut in the yard, saying, 'Come here!' As the boy approaches, Tonio says to me, 'You see this dark black kid, a relative I take care of, *he*'s of African origin [implying *not me*]'. I'm astounded by his reaction to my question, for Tonio himself looks phenotypically black to me, his complexion only a bit lighter than the kid's. Sensing he does not want to be associated with anything related to Africanness, especially to slavery in Puerto Rico, I question the appropriateness of my asking him about the African aspects of his healing practices. I don't wish to offend him. Evidently he sees himself not at all as a black person, but simply as 'just' a Puerto Rican. Coming from Argentina, I had already learned that American racial categories are not applicable elsewhere. A connection to Africa is not what Tonio wants me to see in him in our first meeting. I gather that for Tonio, as for many other Puerto Ricans who do not engage publicly in the politics of race and colour, things African are still cast in a

---

[5] I recently learned that these are the three theological virtues recognized by Masons.

problematic light. 'Here, take some *chavos* [slang, money] for candy,' he tells the kid, sending him off. That was the end of my probing into the African connections of his healing practices.

As I continue to listen attentively to bits and pieces of his life history – the names of living and dead neighbours and family members who had consulted him throughout the years – he begins gradually to add the minute details of his true path as a *brujo*. He knows of all sorts of prayers and healing plants, he tells me, listing a series of ailments and their possible cures with plants, which I manage to jot down quickly if only partially: *sándalo* (sandalwood), *mejorana* (marjoram), *geranio* (geranium), *yerba buena* (spearmint); and for the stomach, a mixture of leaves of *guanábano* (cherimoya tree, *annona muricata*) and *Juana la blanca* (angel trumpet, datura arborea) should be rubbed with olive (San Rafael) oil or oil of camphor, on the navel.

Does he think I came for a consultation? I wonder, but then he begins to reminisce:

> I used to go to the *monte* [rainforest, mountain] to get the bark of *mabí*]seaside buckthorn, *Colubrina elliptica*]. I harvested it at night and used it during the day. I also made *ditas de higuera*, bowls made of the fruit of the calabash tree [*Crescentia cujete* Linnaeus], which I learned from an African healer who used to wear golden earrings, and had a red dragon to overpower *malos pensamientos* (bewitchments).

Suddenly he shifts from plants to people, reviewing the cases he had encountered during his many years as a healer. Reminiscing about another healer, Tonio begins to enact an apparent duel between himself and an old *bruja*, Isidora Osorio, now deceased. While he swings his long hands in front of me, he says,

> She, with her big red-and-white dress, told my apprentice Rafael '*Te voy a sacudir el vestido dos veces*' ['I will waggle my dress two times']. It was a bewitchment – and he got sick. With the leaves of *albahaca* [basil] and *ruda* [rue, *ruta chalepensis*] I made a cleansing bath [to free him from Isidora's bewitchment]. Isidora had [a magical double of] Rafael tied to a plant of *guineo* (plantain). She told me, 'You set him free from my bewitchment? Apparently she had [spiritually] smelled the scent of the cleansing bath that I had made for Rafael and asked me 'Are you now going to do something against me?'[6]

Aware that he could be the object of Isidora's revenge, Tonio then instructed Purita, a woman who was apprenticing with him, 'When you go to Isidora, check whether she prepared something against me. [Then, turning to me] You know, Raquel, the rosary

---

[6] In the glossary of El Monte, Cabrera (1975: 539) provides a few stories about the *ruda*, '*Brujos* hate it, it's their worst enemy.' '*Mata que mata brujo*' ['Plant that kills magic works'], which parallel Tonio's account.

protects you, especially *el Salve* ['Hail Mary'].' I nod, registering this as an advice based on his own experience of praying the rosary as a protection against bewitchments. Tonio is obviously very well versed in Catholic worship, I think to myself, but he might not know that as a Jewish-born woman I have only a superficial sense of what 'the Salve' entails.

Lunchtime is approaching, and Tonio's daughter-in-law, a woman in her fifties who is taking care of him, announces that it will be ready shortly. 'You know, Raquel, I used to cook a lot. I was very good at it.' As he describes old ways of cooking all sorts of meats and vegetables that entail covering them with banana leaves in outdoor cooking hearths, I can see him delighting in those past flavours as much as he delighted himself in recalling the recipes of the cleansing baths and healing bandages he used to concoct as a healer.

I ask him when he began working as a healer.

> At the age of six. One day I had gone as usual to fetch some fresh water from a stream far from the house. I saw a mysterious woman wearing a long dress, down to her feet. I filled one of the buckets with water and then lay down to rest in the centre of some palm trees. When the woman approached me, I asked [seeing that she was distraught], 'Señora, what's the matter with you?' The woman said, 'Give me a little water.' She took my bucket and said, 'O, My Father, San Antonio [Tonio's full name], child of God, help me. They are persecuting me. Where should I go?' After filling another bucket of water for her, I told her to follow me to my house. But she arrived at my house before me. When I got there, my Aunt Jacinta scolded me for arriving so late. 'You should have come earlier,' she said. 'You left about three o'clock to get water.' But what Aunt Jacinta did not know was that I had had a miraculous encounter. Seeing that I had gotten into trouble for helping her, the Virgin said to me, 'I won't say anything more to you, but I'll leave you with something'. That something was the power to 'see' and to heal.

Reinforcing the fact of his gift to heal, Tonio tells me he was born in a zurrón (caul), believed to be an omen of good luck and special *facultades* (spiritual powers) not only in modern Puerto Rico but also, as historians of religion Keith Thomas (1971: 188, 625) and Carlo Ginzburg (1983) point out, in sixteenth- and seventeenth-century European societies. Tonio recalls,

> As a child of 5 years, I used to say to people, 'You have a *causa* [bewitchment].' My mother had to lock me in the house to protect me from the constant flow of people seeking my help. [He then refers to his spiritual powers:] I see the spirits and they talk to me, but above all is God; faith in God is the most important thing.

I understand what he is trying to tell me: he wants me to know that whatever I have heard about him being a *brujo* (implying his connection to what commonly people see as black magic), he is a Catholic healer who believes in, and draws on the power of, the Christian God in his healing practices.

In this our first conversation of many, Tonio tells me he used to work in the sugarcane fields of the area. I read about them before coming to Puerto Rico. Loíza is known, among other characteristics, for its past as a plantation area where slaves of African ancestry lived and worked. He simply adds, 'There used to be lots of cane in this area.'

Tonio remembers the plantation owner – a man from New York – who used to give him a few coins when he was a child to watch over some areas of the plantation, a role that increased as he became an adult to, eventually, overseer of the entire plantation. But I am surprised that he tells me no horror stories about that time. He remembers, instead, the communal sharing of food – the *arroz y habichuelas* (rice and beans) he ate with the sugarcane workers – during breaks. He does tell me some stories, which I only partially grasp, about the few occasions on which he caught some cane workers getting drunk or having illicit sexual relations during work time on the plantation, and how, by means of his spiritual power as a healer, he managed not only to discipline them without jeopardizing their jobs, but more generally, addressed the love, health and family issues that had caused some of these contraventions in the first place.

At the end of our first conversation he tells me that he would like to help me, but now *Soy pájaro con el ala cortá* (I'm a bird with a cut wing). Only a few weeks later did I understand the full meaning of this aphorism when, during another visit, he said, 'Spirits are birds.'[7]

After almost three-quarters of a century treating hundreds – perhaps even thousands – of clients from dawn to dusk, he had to stop a few years ago, Tonio says. Trance and spirit possession had become too strenuous for his frail body. Even his *protecciones*, which helped him 'overpower any *brujo* in the past', could not shield him in his condition now. So he refers me to Haydée, 'a very positive medium and powerful *espiritista bruja* [Spiritist witch-healer]', one of his spiritual godchildren, with whom I would go on to work intensely as an apprentice for more than a year.

I kept visiting him every week, with and without Haydée, until I left Puerto Rico at the end of 1996. I often brought Tonio foods that he liked, or gave him some money for shopping. During these visits I would witness a host of his clients and friends from all parts of the island parade through his house, asking for advice even though, formally, he was no longer working. I also heard him conducting numerous overseas consultations on the phone, and saw packages containing *trabajos* (magical works) being mailed to cities on the mainland such as Kansas City, New York, and Chicago. He was not *a bird with cut wings*, after all.

---

[7] Similarly, according to Steven Feld (1982: 218), the Kaluli of Papua New Guinea assert that birds are the spirits of their deceased, the *ane mama*, or 'the 'gone reflections' of Kaluli who have left the visible world upon death and reappeared '... in the form of birds.

Perhaps as a result of the increasing intimacy and ease of subsequent conversations over the year, I learned not only about Tonio's deeds as a godlyinspired *espiritista* but also as a *brujo malo* (lit. wicked *brujo*; i.e. an *espiritista* who also performs black magic). I also learned about his desire 'to fly over the Yunque,' the rain forest of Puerto Rico, located only a few miles southeast from Loíza. Haydée and Tonio would often tease each other: 'So when are we going to fly over the Yunque?'

When Tonio, the *Brujo* of Loíza, spoke of flying over the Yunque, he was connecting the world of 1996 with the Spanish colonial imaginary of flying witches during the Inquisition, as well as with four centuries of colonial nightmares and fantasies, often misrepresented as typical of Latin American 'magic realism'. But as the spiritual godfather of many present-day Puerto Rican *espiritistas brujos*, he was also more than likely referring, via a Spiritist metaphor, to the experience of mediumship following the Spiritist tradition of Allan Kardec. Who knows, perhaps he was also referring to the post mortem flight of spirits to the abode of ancestors, typical of ancestor-worship beliefs found among Bantú speaking people of West-Central Africa in the Angola-Congo region and their descendants in the Americas, as well as among the indigenous pre-Columbian Taínos.

Healers of many disparate traditions in Puerto Rico consider the Yunque to be the abode of spiritual entities of African, Creole, and Amerindian origin, as well as a mystical realm over which *brujos* hover during their nocturnal flights. Are these the same spiritual flights outsiders have referred to as flying witches over the centuries? Greco-Roman lore traces these flights to the power of witches to transform themselves into birds, especially owls and ravens. A strange connection arises. Tonio said spirits are birds. Books on European medieval witchcraft have plenty of stories about the flight of witches – on brooms, forks, or shovels, or with cats – circling over villages as embodiments of the devil. Ginzburg (1983), for instance, mentions the *benandanti* (lit. 'fighters of evil witches,' often mistaken for the latter) who claimed to use their nightly flights to fight witches (i.e. the devil) in the name of Christ. The Puerto Rican folklorist Teodoro Vidal (1989: 13–23) recorded hundreds of stories about the flight of witches circulating among peasants that depict how Puerto Rican female witches were seen, again, flying like birds, often naked, beating their arms (or a pair of palm branches held underneath their armpits), or thrusting multilayered starched skirts against the wind. [See Figure 10.1.]

Tonio knows the whereabouts of healing plants and animals in every niche and mound of Puerto Rico; in our conversations he recalls hundreds of the names of families and individuals he has helped out in Loíza and other neighbourhoods all over the island. The recitation of places triggers the names of specific healing and magical plants that grow only in one of those places – on a certain mountaintop, for instance, or on the bank of a certain river. The names of healing plants evoke, in turn, their effects: the llantén (a broad-leaf plantain, *Plantaginaceae*), for example, produces *llanto* (tears). The natural effects of plants anticipate, finally, their spiritual effects: the effervescence

Figure 10.1 Flying witches (drawing by Jack Delano, published in Vidal 1989)

of the *mabí* (*colubrina reclinata*, soldierwood) produces renewed energy.[8] A first lesson in the chain of mimesis is being rendered to me. People, plants, and plants' desired effects on people – as Sir James Frazer ([1922] 1960) may have phrased it – are magically connected by various types of contact: by being parts of a whole, and being connected by physical, metaphoric, semantic, and emotional proximity. Plants and their healing and magical effects are also connected to humans by the principle of similarity: the degree of resemblance and correspondence between the sounds and meaning of words and their referents, that is, a linguistic similarity semantically connects plants and human emotions. A personal name that is part of a location, a location that contains a plant, and a plant whose signifier and some of its attributes denote its magical effects on human emotions are all looped in chains of similarity and contact.

I am not permitted to tape our conversations or take photographs of Tonio, for mechanical copies of his voice and face would then be available for abuse (in *trabajos,* for instance) if they fell into the wrong hands. As an unfortunate consequence, rapidly spoken invaluable information vanishes before I can render it in writing. And because of the esoteric nature of much of his communication, many words remain unintelligible even to me, a native Spanish speaker. (It also does not help that he is missing a lower denture!) I find myself drawn even more to his eyes and hands, searching for the gestural equivalents of those meanings I lose when attending only to his words. The antics of his huge hands recreate past worlds and events in front of me. Mesmerized, I follow his arms and head as they move suggestively, yet precisely, mimicking his career as a *brujo*. Detailed recipes for the preparation of magical potions and healing mixtures are dramatized in front of my eyes, his long fingers modelling sizes, quantities, and modes of extracting potent juices that had been injected into a dead lizard or wax figurine. 'I use the round leaf, not the long one,' he says. 'I take a *cantito* (bit) like this, and then – *pángana*! [boom!].' While his hands are measuring the size of the toxic plant he is reminiscing about having used for the preparation of a strong *trabajo*, he grimaces

---

[8] *Mabí* is a Taíno word for the effervescent drink made of its fermented bark and leaves, often referred to as a creole beer. Based on its effervescent properties, the aphorism *subir como el mabí* (to raise like the *mabí*) suggests progress in business or a job.

from the acidity of the magic potion as if he were tasting it right there; and as his arms are knocking down some imaginary enemy, he shouts a passionate *pángana* that converts his grimace into a wicked, victor's smile. He is not only a Catholic healer, then, but a savvy, powerful *brujo* who never hesitates to perform a 'strong' *trabajo* when needed to revoke a misfortune sent against a client.

On my visits to his home, I saw several re-enactments of that powerful, mysterious instant when the magic inflicted upon a dead lizard reaches – through what Sir James Frazer ([1922] 1960) termed 'sympathetic magic' in reference to the principle of similarity (in an iconic way) – its real victim miles away. At times I saw Tonio draw in the air with his thumb pressed against his index finger, as if holding a thin string, the circumference representing the waist of a person. He was mimicking a familiar movement he used to perform to magically tie lovers or estranged couples together. Pieces of clothing worn by individuals who were the object of magic works would be stretched ethereally in front of my eyes between Tonio's hands and placed in imaginary boxes or containers, modelling another principle of magic: contact or contagion, by means of which a part of the whole can affect the whole in a synecdochic way.

In fact, what I was experiencing whenever Tonio mimicked the *trabajos* he used to perform was a re-enactment, a copy of those *trabajos* he had performed years ago. These were copies of copies; also, each re-enactment was a piece of that whole. It was a mimesis of mimesis as much as it was a part that belonged to a whole (his whole life as a *brujo*): the mimetic memory of instances of magical mimesis and contagion. I could not grasp the whole of it then. And even now, I can sense those moments only for a flash before losing them. This is a different way of knowing. Was that the lesson Tonio wished for me to learn?

If it is through copies and contact that magic works, these must bridge the space between the here and the there in order to connect the object of the magic with its simile (in iconic or synecdochic ways) so as to affect the victim through its representation. Similarly, Tonio's mimetic gestures have bridged over time, connecting the performance of that magic act with this embodied mimetic memory. Tonio had bewitched me with the magic of mimesis – with the power of similes twice removed. I had entered the sensuous realm of witchcraft through its chamber of reflecting mirrors. Its various histories flickered in our meetings through chains of similarity and contact, through images, gestures, grimaces, people, and events displaced in space and time. These histories, embodied in Tonio's gestural lexicon were replicas, parts that stood for the whole, for the entirety of his life as a renowned powerful *brujo*.

These somatic accounts are cunning histories, indeed. As I write these lines I wonder: Would this rendering be a third-level replica, a mimetic enactment, a metonymic rendering of those parts that stood for previous wholes? Michael Taussig (1993) has reflected on these issues, characterizing ethnography as 'Embodied Retelling': if ethnographies '*instantiate*', or make '*concrete*', they illustrate the magic of mimesis wherein the replication, the copy, acquires the power of the represented. Making an analogy between ethnographic writing and the magician's art of reproduction with the intention

of estranging writing itself, Taussig further puzzles over the ethnographer's model: '[I]f it works, [the model] gains through its sensuous fidelity something of the power and personality of that of which it is a model,' showing 'the capacity of the imagination to be lifted through representational media, such as marks on a page, into other worlds' (16).

At noon on a day I am visiting Tonio, the phone rings. In a flicker, he crosses himself over his chest and, before picking up the receiver, over the ear against which he will press the receiver. It is a client from Manhattan. He gives her precise and detailed instructions; suggesting that she pray before going to work, buy some herbs, oils, and prayers at a nearby *botánica* on 117th Street; and wait until he mails her a package with a special magic work. Puzzled, I ask him why he crossed himself before picking up the phone and murmured the Rosary after hanging up. 'My enemies might still be trying to bewitch me using every possible means, such as a phone,' he says, 'especially at noon, when magic works most effectively.' He knows this, he continues, because 'I'm a *brujo malo*', a magic work hanging from a cross.

One thing I regret, as I am writing now, not asking then is this: Why did Tonio cross his ear? I had only seen people touch their foreheads and cross their chest. Probably it was an additional protection, the cross acting as a material, literal shield, following the mimetic faculty of magic, against malicious, poisoned arrows. Jesus, represented on the cross, had protected priests and Spaniards in Spain and its colonies from *brujos* and all other sorts of evil since the Inquisition. That same cross, once the symbol of European hegemony – used by colonizers as a spiritual shield against *brujos* and many others who were perceived as fearsome polluters in Spain and the colonies – was now being invoked to protect a *brujo* against other *brujos* in Puerto Rico. By what kind of mimetic cunning has the spiritual power that was once monopolized by priests been seized by witches? Why have those considered the repositories of evil appropriated the very power that was meant to destroy them? After Catholicism had been imposed by force and the threat of annihilation, indigenous and marginal Creole populations crossed themselves and prayed (in innumerable instances, probably) for protection against a host of newly imported evils anathema to Catholicism as well as for protection against the evils of colonial oppression itself. Since the cross was intimately associated with colonial rule, it was most likely seen not only as a religious symbol but also as a symbol of power – a power that was as much coveted and ritually appropriated as it was feared: the power invested in the cross 'pirated' by those who were meant to fear it (Romberg 2005). [See Figure 10.2.]

Accessing chains of similarities at cosmic crossroads between this and other worlds, between North, South, East and West through ritual symbolic gestures of crossing oneself, might have been unexpectedly familiar for some. After all, the European cross might have elicited – via infinite chains of resemblances – safe memories of other

Figure 10.2 A magic work hanging from a cross

crosses, those that used to summon the Congo gods unwillingly left across the ocean.[9] Taussig ponders where this wondrous chain of sympathy begins and ends (1993: 72). All those gestures encompassing 'first contact' between colonizers and colonized at the point of conquest were marked by an essential mishap and misrecognition of chains of similarity, forever permeating future interactions between indigenous and marginalized populations and hegemonic colonial forces and the colonial boundaries between good and evil, Catholic and pagan, civilized and wild (Taussig 1987; 1993).

But the civilizing power, which for centuries and under many guises had aimed at taming the unruly, wild sphere of African and Amerindian healing and magic through Catholicism (and, later, through modernity), was transformed by witches and reflected back on the civilized world in uncanny ways: the fascination of witches with the symbols of civilizing powers that had meant to subdue them was rechanneled into actual magic power. How can one not be amazed by such magic?

The magic world of fierce Amerindians and mischievous Congo spirits must have reappeared in the imaginary world of 'Mr Allen' – an American accountant for a US-owned plantation where Tonio worked as an overseer of cane cutters – each time the accountant came to Tonio for a consultation. Several geographic and moral boundaries that separated the order of the corporation and the disorderly realm of coastal plantations, the English language of rationality and the Spanish language of backwardness, and the colour line of imagined danger and impurity as well as sensuousness were crossed each time Mr Allen came to consult Tonio in the 1950s. Attracted by the desire to seize upon the power of this tall 'black' *brujo*, he knew that when Tonio in trance embodied an indigenous hunter-spirit of the rain forest, no person who was the target of his spiritual performance could resist his power. Mr Allen must have been told time and again that estranged lovers could not resist Tonio's love incantations, and he must have witnessed the famous *ligas* (ties) Tonio used to perform (and which I have witnessed him miming), pleading with Changó the Afro-American spirit of seduction) to reunite lovers and seal their love forever in the cosmos. Tonio no doubt executed this work in the same slow and profound voice I hear him now recount Mr Allen's long-ago appeal to him. As Tonio utters the magical words of the past, his eyes open wide; I feel that Changó himself is there in front of me, directly casting his spell on me. I watch Tonio trace in the air with his long fingers the contours of an invisible calabash, delightfully taking his time to mime with exquisite care the motions of stuffing it with honey, pieces of bark, fresh aromatic plants, and all sorts of inebriating perfumes, before offering it to Changó in exchange for his interventions in bringing back estranged spouses or lovers.

I can only imagine Mr Allen's fascination with the copy of him produced by the dexterous hands of Tonio manipulating the tiny wax figurine (simile) of his body with

---

[9] Robert Farris Thompson (1984; 1993; 2005) has traced and recorded how the memories of the various cultures of Africa had been materialized in the adornment of yards, homes, and bodies, as well as the gestures of dance and worship in different parts of the Americas. See also Gundaker (1993).

penetrating aromas and oils before performing a *trabajo* for him. I feel a fleeting vertigo in savouring the copy of that copy being re-enacted in front of my eyes as I recognize in Tonio's face the lavishness I have learned to recognize in Changó, the irresistible African seducer. Perhaps this is the same vertigo that might have enthralled Mr Allen as he saw himself reflected in the copy of himself crafted and manipulated by Tonio.

Through the magical power of mimesis and contact with Tonio (and, through Tonio, with Changó's attributes), Mr Allen had experienced, if only for an instant, the moral elsewhere of magic; he had possessed the uncanny, unruly, uncultivated space of the Other. Mr Allen, the agent of development and progress, saw himself reflected in the image Tonio had created of him, an image that reflected his own desires to possess the sensual, polluted world of the Other. When Mr Allen came to Loíza from Washington DC, he not only had crossed the Atlantic, separating the US mainland from Puerto Rico, but had also surreptitiously traversed into a space and time foreign, in his mind, to the organizational sphere of progress and development. Through the power of mimetic desire he had entered the mysterious yet equally powerful ancient world of Amerindian and African spirits.

Was I also mesmerized by the magical power of mimesis, enacted in front of my eyes in Tonio's gestures, in the innumerable, scattered mimetic memories of his life as a *brujo*? I could only see a rendering, an imperfect, incomplete copy of that copy of the object of that magic work. Yet, even imperfect copies, as Taussig reminds us (1993: 17), are effective in acquiring the power of the original, in appropriating the essence of the object represented. Also, Paul Stoller (1997: 12–13) tells us that the sorcerer knows that while the arrow he shoots – carrying sickness to a rival – may fall idle on the floor, the 'inside arrow' flies – if the sorcerer's aim is good – above the sky to meet its target. Maybe, as Stoller suggests, the shooting light that the rationalist Edward Evans-Pritchard ([1937] 1976: 11) reported seeing in the middle of the night and interpreted as a bonfire, was – as the natives later assured him – the 'fire' of witchcraft, an 'emanation' of the witch's body, dispatched to cause death to a unsuspecting victim in a nearby compound. That is how magic works. Outwardly imperfect copies may carry the internally perfect copy.

How many imperfect copies had the earth seen Tonio bury, had the rivers seen him deposit for the current to take away, had the forests seen him hide beneath its foliage! Now, I see his fingers touch, prick, stuff, cover, and powder invisible doves, lizards, snakes. In a parallel realm their targeted doubles may have sensed the pain, the restraint, or the liberation that these animals had suffered or enjoyed on another plane. I follow Tonio's stand-in performance, as he moves his mimicking hands through the antics of sewing a calf's tongue stuffed with hot peppers, severing the throat of a chicken, and mixing an enemy's semen with bitter herbs. Some of these mimetic memories trigger the names of lawyers who won 'lost' cases thanks to their suddenly 'overpowering tongues' in the courtroom. The memories elicit a vindictive smile, a re-enactment of the joy he must have felt when accusing witnesses literally lost their voices or abusive husbands lost their sexual powers while their wives eliminated competition

from female rivals. Meticulously pointing to the locations where these magical works had been placed or buried, his long arms move, drawing in the air the directions that correspond to a distant rain forest or nearby cemetery and river. Putting his hands in front of me, Tonio says, 'All the *trabajos* I have made, the earth ate them. How many magic works have I done with these hands!'

The embodied memories I see Tonio enacting before my eyes are current reflections of past reflections of power that had once bewitched *brujos* and now serve their magic works.[10] These are habits acquired through centuries of Catholic indoctrination and repression (and, more recently, through the enticements of modernity and the state), all of which I can now sense and transmit as sensuous renditions.

I think I understand: the gestures of magic bewitch not only once but twice. 'Remember, Raquel,' Tonio said, 'we *brujos* have a power that makes things we say, in whatever form, happen that same way.' I remember him wishing to fly over the Yunque. I wonder, was it a flight that meant to mirror those other ones of preconquest Taíno, of enslaved Africans on American soils longing for Africa, or of flying witches on brooms imagined in the Spanish Inquisition? Or maybe it was a flight that encompassed all those flights altogether. After all, contact, an essential precondition for magic to work, pervades the history of witchcraft in Puerto Rico to this day.

After their usual long gossip session on one of the many occasions I visited Tonio with Haydée, she asked him, half jokingly in order to lift his spirit in light of his declining health, 'So, Tonio, when are we going to fly over the Yunque?' As if to alleviate her concern, he responded as a true *brujo malo*, 'Weed never dies.' He smiled in complicity and continued gossiping. He must have known that flying over the Yunque as a *brujo* was no longer a possibility for him as much as flying as a spirit would take just a bit longer.

The last time I saw Tonio was on the day I left Puerto Rico. He was in bed, and for the first time in the eighteen months I had known him he let me take his picture – and videotape a conversation between him and Haydée. With his usual, overtly sensuous self, he seemed to enjoy our attention to his playful self-display. He smiled and posed for my camera, even though bed-ridden and clothed only in baggy pyjamas. It was a joyful occasion for all three of us, one worthy – we all seemed to realize at that very moment – of being documented as a commemorative trophy. I kept a copy of these photos for myself and mailed two other sets to Haydée to share with Tonio. [See Figure 10.3.]

Now I have a copy of Tonio for myself. Possessing a part of him, his reflection on paper, makes me feel empowered. Somewhat like a *bruja* seizing one of the infinite links of chains of resemblance, I can invoke my memories of Tonio. But, curiously, I still feel apprehensive about showing these pictures for the first time in public. When I left that day, Tonio said that the next time I came to Puerto Rico he would not be

---

[10] Here I was inspired by Stoller's (1995) suggestion that colonial memories are embodied in spirit possession.

Figure 10.3 Tonio describing the trabajos he used to make

there anymore. I would like to regard his last gesture of allowing me to photograph him as a gift to me. [See Figure 10.4.]

On one sad day in December 1998, Haydée phoned me, crying, telling me that Tonio, her spiritual father, had just died. His wake and burial were like those of the most esteemed public figures. Hundreds of flower crowns, including the one I had sent from Philadelphia in recognition for his generosity to me, accompanied his way to the Loíza cemetery, where the mayor, in line with his public and personal duty to the *Brujo de Loíza*, the Witch-healer of Loíza, delivered the eulogy, lending to the event the official status usually accorded only to public figures. His eulogy was again a copy, empowering again.

As the successor of a long line of *brujos* in his father's lineage, the Lacéns, who had originated in Antigua, Barbados, during the slave past, Tonio had mimed their memories in countless magical works, visions and spirit possession. Flying over the Yunque might have been a quest to fly the imagined journeys of European witches, or those of African sorcerers; or maybe still to flutter in the ethereal space of creole saints, Congo deities, and nature and ancestor spirits – of the fierce Taíno, national heroes, and enlightened free-thinkers. His quest might have been aimed at mirroring the stages of spiritual development envisioned by *espiritismo*, that guide the upward path connecting the recent dead, the near-spirits, the ancient spirits and deities, and the enlightened ones.[11] His material death, what *espiritistas* refer to as 'disincarnation', might have initiated his path through several spiritual realms, bringing him into contact

---

[11] A similar form of journeying is reported by Michael Jackson in relation to Walpiri Dreaming: 'In sleep, unconsciousness, or death, the spirit (*pirlirrpa*) is said to go out of the body, often journeying to

Figure 10.4 Tonio's hands

with the spirits of other powerful *brujos* and closer to his intended and much anticipated flight over the Yunque, the paradise of *brujos* and timeless spirits: a flight perfected at death.[12] [See Figure 10.5.]

Figure 10.5 Tonio, the Brujo of Loíza

the country from which it originates – and it is to this same country that the spirit returns when the body dies' (2002: 275).

[12] A few months after Tonio's death in 1999, I presented an earlier version of this chapter at 'The Academic Storyteller', a session convened by Haim Hazan for the 34th World Congress of the International Institute of Sociology on Multiple Modernities in an Era of Globalization (Tel Aviv, Israel, 11–15 July 1999).

Coda

On 21 March 2000, at the age of 55 and only two years after Tonio's death, Haydée died of a massive heart attack while in the Dominican Republic. She had gone there in hopes of recovering from the overwhelming sorrow and depression brought on by the death, three months earlier, of Eliseo, the youngest and last surviving of her three children.

Their flight perfected at death.

References

Cabrera, Lydia. 1975 [1954]. *El Monte*. Miami: Ediciones Universal.

Cantwell, Robert. 1993. *Ethnomimesis: Folklore and the Representation of Culture*. Chapel Hill: University of North Carolina Press.

Clifford, James. 1997. *Routes: Travel and Translation in the Late Twentieth Century*. Cambridge, MA: Harvard University Press.

de Certeau, Michel. 1984. *The Practice of Everyday Life*. Berkeley: University of California Press.

Deleuze, Gilles. 1987. *A Thousand Plateaus: Capitalism and Schizophrenia*. Minneapolis: University of Minnesota Press.

Evans-Pritchard, E.E. 1976 [1937]. *Witchcraft, Oracles and Magic among the Azande*. Oxford: Clarendon Press.

Feld, Steven. 1990 [1982]. *Sound and Sentiment: Birds, Weeping, Poetics, and Song in Kaluli Expression*, second edition. Philadelphia: University of Pennsylvania Press.

Frazer, Sir James George. 1960 [1922]. *The Golden Bough: A Study in Magic and Religion*. vol. 1, New York: Macmillan.

Ginzburg, Carlo. 1983. *The Night Battles: Witchcraft and Agrarian Cults in the Sixteenth and Seventeenth Centuries*, trans. J.A.A. Tedeschi. Baltimore, MD: Johns Hopkins University Press.

Goethe, Johann W. von. 1982 [1816–17]. *Italian Journey: 1786–1788*, trans. W.H. Auden and E. Mayer. San Francisco, CA: North Point Press.

Gundaker, Grey. 1993. Tradition and Innovation in African American Yards. *African Arts*, 26(2): 58–71, 94–6.

Jackson, Michael. 2002. *The Politics of Storytelling: Violence, Transgression, and Intersubjectivity*. Copenhagen, University of Copenhagen: Museum Tusculanum Press.

Montesquieu, Charles de Secondat, baron de. 1964 [1721]. *The Persian Letters*, trans. G.R. Healy. Indianapolis, IN: Bobbs-Merrill.

Proust, Marcel. 1982. *Remembrance of Things Past*, trans. C.K. Scott Moncrieff and Terence Kilmartin. New York: Vintage Books.

Romberg, Raquel. 2003. *Witchcraft and Welfare: Spiritual Capital and the Business of Magic in Modern Puerto Rico*. Austin: University of Texas Press.

———. 2005. Ritual Piracy: Or Creolization with an Attitude. *New West Indian Guide*, 79 (3 and 4): 175–218.

Stoller, Paul. 1995. *Embodying Colonial Memories: Spirit Possession, Power, and the Hauka in West Africa*. New York: Routledge.

———. 1997. *Sensuous Scholarship*. Philadelphia: University of Pennsylvania Press.

Taussig, Michael. 1987. *Shamanism and Colonialism and the Wild Man: A Study of Terror and Healing*. Chicago, IL: The University of Chicago Press.

———. 1993. *Mimesis and Alterity, A Particular History of the Senses*. New York: Routledge.

Thomas, Keith. 1971. *Religion and the Decline of Magic: Studies of Popular Beliefs in Sixteenth- and Seventeenth-Century England*, London: Weidenfeld and Nicolson.

Thompson, Robert Farris. 1984. *Flash of the Spirit: African and Afro-American Art and Philosophy*. New York: Vintage.

———. 1993. *Face of the Gods: Art and Altars of Africa and the African Americas*. New York: Museum for African Art.

———. 2005. *Tango: Art History of Love*, New York: Pantheon.

Vidal, Teodoro. 1989. *Tradiciones en la brujería puertorriqueña*. San Juan: Ediciones Alba.

Chapter 11: The Anthropologist as a Nomad in Dangerous Fields and the Emergence of Cultural Criminology

Dina Siegel

Anthropologists are nomads, in more than just the geographical sense of the word. They are perfectly suited to play the role of 'interdisciplinary nomad', wandering from one intellectual area to another, breaking taboos, confronting stereotypes and questioning issues that are taken for granted. Today's anthropologists are in a position to contribute to other fields of social research by introducing their unique research methods. In the following, I will focus on criminology, a hybrid between social science and law. The research method of participant observation is rarely used in criminology. The reasons given vary from safety considerations to ethical grounds. However, in my view, the best criminological research is always based on ethnographic research.

Emanuel Marx's contribution to criminological research is perhaps underestimated, both by himself and by his students. In this context, two of his studies should be mentioned: his analysis of the social context of violent behaviour in an immigrant community (Marx 1967) and his description of drug smuggling patterns among the Bedouin of South Sinai (Marx 2006). In these studies, Marx demonstrates the advantages of ethnographic research in the study of crime, by showing that violence or drug smuggling are sometimes considered normal and acceptable behaviour in certain cultural environments, while decision-makers and law enforcement officials, disregarding the social-cultural context, perceive these activities as criminal or immoral.

I will attempt to illustrate the advantages of ethnographic research in the study of organized crime in general and my own study of the Russian Mafia in particular. I will suggest that other disciplines can benefit from the anthropological approach and research methods, and conclude by describing the emergence of cultural criminology.

An Interdisciplinary Mission

Real anthropologists have always wandered around the planet: from the diamond mines in Angola and Sierra Leone to the ghettos in big American cities to oldpeople's

homes in England or in Israel. However, the romantic stereotype of the anthropologist as an adventurer or an explorer is more or less passé. Modern and post-modern anthropologists are mainly involved in discourse on different theoretical issues, rather than in describing newly discovered tribes and places.

Emanuel Marx never thought of himself as a criminologist. However, there has always been a connection between his fields of interest and the study of crime. In the foreword to the first edition of Marx's book 'The social context of violent behaviour' (1976), Max Gluckman mentioned that according to Marx 'it was an invitation to give an address to a criminological congress which led him to his analysis of violent behaviour' (1976: x). Forty years later, Marx presented a paper on 'Hashish smuggling among the Bedouin of South Sinai' at a criminological conference on organized crime in Amsterdam, focusing on the problems of the criminalization of drugs in different societies.[1]

Anthropological fieldwork methods have seldom been used in criminology. To begin with, there is the problem of access to the field. Researchers have to be fluent in the language of the ethnic group they are studying. Secondly, there is the obstacle of how the group will react to the particular subject of the research. Thirdly, there is the problem of ethical restraint. Some criminologists have ethical objections to field research among criminals (Sutherland and Cressey 1960: 69). They seem to think that trying to get closer to informants necessarily means identifying with them. The fourth reason why fieldwork is not very common in criminology is the aspect of danger. Several criminologists have emphasized the personal risks involved in the ethnographic study of organized crime (Finckenauer and Waring 1998; Sluka 1990; Ferrell and Hamm 1998).

Organized Crime, Violence and Fieldwork

In spite of the abovementioned obstacles, some of the best studies on organized crime are nevertheless of an ethnographic nature. They were successful because they entailed participant observation methods (Ianni 1972; Polsky 1969; Adler 1985; Chambliss 1978). In his study of criminal families, Ianni emphasized the value of field research as follows: 'My training and subsequent field experience have convinced me that direct observation of human action is better than the collection of verbal statements about that action if one would understand how a social system functions' (1972: 182). Polsky interviewed poolroom players ('hustlers') and described their lives after years of participant observation. 'Experience with adult, unreformed, "serious" criminals in their natural environment ... has convinced me that if we are to make a major advance in our scientific understanding of criminal lifestyles, criminal sub-cultures, and their relation to the larger society, we must undertake genuine field research on these people'

---

[1] CIROC (Center for Information and Research on Organized Crime) seminar, April 2006, Amsterdam, Vrije Universiteit.

(Polsky 1969: 115). Similarly, Chambliss wrote that although field research is difficult, it is not impossible.

> It is possible to find out what is going on 'out there'. We are not permanently stuck with government reports and college students' responses. The data on organized crime as other presumably difficult-to-study events are much more available than we usually think. All we really have to do is to get out of our offices and onto the streets. The data are there; the problem is that too often sociologists are not. (Chambliss 1975: 39)

One of the main reasons for criminologists to avoid fieldwork methods of research is the threat of violence, which they may encounter during their study. Paradoxically, violence is one of the most characteristic aspects of criminal activity: the study of organized crime usually involves the study of violent behaviour as well. When studying violence, criminologists mainly use secondary sources, because it is difficult if not impossible to observe violent incidents during fieldwork.

Marx studied the relationship between immigrants and officials in Israeli development towns in 1962–64. He treated violent incidents as 'normal' and sometimes inevitable events, which must be explained within their social context. In the last paragraph of his conclusion on violence in Galilah, Marx mentions that research on violence requires an indirect approach. He suggests that anthropologists engaged in fieldwork should attach more importance to violent incidents and investigate them (Marx 1976: 111). This indirect approach was also crucial in my research on Russian organized crime in the Netherlands. My focus was on the images and perceptions of crime and criminals inside the Russianspeaking[2] community in the Netherlands and most of my informants were noncriminal immigrants, but on different occasions I came in contact with Russianspeakers who were regarded by their compatriots as criminals, some of them as 'extremely violent and dangerous'. Several were known as members of criminal organizations; others were involved in 'illicit business practices', or purposely violating Dutch law. My respondents would often tell stories of extreme violence (and volunteer their interpretation of events), but only once during my fieldwork did I actually witness a violent incident.

In the eyes of Russian-speakers, a violent criminal is someone involved in threats and killings. Anthropologists are familiar with this perception of crime. Among the Nuer of the Southern Sudan, a crime involves an actual killing (EvansPritchard 1951). The Bedouin of the Negev also considered disputes where blood is shed as a crime (Marx 1967). In modern states the perceptions of crime and violence are different, defined in law and with political implications. '... the greater the concentration of force-wielding

---

[2] The term 'Russian-speakers' includes people from all over the former Soviet Union, including republics that only recently gained their independence and where Russian is no longer the official language.

organizations in a society, the more inclusive is its legal definition of criminal violence' (Marx 1976: 16).

In my own research, I learned that Russian-speakers distinguish between the 'normal Mafia' and the 'tough Mafia', where the former is associated with traditional criminal activities such as extortion, prostitution and drug dealing. The 'tough Russian Mafia' is involved in penetrating the legal economic and political structures. This kind of Russian Mafia is considered by Russian-speakers as highly sophisticated and involved in the most complicated technological and financial operations. In this context, violence was inevitable for conquering their position in the criminal arena in the Netherlands. Contract killings are still viewed as the specialty of the Russian Mafia:[3] the fact that the killers are never found is seen as an indication of the Russian Mafia's professionalism, its *krutost* (toughness) and its unscrupulousness.

The Russian Mafia as an Anthropological Study

The first alarm bells on the 'Red Mafia' in the Netherlands went off in 1994, when the Dutch secret service (BVD) issued a serious warning to Dutch businessmen considering doing business in Russia. Its annual report predicted the imminent arrival of Red Mafias in the Netherlands (Jaarverslag BVD 1994). Two years earlier, the body of a Russian, supposedly the leader of a gang of Russian extortionists operating in Belgium and the Netherlands, was found in Amsterdam. Another early story about the Russian Mafia originated in The Hague, where the local underworld had forcibly removed a Russian-speaking mob from the Red Light district in 1994. When the Russians left the scene, they announced that they would be back. Although they failed in The Hague, some Russians eventually succeeded in pushing out local criminals from other red light districts, especially in the south of the country.

With the publication of the BVD report, a first official message was formulated: Russian Mafiosi are operating in the Netherlands and they present a threat to the Dutch economy and to democratic institutions. After the disappearance of the Russian Jewish businessman Boris Fastovski in 1995, the picture seemed to be getting clearer: what was happening in other European countries was about to happen in the Netherlands. The Russian Mafia was on its way; probably they were already active in some places. The wave of immigrants from the former Soviet Union to the Netherlands in the years 1990–96 and a series of killings and other criminal activities among Russian-speakers led to questions about the link between the immigrant community and the Russian Mafia. Dutch society seemed to have opened its doors not only to innocent immigrants who happened to speak Russian, but also to the Russian Mafia.

Various institutions, including the police, the secret service, municipalities, press and media, financial and commercial organizations were alarmed about this new phe-

---

[3] In the period between 1992 and 1997 there were eight liquidations of Russian businessmen in the Netherlands.

nomenon. Their reaction was to initiate fact finding and academic research. The Dutch government subsidized a number of investigations with the purpose of assessing the threat of Russian organized crime.

According to a theory on 'media hypes' or 'moral panic', real problems can be made out to be far greater than they really are. Who would be in a position to do this? Obviously parties with the 'power of definition': journalists, officials of the relevant institutions, politicians, etc. The Dutch press, the police and various government institutions not only called attention to certain incidents but also gathered, registered and presented their data, sometimes proposing different solutions to the problem.

At the time, journalists all over the world knew that the Russian Mafia made for great copy. 'The world-wide image of Moscow as a centre of a global communist conspiracy has been replaced, almost overnight, by the myth of the Russian Mafia' (Rutland and Kogan 1998: 139). Everybody had their own reasons for making the Mafia larger than it was. And like everywhere else in the world, the activities of the Russian Mafia were widely covered in the Dutch media. Gradually the composite portrait of the Russian 'Mafioso' in the Netherlands emerged: he was a rich, sophisticated and extremely violent criminal. While the press was collecting colourful stories about the bloodthirsty Russian Mafia, thereby creating a new 'public problem', criminal analysts and the police were asking questions about numbers, names and types of criminal activities. Statistics were of little help because of the small number of Russian-speaking immigrants in the Netherlands. A different approach and research method was needed; in this case an anthropological approach.

My research was one of many studies on Russian organized crime conducted during the time. But, while the interest of the institution which subsidized my research[4] was no different from that of other organizations, namely assessing the seriousness of the threat of Russian organized crime in the Netherlands, the purpose of my research went beyond a description of organized crime among a group of Russian immigrants, or an analysis of statistical data. It dealt with the Russian-speaking immigrants, and more specifically with questions of how they viewed and explained the activities of the Russian Mafia in the Netherlands. My research was actually one of the first attempts to study Russian organized crime empirically, from inside the community, by analysing the perceptions and speculations of Russian-speakers on the subject of Russian crime and violence in their midst.

* * *

My fieldwork took place in the period between 1999 and 2001. It turned out to be not all that difficult to find informants. In fact, few topics of conversation seemed to be more popular among Russian-speakers than Russian 'business', i.e. organized crime. Before approaching Russian-speaking immigrants (some of whom I had met in previous

---

[4] WODC – Wetenschappelijk Onderzoek en Documentatie Centrum – Scientific Research and Documentation Centre at the Dutch Ministry of Justice, The Hague.

years) with questions, I had to find a way to present myself and the subject of my study. It seemed clear to me at the time that it would not be possible to tell them about my interest in the Russian Mafia in the Netherlands. The most likely reactions would have varied from either refusal to talk at all, to laughter or jokes ('Do I look like a Russian Mafioso?'). The next best thing I could think of was to tell my potential informants that I was writing a book on Russian business in the Netherlands. This sounded like a very neutral idea; there would not be any focus on specific individual businessmen or criminal activities. I also decided that I would not ask for official interviews or even names. The reactions I received came as a surprise:

> Oh, you write about business, you mean the Russian Mafia!
>
> Business? You mean illegal business!
>
> Russian business does not exist – there is only the Russian Mafia!

Some respondents immediately tried to explain why they thought Russian business was suspicious or criminal. Usually their explanations were based on personal contacts with a specific businessman or businesswoman, either in the Netherlands or in their 'previous lives' in the former Soviet Union. There were also general statements to be heard, such as 'All Russians here are criminals and bandits', or speculations and noisy discussions about murder cases that had taken place in the Netherlands between 1992 and 1997. Some informants had known the victims or their families personally, or they had heard about them in various stories told by other immigrants. My interest in 'business' was in many cases reduced by my informants themselves to 'illegal business' or 'organized crime' and the murdered businessmen were often used as examples.

My informants provided me with a wealth of information on various aspects of organized crime, on the backgrounds and the logic of specific criminal activities, such as smuggling, kidnappings and even killings. They explained the background of the rise and development of the Russian mafia from their own point of view, and compared their own views with Dutch media and police perceptions. There was indeed a discrepancy between the official presentation of Russian organized crime in the Netherlands and its image among the Russian-speaking immigrants. While the Dutch police and media were looking for traditional Mafia activities in the immigrant community, such as drugs and prostitution, Russian-speakers thought of the real Mafia as a sophisticated transnational organization dealing in economic and financial crimes. Petty crimes (in their view), especially the ones where no apparent violence was involved, were not viewed as connected to organized crime; some were not even considered to be crimes at all.

In Russia, ideas about crime and criminals come from a long and rich tradition of images and codes of behaviour, developed in Tsarist, Soviet and post-reform Russia. Russian organized crime is not a recent development stemming from the period of post-communist reforms, as some scholars claim, but a deep-rooted social and historical

phenomenon. The social banditry as protest against the Tsarist regime in Russia and later the development of the corrupt system of Soviet economy and politics, created specific norms and rules in which unique perceptions on crime and organized crime evolved. In this set of perceptions a distinction was made between violent crimes against an individual and mass crimes against society. The Soviet people had to 'beat the system' in order to survive. Since there was a discrepancy between the official ideology and the difficult conditions of daily life, stealing from the workplace, corruption and abuse of power were not considered crimes. In the times of Gorbachev there was even more confusion about the ideas surrounding business and crime. The old nomenclatural bureaucrats and former KGB officials became bank directors or businessmen. Vory v zakone ('thieves under the law') and other criminals also entered the business world. The criminal groups took advantage of the confusion and uncertainty of the people. There was a thin line between business and crime.

The Russian-speaking immigrants brought some of these perceptions to the West. These included, for example, the idea that government officials in the Netherlands must be as corrupt as Russian officials, or the idea that illegal migrants are 'heroes' who have the courage to challenge official policy. The Russianspeakers often did not distinguish between 'legal' and 'illegal' in business, because the two were seen as interconnected. Business in Russia (or with Russia) was considered to be linked to the Mafia, and protection (*krysha* or 'roof') was viewed as a necessity. Some types of business were perceived as 'exclusively criminal', such as the car-business, the trade in oil and metals, and the diamond business. Russian-speaking partners in the Netherlands were sometimes used by criminals for laundering money and evading Russian taxes.

I discovered that most Russian criminals viewed the Netherlands as a 'Mafiafree country', where they could safely settle their families while they themselves continued to operate internationally, with their headquarters in Russia. They bought expensive houses for their families, financed the education of their children, provided them with all they needed and saw to it that their family was financially and materially secure. They came to the Netherlands to relax, to invest money, to meet with business partners, and in some cases to escape problems in Russia, such as rivalry with other criminal groups. Some of them considered the Netherlands too small a country to be worth the effort of serious criminal activity. Paradoxically, because the country was perceived as 'Mafia-free', many Russian criminals preferred to come to the Netherlands, either for the sake of their families or to seek refuge from other criminals. In many cases their idea of the Netherlands as a safe country, where all citizens are protected from crime and violence, turned out to be mistaken.

Smuggling and Organized Crime

One of the many activities of Russian organized crime in the Netherlands I encountered during my fieldwork was the smuggling of art and drugs. The smuggling of art

was regarded in Russian circles as a typical and popular criminal activity of the Russian mafia. Since the beginning of the 1990s, the Russian authorities have reported the disappearance of thousands of items from museums and churches all around Russia. Many of these items were later found in West Europe, including some impressive art collections (Siegel 2005; Rottenberg 1999). The profits that can be made from art smuggling are the main reason for criminals to accept the risks involved.

The smuggling of drugs, on the other hand, is about more than just financial gain. Although Russian criminals do not participate significantly in the international drugs market, drugs are more and more often smuggled from the Netherlands to Russia and other former Soviet republics. The most popular drug is XTC, not only because the pills are small and difficult to trace by customs control, but also because the famous XTC pills 'made in Holland' earns criminals the reputation of belonging to a large transnational organization (Blickman et al. 2003), comprised Dutch producers, Israelis financiers and organizers of transports, and Russian couriers and distributors on the local markets.

In his article on the smuggling of hashish through the Sinai, Marx describes a similar picture: '... the drug is purveyed by powerful international organizations, and the Bedouin are proud to play a significant part in a great commercial enterprise' (Marx 2006: 5). The Bedouin viewed the chiefs of smuggling gangs as entrepreneurs who had brought them a better standard of living. Awareness of the fact that there are risks involved in the smuggling of drugs and art does not stop either Bedouin hashish smugglers or Russian criminal organizations from continuing their illicit enterprises.

The answer to the question of whether the data gathered with the help of anthropological methods of research could have been collected through other methods, such as analysing police files or interviewing law enforcement officials, must be negative. Participant observation may be considered by some to be dangerous, difficult or even unethical, but it is without a doubt a method which produces a wealth of valuable information on various aspects of crime. Listening to Russian-speakers analysing the Russian Mafia, discussing its structure, its activities, and specific individuals connected to criminal organizations, provided me with detailed and useful information. The Bedouin drug-dealers were equally ready to provide their insights into the smuggling of hashish.

Cultural Criminology

Ethnographic research inside 'dangerous' communities can provide us with a unique insight into crime and the people involved. It can also analyse the inner social dynamics of establishing links with criminal groups, and perhaps most importantly: research inside a community can show the discrepancies between official attitudes and prevailing stereotypes and the emic, unofficial images of crime and Mafia. Ideas like this are endorsed by a growing number of Dutch criminologists who have applied anthropological methods to the study of various organized crime groups in the Netherlands, such

as the Turkish mafia (Bovenkerk and Yesilgoz 1998) and Colombian cocaine traffickers (Zaitch 2004).

Over the last few years, this new trend in criminology has become increasingly popular and influential. Many criminologists are beginning to realize that numbers, dark numbers, surveys, non-response, over-response, self-report studies, police files, or the findings of victimologists, are not necessarily sufficient to arrive at an accurate description of crime and criminal organizations. Understanding crime – organized or otherwise – should not be left to statisticians or, to use Presdee's term, 'administrative criminologists' (2000: 276).

The new trend is known as 'cultural criminology'. Cultural criminology advocates a 'methodology of attentiveness', meaning an 'ethnography immersed in culture and interested in lifestyles(s), the symbolic, the aesthetic, and the visual' (Hayward and Young 2004: 268). Crime has become more than just facts and figures. 'Crime is as much about emotions, hatred, anger, frustration, excitement and love – as it is about poverty, possessing and wealth' (Presdee 2000: 4). It is important to understand the performance of criminal acts as well as the motives and background of the offender, his environment, and his interpretation of crime. Taking risks, carefully and professionally planning each step, followed by 'the feeling of 'getting away with it', that is part and parcel of doing wrong, the buzz and excitement of the act of doing wrong itself, of living on the 'edge' of law and order are all emotions' (ibid.: 5) These emotions must be taken into account in the study of criminals and criminality and this kind of research is impossible to conduct without 'being there'. No consensus has yet been reached on the definition of cultural criminology, as it includes a wide range of disciplines, but there can be no dispute that the nomadic anthropologist has a significant contribution to make to the study of crime.

References

Adler, Patricia. 1985. *Wheeling and Dealing: An Ethnography of an Upper-Level Drug Dealing and Smuggling Community*. New York: Colombia University Press.

Blickman, Tom., Korf, Dirk., Siegel, Dina and Zaitch, Damian. 2003. Synthetic Drug Trafficking in Amsterdam, in *Synthetic Drugs Trafficking in Three European Cities: Major Trends and the Involvement of Organized Crime*. Final Report, Gruppo Abele, TNI – IECAH, UNICRI.

Bovenkerk, Frank and Yesilgoz, Yucel. 1998. *De Maffia van Turkije*. Amsterdam: Meulenhoff.

Chambliss, William. 1978. *On the Take – From Petty Crooks to Presidents*. Bloomington: Indiana University Press.

Evans-Pritchard, Edward E. 1951. *Kinship and Marriage among the Nuer*. Oxford: Clarendon Press.

Ferrell, Jeff and Hamm, Mark. 1998. *Ethnography at the Edge: Crime, Deviance and Field Research.* Boston, MA: Northeastern University Press.

Finckenauer, James O. and Waring, Elin J. 1998 *Russian Mafia in America: Immigration, Culture and Crime.* Boston, MA: Northeastern University Press.

Hayward, Keith and Young, Jock. 2004. Cultural criminology: Some notes on the script. *Theoretical Criminology.* Special issue, 8(3): 259–73.

Ianni, Francis. 1972. *A Family Business: Kinship and Social Control in Organized Crime.* New York: Russell Sage Foundation.

Marx, Emanuel. 1967. *Bedouin of the Negev.* Manchester: Manchester University Press.

——. 1976. *The Social Context of Violent Behaviour.* London, Henley and Boston: Routledge and Kegan Paul.

——. 2006 Hashish Smuggling by Bedouin in South Sinai, Paper presented at the CIROC seminar, March 15, 2006, Vrije Universiteit Amsterdam

Polsky, Ned. 1969. *Hustlers, Beats and Others.* New York: The Lyons Press.

Presdee, Mike. 2000. *Cultural Criminology and the Carnival of Crime.* London and New York: Routledge, Taylor and Francis Group.

Rottenberg, Hella. 1999. *Meesters, maradeurs. De lotgevallen van de collectieChardzjiev.* Amsterdam: Uitgeverij Jan Mets.

Rutland, Peter and Kogan, Natasha. 1998. The Russian Mafia: Between Hype and Reality. *Transitions*, March: 24–34.

Siegel, Dina. 2005. *Russische bizniz.* Amsterdam: Meulenhoff.

Sluka, Jeff A. 1990. Participant Observation in Violent Social Contexts. *Human Organization*, 49(2): 114–26.

Sutherland, Edwin and Cressey, Donald. 1960. *Principles of Criminology.* Philadelphia, PA: J.B. Lippincott.

Zaitch, Damian. 2002. *Trafficking Cocaine: Colombian Drug Entrepreneurs in the Netherlands.* The Hague: Kluwer Law International.

Chapter 12: The Straw in Anthropologists' Boots: Studying Nobility in Poland

Longina Jakubowska

The term 'anthropological fieldwork' is a gloss that covers a promiscuous array of different research strategies, mostly qualitative, often anecdotal and invariably personal and subjective (Shore 2002: 12).

In this essay, I intend to reflect on the project of anthropological fieldwork through the lens of the less travelled road of ethnographic research. After engaging in a long-term ethnographic fieldwork with the Bedouin in the Negev Desert of Israel, I undertook research in my native country of Poland. There were various reasons for that, one of them being my increasing sense of alienation from the country in which I was born, raised and educated following my long absence and amplified by the changes that swept through Eastern Europe in the 1990s. Another reason was my newly found interest in inertia and durability of social structures in spite of reforms and revolutions. My attention was caught by Polish nobility, which at the time made a powerful come back on the national scene. In those early years after 1989, the gentry experienced a surge of popularity. With each consecutive visit to Poland, I found more books on the market, some written by the gentry and some about them. They multiplied with a daunting speed: sepia colour photo albums, gentry registries, genealogies of aristocratic families, histories of noble houses, who-is-who in the past and the present, and above all, memoirs of all kinds. It seemed that everyone was engaged in the business of reminiscing. The fascination of the public with everything noble appeared insatiable at that time. The gentry – who suddenly became visible – were almost giddy with delight. Put on public stage again, they resurrected relics of their former life style. There were noble balls, St Hubert's Day foxhunts and glamorous weddings, the splendour of which equalled royal ceremonies in Western Europe. Obituaries began to specify titles and former possessions. The Order of Malta conferred new members. Within few years, no less than four journals appeared on the market all explicitly catering to the gentry's interests.

My question thus became how did the Polish gentry manage to persist and reproduce itself in a hostile political environment void of its former means of existence? What strategies did they historically develop in order to maintain group identity and

cohesiveness? Which properties and attributes facilitated their transformation into a meaningful category of the new social structure? While the research inquired into the processes of elite reproduction, it also asked a larger question of features constituent of social groups and societies, which allow them (or not) to endure over long periods in history across various social and political regimes.

Beginning in the 1980s, the shift to 'anthropology at home' brought a growing number of anthropologists to engage in the study of their own society. Often writing for local audiences in their own languages, they narrowed the gap between the researcher and the researched and raised questions of reflexivity and personal aspect of fieldwork. Along the way this new approach to the practice of anthropology problematized and deconstructed many categories, which had been part of traditional epistemology since inception of the discipline. Among others, such categories as 'anthropologist – native', or 'outsider – insider' also collapsed. It is now generally agreed that all ethnographers are positioned subjects constrained by their membership in a particular society at a particular period in history, their gender, age, class, and by the power inequality that often exists between the observed and the interpreters of history and society (Ohnuki-Tierney 1990; Jackson 1987; Okely and Callaway 1992).

Born and raised in Poland, I shared many features with the gentry that come about from living in the same country, understanding its ideological and cultural underpinnings and the practices by which it is governed. And yet I was as much a stranger to their world as I was to the Bedouin of the Negev Desert with whom I worked a decade earlier. In both field situations my position as a researcher was mediated by a perception each population held of me; each required a different research strategy and a different presentation of the self.

In both cases my nationality, more than any other identity, such as gender for example, worked in my favour and in each it was to my benefit to be 'Polish' although for different reasons. In Israel, my nationality mediated between conflicted loyalties and ambiguous identities of the settled Arab Muslim Bedouin community living in a Jewish state. In Poland, it provided a common general cultural background, which facilitated a flow of communication based on a shared understanding of the system in which both interlocutors and I, then as an individual in a national community and now as the researcher, functioned. To an extent, these commonalities, together with my foreign credentials and academic standing also mediated class distance (Lotter 2004). Being educated in the United States, where I also held a university appointment, positioned me within the circuit of intelligentsia, a cultural niche of Polish gentry. Together with worldliness and foreign experience, which have always been of importance to them, it facilitated yet another cross point of our respective identities. And yet there was a sea of difference between us; my parents were middle class professionals and the family did not have deep genealogical memory, which – given the near absence of bourgeoisie in Polish society – made me suspect that my ancestors must have been once the peasant subjects of the gentry I studied.

Elites are usually defined as groups that control specific resources by means of which they acquire political power and material advantage (Pina-Cabral and Pedroso de Lima 2000: 2). Anthropological literature concerning elites concentrates on the power that elites exert in a given society, and subsequently the power they might exert on the researcher and the research outcome. The latter is of importance, and indeed occupies a significant space in discussions on methodologies and dilemmas inherent in elite studies, primarily because of the asymmetrical relationship intrinsic to the relationship between the researcher and the subject of study. This is nothing new, of course, except that this time power balance is skewed in the opposite direction from the usual approach in anthropological research of studying the underprivileged.

It is the very nature of elites to be exclusive and inaccessible. Indeed, the aura of mystique and inherent worthiness, which surrounds them, is predicated upon it, and the reluctance to engage in commonplace interlocution constitutes a boundary marker. This is particularly true of hereditary elites, and above all nobility, among whom kinship, descent, 'shared essence' and the power derived from their place in history provide a basis for exclusivity which makes the line that divides those who are 'in' and those who are 'out' rigid. Yet there is certain vagueness about them; never flashy, always low-key, they create an impression of easiness and informality and still remain just slightly unattainable, producing sort of a glass door effect. Performance is here, of course, of essence. Just as a habit of servitude is incorporated in the behaviour of a servile group by way of their own body deportment, so is the habit of superiority among the elite. A noble incarnates embodied authority and, more importantly, does it not by mechanically executing codes but by the ease of practised performance. This sets him apart from the class other, who, as hard as he may try, is unable to embody the acknowledged model effortlessly and gracefully (Connerton 1989). Whether the elites cultivate such a demeanour and the effect is premeditated, or whether the reactions of the outsiders are formed by their own class perceptions of self, their stance creates an obstacle to interaction. Since structures of authority and prestige are embedded in all members of the collective social body, Herzfeld's analogy to bureaucracy is instructive here: 'If there are no clients to affirm their superiority,' he writes, 'that superiority has no grounds' (2000: 230).

A considerable attention in the literature on elite studies is paid to methodological difficulties in accessing elites, be they political, economic, managerial, or noble (Pina-Cabral, Shore, etc.). This was not my experience, possibly because Polish nobles were in some respects different: they were once a large and powerful force that formed the nation's core and shaped its destiny perhaps to a greater extent than their counterparts in other European countries but their fortune waned and the socialist regime made them disappear from the public forum. Forced into silence for half a century, they had a powerful need to tell their story. By the time I began my research in 1994, the public attention given to them began to wear off. The nation's attention shifted and its fascination with nobility and history in general was steadily overridden by the severe economic problems at hand. The gentry became more careful about granting

interviews expressing a wish for a more 'scientific' approach but their own desire to be heard was not yet assuaged. Most importantly, I fitted into their current agenda.

Heavily relying on the discourse of victimization privileged after 1989, gentry advocated resurrection of the group in the public sphere, recognition of injustices inflicted upon them, and most importantly, called for a revision of the land reform, which they proclaimed an unlawful act. Reprivatization high on the national agenda, they lobbied for the return of the property unlawfully, or so argued, seized from them in the course of the land reform in 1944/5. The subject proved to be more controversial than the gentry had expected. Not surprisingly, peasants and parties that represented them rejected the proposal. In an effort to deal with the resistance, gentry representatives reduced their request to what in the land reform parlance became known as 'residuaries', mainly manors and parks which surround them. Since these had been converted into recreational, scientific, or housing facilities, the request put their former owners in a direct conflict with those agencies and the local population that made use of them.[1] This created a situation in which the gentry became willing and even eager interlocutors, who used the interviews as an opportunity to voice their views, rectify the negative image constructed of them by the communist propaganda, and set their personal and collective record straight. Thus the nobility were willing to speak and so the problem was not gaining access to informants but the fact that they were telling the same story; what would have under other circumstances generated enthusiasm, made me wonder. Contrary to the practice of community-oriented anthropology when oftentimes ethnographers become spokespersons on behalf of the community they study, the challenge was to resist their agendas.

This is not to say that pursuing own agendas, controlling information, and possibly affecting the research program is a unique situation. During my fieldwork among Bedouin, I was discouraged to pursue a particular line of inquiry so as not to stumble on political activities of an informant, of which I only became aware many years later. The cunning use of cultural tropes by each of us, in this case gender behaviour, family honour, and authority structure in the family, held us jostling for power until finally the family patriarch resolved the situation. What is striking about the gentry and what sets them apart is their belief in the inherent worthiness of the story they are willing to share in comparison to the cautious attitude I often encountered in my work with the Bedouin. This is, perhaps indirectly, related to power. One is less vulnerable if one can control the story, its content flow, and ultimately its uses, than one who has no such capability. Furthermore, the willingness to impart one's life story is related to the belief in an inherent value of that story; the higher position one presumes in the social structure, the more conviction this belief assumes.

---

[1] Not all gentry support reprivatization; many accepted their redefined position in the national community, and although they welcome the revival of nobility, they also acknowledge the impossibility of returning to the old social structure and the life style that it had facilitated.

As is common in elite studies, I pursued what Rosa Luhrman called 'appointment anthropology' (Luhrman 1996). The gentry are dispersed forming no residential community. To my great surprise, aristocrats, descendants of titled families with old genealogies, were easiest to approach. First, they had become visible through the media coverage. Secondly, their historically high profile prompted instantaneous name recognition while names of less 'distinguished' gentry might not be immediately identified as noble. If one met a Radziwiłł, one knew it and was smug to point it out. Also, Polish aristocracy prides itself on its commitment and service to the country and perhaps also for this reason they appeared amiable to granting interviews. Conversely, petty gentry were more difficult to trace and less eager to relate their experiences; the more information they revealed of themselves, the more apparent became their minor position in the gentry hierarchy. As one of the consequences of socialist era was levelling of the entire group, which might have once been as large as ten per cent of the entire population, it was to their benefit to gloss over their particular social standing.

Conversations took place in gentry's own environment, be it an apartment or a house, mostly in the urban metropolis of Warsaw, Kraków, and Wrocław. They were distinct in their location – usually in parts of the city that were fortunate to survive the ravages of war, the buildings retained at least some of their former elegance, which set them apart from the impersonal concrete and steel high-rises of socialist apartment blocks. Each interview was taped, transcribed and if requested also authorized. They lasted, on average, four hours and, when this interval was insufficient, another session was scheduled. Interlocutors often followed up themselves calling with additional information, or providing documents they thought useful to my research. Given the forum, gentry were in control of the content of the conversation, the story, and the message they wanted to impart. In effect, they were directing the show to which I was an audience.

Because of the subject matter and the duration of the interview, conversations were emotionally and physically taxing. Nonetheless even the eldest of them (and many were of advanced age) demonstrated great presence, clarity of mind, and a remarkable endurance, on occasion greater than mine. It was hard not to be impressed by their stamina on the one hand, and the ambience of their residences on the other. Unintentionally, this registered in the written text because, as one of the readers of my manuscript remarked, I seemed to be in awe of nobility. I was certainly bewildered by the parallel cultural universes, that of the gentry and mine.

Diary entry, 1994

> I was introduced to Mrs S. by TH, a friend from my university days in
> Warsaw. Of gentry's origin himself, he knows her daughter professionally,
> for they both work at the Polish Academy of Sciences, as well as socially
> because he is befriended with her grandson. TH made the appointment for
> us on an afternoon a few days after Easter. This was my first interview
> and I was not sure what to expect. I had to think of what to wear, perhaps

something elegant yet simple. Etiquette was important here and I felt ill at ease. I had to live up to the expectations of TH – his reputation was at stake. He was wearing a jacket and a tie. We came to a three-story house with a gate and an intercom system. The apartment was spacious, with a long entrance hall and rooms to both sides. It looked warm and cozy but the variety of shapes surprised me and the doors – so many of them – were mystifying. Where did they lead and what was behind them? So different from the socialist mass produced apartments with their predictability of spatial arrangements and standard multi-purpose furniture, all of which one can take in with just a single glance, the likes of which I grew up in.

The sitting room – a prerogative of the blessed few in Poland – was round with large windows. I could not tell whether the furniture was valuable or just had an air of antiquity about it; there was an old-fashioned tiled coal burning stove, next to it a rocking chair with a hand-woven blanket thrown over its back, heavy curtains. We were seated at the tea table laden with delicate china. The lady, who entered shortly and was introduced as Mrs S., fit the surroundings perfectly – old and distinguished looking. She must have been a beautiful woman once. Her grey hair braided in a bun, she appeared cheerful and dignified. I felt as if I entered a stage set, or a film script about an old gentry family. Tea was served on Wedgewood china accompanied by cakes, biscuits and homemade preserves. Her grandson entered and there was some exchange about a letter she was helping him write in English.

We spoke of politics, local and worldwide. She was neither pleased with Wałęsa nor Clinton. She had one good thing to say about the latter; the word had it that he was about to appoint a Polish émigré aristocrat, Mr Rey, to the post of ambassador in Warsaw. Mr Rey was 'representative' (as in good-looking) and 'well connected' (as in with good family connections), Mrs S. stated. He was a family relation of hers.

My interlocutors were with no exception charming, intelligent, 'cultured' people bearing unmistaken marks of gentry's habitus. As I came to see during numerous visits that followed this initial interview, they commonly surround themselves with objects from the past, a bricolage of anything and everything that is reminiscent of life in a manor. It appears that as the past recedes, they seek to evoke it by preserving its relics. Antique furniture of all styles, ancestors' portraits no matter what their artistic merit, grandmother's trousseau china, family photographs, chandeliers rescued from the family mansion crowd the living space and overwhelm the frail figures in their midst. More than memory objects of youth foregone, or vanish era, it is a style which features in dwellings of younger and older generations alike. Fragments of the past, reminders of by-gone ways, memorabilia, if you will, they are also signifiers of the gentry identity, statements of status, material manifestations of intrinsic value which link the personal, the familial-genealogical, and the class with the making of Polish history. The significance of these objects is not located in their monetary value, but in the fact of having been in the possession of the family which in itself bestows

exceptional properties upon them derived from their origin in the family seat and the locus of gentry identity – the manor. The atmosphere they generate is indicative of their expressive power and it is impossible not to note the objects' performative capacity and their effect on human behaviour that sets those who belong apart from those who do not.

Distinct by their grooming habits, manners, speech patterns, the total way of presentation of self, nobility stood apart from the 'common folk'. Gentry women seemed particularly easy to identify, perhaps because they found it harder to shed, or hide, the numerous traces of cultivated distinction inscribed onto their bodies by centuries of tradition and lifelong practice. Their past, as it were, was locked in habitual memory. Mnemonic of the body manifested itself in practices of everyday life: in the hats women wore, in the manner they sipped their tea, in the gentility of their vocabulary, in the fact that they kept nannies. Just as the gentry can instantaneously recognize each other, so are the non-gentry alert to distinction manifested in body deportment. My question whether such-and-such was of gentry origin often elicited the answer that he must be because he looks the part. The explanation that followed was as vague as the image itself. This 'something' so difficult to define is what distinguishes many gentry from among others – a bearing worn as if second skin – is what Bourdieu (1984) calls habitus and the gentry 'race' (pol. *rasa*).[2] 'Race' connotes a demeanour and an attitude but most significantly the physical appearance, separate and yet inseparable from the sanguine poise generated from within. Just what precisely this idealized purity embodies is difficult to pinpoint but 'race' certainly included regular facial features, well-formed body, a resounding voice and vigorous stride, a polite but arrogant air, a confident grace and a sense of authority, in brief, an entire habitus, which marks the contrast between the appearances of the mighty and the lowly and which do not necessarily originate in different genetic stock, but in socially inherited differences in wealth, prestige, and power.

Although the past is continually and involuntarily re-enacted in the present conduct, that conduct is also purposefully kept, even cultivated. In the absence of property to transmit, a code of conduct, firmly regulated behaviours, and strict manners become the social badge that distinguishes the gentry from the 'common people'. As observed by Simmel, 'the more precarious is the material basis for one's existence, the weaker the moral relevance of higher classes, the more significant becomes the *personal art of existence*' (1971: 209; italics in original). In the midst of a changed and at times hostile environment, the maintenance of such conduct became ever more important. Impeccably dressed with pressed shirts and polished shoes, the gentry would sit down to a table decked with crested china even if they were to be served a simple dish

---

[2] There is a vague relation between the common definition of the term and the gentry's usage of it. One can speculate that 'race' refers to the myth of Sarmatian origins, the belief of the Polish gentry as descendants of eastern warriors conquering indigenous peasant populations, genetically separate and inferior. Remnants of this eastern tradition could be found in male clothing worn on ritual occasions, in the popularity of oriental carpets adorning mansion walls and curved swords hang on top of them.

of potatoes. The self-imposed discipline and the insistence on preserving standards of behaviour and proper habits reminded them about whom they once were and informed the younger generation about how to be, ultimately helping to assert and safeguard their social superiority. These inherited and inculcated bodily practices of distinction perpetuated class differences and challenged the overtly democratic and egalitarian social order. Even the necessity to work did not affect their sense of decorum and every effort was made to appear dignified, polished, and untainted by the growing influence of popular plebeian culture. The latter was traditionally conceptualized as 'chamstwo', an equivalent of common and uncivilized behaviour, a property of the masses. Categorical opposition between 'lord' and 'plebes' lay at the very foundation of the gentry identity and transgressing it was a mark of declassation. Moreover, 'gentry culture' was synonymous with 'culture'; guarding it against intrusions of non-culture, or 'chamstwo', was vital to the integrity of the gentry as a group, a group, which also made a special claim on the national history and Polishness. Hence even their language registrar served to protect the purity of Polish language both in style and in spirit – through cultivating the correct grammar, vocabulary, and forms of address – against sovietization, Russification, or simply vulgarism. Description of an adversary in spoken or written narratives – an official of the regime, a communist party or agricultural collective's member – indicate every slip of grammar, incorrect use of a word, an accent in an attempt to discredit the person as capable, qualified, morally authorized, or even Polish (the equally damning alternatives of which were Jewish or Russian). As an utter disapproval of someone with a high position in the socialist reality, ambition, and perhaps education, which the gentry thought not commensurable with class origins, many nobles used the expression of 'straw still sticking from his/her boots', referring to the habit of poor peasantry to insulate their shoes in a freezing weather.

The socialist structures of interdependency might have diluted the gentry's distinctiveness if it were not for their conscious resistance to the social mixing of classes. In the absence of other status markers, purity of blood with its correlate of endogamy protected against penetration by class others. Hence the ability to recognize and place people in an appropriate social category was essential to the preservation of group's integrity. The idiom of 'family' is commonly used to determine whether one has origins in common, or is of common origin.

One day in 1995, I gave a lift in a taxi to a nobleman whom I had just interviewed. After dropping him off in front of his villa on an elegant and quiet Warsaw street, the driver turned to me and, apologizing in advance for asking the question, inquired whether the man was Count X himself. Indeed, he was, I answered and amused at the situation, I inquired about how he came to know the Count. The taxi driver's wife used to be a classmate of X's daughter. Once they both visited the Xs in their summerhouse in a posh suburb of Warsaw. He vividly remembered Mrs X greeting him with a polite inquiry 'which family are you from?', his utter lack of comprehension of it, and a dim-witted answer: 'a working class family, madam'. Since 'family' as a manifestation of

202

social origin did not exist as a category among the working class, he found himself at a loss about the answer. The memory of this question survived the twenty years that separated the incident from the taxi ride with Count X and was still painful because it was an apparent mark of exclusion.

Asking about one's 'family', 'family' implying here one's origin, is a means of finding information about a person's social standing. Having a 'family' implies a place in the historical chain of ancestors, known predecessors, a succession of persons who long after their demise still kept their individuality, and hence corporeality, because their names were registered in the records of memory, a text, a portrait. It is not that working class people do not have ancestors but that their ancestors had become lost in historical anonymity, their names had vanished from records and memory, and hence they appear as if they had not existed at all. They do not have 'family' in the sense the gentry has it for their roots are not traceable in societal history and memory of their descendants alike.

Quickly it also became apparent that it was of importance to my interlocutors whether I was of gentry or not and, as my research progressed, I began to expect a question to that effect. Most made an effort to locate me in the gentry social milieu, browsing through their memory to recall all the Jakubowskis they have ever known as if to forge a bond, to spark a connection which when reactivated would comfortably place us on the same level. My origins, as well as all the other gentry attributes I have so far described, consistently placed me out of their cultural universe thereby making the issue of anthropologist's 'belonging' moot; I wondered whether they could see the straw sticking out of my boots. In accordance with the gentry's modus operandi, interviews had a performative dimension, and the presentation of self was highly ritualized. Having to create an adequate persona through attentiveness to hypercorrect Polish, proper manners, and appropriate forms of address, I never felt at ease preoccupied by the stage and the performance as much as the content of the conversation.

In the practice of traditional community-oriented anthropology, ethnographers usually develop a close, and often personal, relationship with the community, as I have done during my research in the Bedouin community. Over the years I followed their lives, saw children get married, attended life transition ceremonies, mourned deaths, gave advise on educational and professional careers, and received them in my home. As was the dream of my generation of anthropologists, I habitually referred to the various family members with whom I lived during intermittent fieldwork as 'my Bedouin mother', 'father', 'brother', and so on. Empathy for interlocutors as individuals and as members of a larger body, on the other hand, which is normally taken for granted, becomes problematic in the study of elite groups especially if it is a study of elite in an ethnographer's 'home' society. As observed by Marcus (1983), elite research requires normative distancing beyond the usual relativism and even then it occurs in an ideologically charged atmosphere. The interview situation, the character of my visits, the nature of reminiscences about lives sketched with broad strokes and shared with a stranger were not conducive to establishing intimate relations with the informants

and did indeed not yield any beyond some sympathies and some antipathies. I empathized with the traumas brought by the war, dispossession and displacement, which are, in essence, similar in its pattern to the dispossession, dispersal and discrimination of the Negev Bedouin. I felt saddened by individual tragedies that befell many and touched by the aging figures in the midst of spaces crowded with objects of the past, their hands shaking while drinking tea from the precious china cups but was also irritated by their unyielding claim over the definition of Polishness. Some statements made me wince, such as disdain for their former subjects, unequivocal support of ultra-conservative Catholic Church, dormant anti-Semitism. Although I could analyse and understand the dilemmas of the gentry, I was troubled by their request for reprivatization and found ideological differences difficult to overcome. I obviously disagreed with the Bedouin on many issues as well, but I found it easier to relativize, the task of which is harder when one is subject to the same structures governing inclusion and exclusion. While in the process of my research I learned a great deal about Poland, I also became more conscious of the deep divisions within the society and the undertaking left me more of an 'outsider' then when I started.

Even if empathy does not become a personal problem for the ethnographer of elites and she manages to suspend normative judgment, as noted by Marcus,

> reactions of the readers are nonetheless just as likely to be normatively based, since elite research of any kind has so routinely been received in an ideological atmosphere. Working sympathy for one's subjects can be misconstrued as ideological sympathy; ideological distancing from one's subjects, to the point of disapproval, is a difficult condition of work in an ethnographic style of research [...]; and ambivalence or silence in judgment on subjects makes the ethnographer's research equally vulnerable to a charge of elitism, or conversely to its use in a polemical condemnation of elites (ibid.: 23).

Consequently, more so than in other studies, the art of ethnography of the elites lies in being able to convey the humanity of individual experience without losing the sight of their implicit agendas.

References

Connerton, Paul. 1989. *How Societies Remember*. Cambridge: Cambridge University Press.

Gusterson, Hugh. 1997. Studying Up Revisited. *Political and Legal Anthropology Review*, 20: 114–19.

Hertz, Rosanna and Imber, Jonathan. 1993. Fieldwork in Elite Settings: Introduction. *Journal of Contemporary Ethnography*, 22: 1–3.

Herzfeld, Michael. 2000. Uncanny Success: Some Closing Remarks, in, *Elites: Choice Leadership and Succession*, edited by J. de Pina-Cabral and A. Pedroso de Lima. Oxford: Berg, 227–36.

Jackson, Anthony (ed.). 1987. *Anthropology at Home.* London: Tavistock.

Lotter, Stefanie. 2004. Studying-up Those Who Fell Down: Elite Transformation in Nepal. *Anthropology Matters Journal*, 6(2): 1–9.

Luhrman, Tanya. 1996. *The Good Parsi: The Fate of a Colonial Elite in a Postcolonial Society.* Cambridge, MA: Harvard University Press.

Marcus, George. 1983. *Elites: Ethnographic Issues.* Albuquerque: University of New Mexico Press.

Ohnuki-Tierney, Emiko (ed.) 1990. *Culture through Time: Anthropological Approaches.* Stanford, CA: Stanford University Press.

Okely, Judith and Callaway, Helen (eds). 1992. *Anthropology and Autobiography.* London and New York: Routledge.

Pina-Cabral, Joäo de and Pedroso de Lima, Antonia (eds). 2002. *Elites: Choice Leadership and Succession.* Oxford: Berg.

Shore, Cris. and Nugent, Stefen (eds). 2002. *Elite Cultures: Anthropological Perspectives, ASA Monographs.* Vol. 38. London: Routledge.

Simmel, Georg. 1971. *On Individuality and Social Forms.* Chicago, IL: University of Chicago Press.

Part IV: Wandering

Chapter 13: The Cosmopolitan Movement of the Global Guest

Nigel Rapport

In his edited collection on Israeli nationality *A Composite Portrait of Israel* (1980), Emanuel Marx urges against conceptualization that tends towards the bounded, closed and monolithic. Even a powerful centralistic state such as Israel is not a single entity but the sum of certain relationships, activities, processes and ideologies. Nor is 'the state' coterminous with 'the nation'; nor is a nation territorially fixed. 'Nodes' in 'fields' and 'environments' better convey the open multidimensionality of social arrangements whose bounds may vary per situation. 'Social phenomena', Marx writes in another place, 'are too complex to yield to summary treatment' (1976: 95). What one wishes to convey is social life as a series of partially overlapping open systems (cf. Strathern 1990).

This essay is a paean to '*Luftmenschlichkeit*', to being of the air. *Luftmenschen* was a favourite slur of the Nazis: the people of the air were the rootless, those who would not or could not take root. Renouncing the *Blut und Ehre* (blood and honour) of a genealogical attachment to the earth they remained 'guests among others' (Steiner 1997: 237). The paradigm cases of *Luftmenschen* were, of course, Jews and Gypsies. In this the Nazis could be said to follow Hegel [so George Steiner elaborates (1997: 307)], who saw in these people an awesome pathology. In leaving the Garden of Eden, the pastures of Abraham, the Land of Israel, the Jews had entered into exile from their natural, human place in the world, their embodying of an unconscious, organic and singular totality with a place. Here was an alienation from self and from humanity: from love, trust, family and community, the spontaneous, the active and the potent. In choosing to house themselves in their *Sefer Torah*, their Book of Law, Jews had perpetrated their suicidal renunciation from the family of nations.

As a paean to *Luftmenschlichkeit* this essay argues for an appreciation and an education of 'global guesthood'. I wish to do justice to a particular conception of human identity: as that which is born in and of movement, and manifested in multiplicity. I look to movement and multiplicity, too, as ideal ways to measure just procedures of state: to formulate ideas of justice and just relations between human beings. Justice entails ensuring the free movement that is fundamental to human being and becoming, to its potential for multiplicity. Finally, I look to the city as a site where an embodiment

of global guesthood might be secured and enshrined. Openness is advanced as a social value and an analytical value alike (Rapport 2001).

I. The Guest of Movement

For George Steiner, the shock waves of the Dreyfus Affair still reverberate. In 1894 Albert Dreyfus, an officer in the French army and an assimilated Jew, was accused of treason against the French State, and even when the falseness of the charges had been revealed, it was felt better by many in the French establishment (also anti-establishment) that he be sacrificed for the greater good of France. For Steiner, the Dreyfus Affair passed judgement on communitarianism. There is no nation, no ethnic community, polis or church that is 'not worth leaving', he contends (1997: 237), and 'no community of love, no family, no interest, caste, profession or social class not worth resigning from', the reason being that every nation will end up behaving unacceptably: will sacrifice a Dreyfus; while every church will excommunicate a Spinoza. Communitarianism – the ideology of discrete national, ethnic or religious communities, homogeneous, tied by blood, sharing a common birthright of culture, tradition or land – entails the sleep of reason; overriding loyalty to community entails an abstention from free thought and also from a disinterested pursuit of justice. In short, 'the locus of truth is always extra-territorial' (Steiner 1997: 322).

One may recall a similar formulation of Ernest Gellner's (1995: 66). A collectivity united in a belief is a culture, he begins:

> More particularly, a collectivity united in a false belief is a culture. Truths, especially demonstrable truths, are available to all and sundry, and do not define any continuity of faith. But errors, especially dramatic errors, are culturespecific. They do tend to be the badges of community and loyalty. Assent to an absurdity is an intellectual *rite de passage*, a gateway to the community defined by that commitment to that conviction.

It is Rousseau who provides Steiner with a name for the habitus he would set against communitarianism: 'humanism'. Humanism, says Rousseau, is 'a theft committed on *la patrie*' (cited in Steiner 1997: 324). Humanism replaces relativities and local contingencies with universalities and an aspiring to global truths. 'Anthropology', 'the writing of the human', deriving from Kant, was part of a vision of a 'cosmopolis' that might combine warring localisms, contesting theisms and opposed ethnicities in a polity that represented the earth entire, and that looked out from that globe to the cosmos beyond. The enlightened notions of Rousseau and Kant then fed into those great liberal experiments of modernity: the French and American Revolutions, British liberalism and utilitarianism, and socialism.

But Steiner wants to situate the humanist habitus, the loyalty to truth and to living authentically, in an earlier history than this. He calls it 'Jewish'. Obviously there is a certain irony in this appellation, given his diatribe against tribalist notions, but Steiner wants to draw attention to certain practices of being and doing that, he feels, have a long historical provenance and, as with the Nazis, Hegel, and others before them, have long been stereotyped as 'Jewish'. Let us invert the slur of *luftmenschlische Yiddishkeit*.

Homeland, for the Jew, was a text, Steiner begins. Even before the Diaspora and exile from the Land of Israel, in essential ways, Jewish identity was intrinsically connected with knowing and observing the Book of Law, reading and being answerable to it. The Book, as Heinrich Heine put it, was the land of one's fathers. This effected a certain continuity in Jewish life from Israel to Diaspora (and back), and throughout the pogromatic history of the latter. One remained at home in transit across different lands (one's sentimental attachment only to a messianic one) because one enjoyed a home-coming (cognitive, emotional, physical) each time one returned to the text; each re-reading was a return home. But the text was also a living text, what Steiner calls a 'real presence' (1997: 308). It contained a living truth such that each new reading and commentary could be expected to deliver up a new interpretation: making the text yield new existential applications, in unpredictable ways, in the contemporary context. The Book of Law was understood as comprising a literal and ongoing contract between God and Humanity: His Covenant. The details of this contract were the subject of constant probing and deliberation.

Over the centuries, these practices could develop in fetishistic ways, as Hegel and others noted: reading and transcribing the text assuming the role not of an accompaniment to life but of life itself.<strong>Notwithstanding, from this 'Jewish' tradition, this 'nationhood', Steiner would draw important lessons for a possible post-nationalistic, post-communitarian future:<strong> 'Could [the Jews'] true mission be that not of a nation-state, as abominable or brave, as corrupt or inventive as any other, but that of guests (be they unwelcome) among men?' (1998: 15).

In underwriting an identity not on the basis of territory or the fixity of any earthbound relationship, exclusion, knowledge or practice, but on the basis of an addiction to insight, and to a truth a nomad could carry within him or her – alone with God, with Truth – one embodies and idealizes homeliness, belonging, in a particularly humane way. This is what Steiner depicts as the habitus of a guest. The humane-ness of the Diasporic Jew derived from the fact that he or she was too weak to make others as wretched as themselves. In the Jew's resilience, however, is a pointer towards the dignity of guesthood as such. As guests – of one another, of land, of life – we lodge our identities, our bodies, our relationships, in time, in moments of being, not space. [It is time which is truth's 'native ground' (Steiner 1997: 327).] By conceiving of ourselves as mutual guests in sociocultural milieu, humanity might escape the mutual destruction that derived from singular, exclusivist identities fixed to territories.

In what follows, guesthood, the possibilities of sentimental attachment and institutional security in identities manifested in movement and flux – in abstractions, indeed,

such as truth – will be explored in ways intended to test Steiner's notions as recipes for possible practical action. Guesthood claims certain mutuality. But how is the attitude to be operationalized, the mutuality safeguarded? How might we regulate the reciprocities of our being alternatively hosts and guests to one another? Implicitly, Steiner contrasts the Jew and the Israeli but the connections between them are instructive. When the Jew was a Diasporic guest, he depended on the 'liberality', at least acquiescence, of certain hosts. If we are ultimately all guests, who will host us? How is an equitable use and distribution of resources to be assured? While we might aspire to overcome or revalue the limitations of space by conceiving of ourselves in time, any use of land nevertheless carries its own very particular fixities and absoluteness. Whether in a liberal Lockean economy or a socialist command one, land and benefit from its production entail notions of decisions and interests, of labour and profit, of ownership and inheritance, of exclusion and homelessness, with which guesthood must come to terms.

II. The Liberal State of Movement

To begin to tackle these issues I turn briefly to the history of liberalism as a political philosophy. The above account of guesthood and view of its potentials is imbued with an intrinsically liberal or cosmopolitan hope, and it may be beneficial to locate the notions of movement inherent in guesthood's mutuality – between places and between identities and roles – in a historical context.

As recently enunciated by Brian Barry (2001), the history of liberalism as a political philosophy may be traced back to sixteenth-century needs, after the European wars of religion (between Catholics and Protestants primarily), to reconcile cultural, specifically religious, differences within a single working polity. Before the Protestant Reformation the political assumption was that civil peace required one religion per state, and that states might then recognize each other's sovereign territories – and churches. Culture and religion were political affairs: loyalty to the state and to specific religious beliefs and cultural practices were inseparable. The innovation of liberalism was to advocate a depoliticization of culture and religion. As a means to civil peace there was to be a strict differentiation between public and private spheres: between *domestic* practice and belief, and a *civil* life of public exchange where there would be no determinate culturo-religious ideology.

Crucial to this history is the appreciation that it was *because* liberalism recognized how important religion might be in people's lives that it sought to depoliticize it: restrict it to the private sphere. Liberalism set out to provide a neutral ground on which Catholic and Protestant members of the polity – ultimately, people of all cultures and religions – could meet and be treated equally, and participate equally, and privatizing aspects of culturo-religious behaviour was the best way to accomplish this: to

ensure that none of the warring, mutually exclusive, religious doctrines, authorities or institutions was given a privileged position.

The philosophical innovation enabling this to happen was the treating of culturoreligious belief and practice as matters of *individual* conscience and preference: recognizing that faith depended on revelation, a kind of communication and knowledge which others had to take on trust. The political innovation of the liberal polity was to distinguish between loyalty to the state by individual 'citizens', and other loyalties which those individuals might hold dear. The state abrogated to itself a loyalty and duty – of good citizenship – which preceded all others; so long as they were good citizens, individuals could be anything else they chose in their 'private' life.

However accidental might have been this politico-philosophical innovation in early-modern Europe, however much a matter of *realpolitisches* statecraft and the need to cover over wounds of culturo-religious conflict for the maintenance of civil order, the history of European liberalism has involved a fleshing out and a shoring up of the concept of 'individual as citizen' to the point where the preferences for 'private belief and practice' now also include the choice of state. A liberal universalism has come to treat the individual as a global citizen, with a duty of loyalty to the ideal-typical notion of Good Government: to the rights and duties of being a Good Citizen and receiving equitable treatment among a Global Community of Good Citizens. So long as they are properly 'cosmopolitan' – citizens of the globe – liberalism leads individuals to expect the right to pursue their preferences to the extent of regarding themselves as distinct from all natal groups, all categorial ascriptions, including the nation-state.

That individuals in turn feel free to avail themselves of the possibility of trading one nation-state for another calls for a particular, non-sentimental attitude to land: their being able to imagine trading one particular plot for another. And here, too, there are significant echoes from the history of liberalism, as outlined now by Alan Macfarlane (1985). For Macfarlane, treating land as a commodity, as something whose ownership and use the individual regards as part and parcel of a life-trajectory of geographical and social mobility, has for long been a crucial component in the traits that characterize the freedoms of a liberal society.

England represents for Macfarlane the archetypal historical locus of democratic liberalism. A stress on the rights of the individual vis-à-vis the collective or state, and a notion that the constituent units of society were separate individual actors – autonomous and equal before the law – whose welfare was the measure of moral value, has a venerable history in England. As Macfarlane sums up the case (1985: 163): 'the majority of ordinary people in England from at least the thirteenth century were rampant individualists, highly mobile both geographically and socially, economically "rational", market-oriented and acquisitive, egocentred in kinship and social life.' So that when Thomas Jefferson wrote in the American Declaration of Independence, how, 'We hold these truths to be sacred and undeniable; that all men are created equal and independent, that from that equal creation they derive rights inherent and inalienable', he was putting into words a view of the individual and of society which had its roots

in the English Middle Ages if not before. When we espy a world where economic rationality is paramount and a market in goods, skills, tastes and ideas includes all, this socalled McDonalds-ization of the American Empire has its roots in the mobility and freedom of exchange of mediaeval England.

The details of Macfarlane's argument are complex and need not detain us here. The broad sweep of his conclusion, however, is that the freedom to dispose of land was present in England in the thirteenth century as in the twenty-first. Not only could land be sold, but there was a vibrant, crowded land market: selling, leasing, sub-letting, exchanging. Land was commodified, with no jural links existing between a family or household and the land, and no special sentimental links either. Although land was the main source of wealth, there was, throughout, a high degree of occupational specialization: cottage-industries, a developed bureaucracy, religious and state hierarchies, many adults being propertyless servants and labourers. In short, as far back as the thirteenth-century, England possessed an open and mobile society, with a division of labour and occupational differentiation characterizing village as well as town and city. Individualism was embedded in the law: a conception of individual rights and independence, and liberty of thought and religion. The market orientation, according to Macfarlane, led to unusual affluence, spread widely over the population, with considerable social mobility (status being based on wealth more than blood) and with few strong or permanent boundaries between occupational groups: between rich and poor, town and country. People moved up and down a social hierarchy as individuals so that the grandchildren of the same person could find themselves at opposite ends of the scale of wealth.

Rather than the economic aspects of Macfarlane's argument, however, what is more salient to my purposes here is the link which his history of democratic liberalism in England makes between mobility and attitude to land. Individual ownership and a market for transactions in land, a freedom to mortgage, sell, let, gift and will land, meant that parishes and villages, like towns and cities, represented geographical areas through which people moved (as individuals or members of nuclear families) rather than bounded, historical and affective communities to which people always belonged. Trade, cash, towns and markets, a thick screen of legal, political and social institutions, 'separated' people from the land in England; even small-holders entered the labour market – where wages were relatively high – and did not depend on agricultural subsistence. People were land-owners and landless, servants, labourers, traders, merchants, clerics and bureaucrats. Often they were these at the same time, or over time during one lifetime. Accompanying the history of English liberalism is a multiplicity and a fluidity of occupation, a diversity of relations to the land.

Moreover, while in the case of England it is a cash nexus – capitalism – which underwrites the above, providing the place of meeting and exchange between both occupational specialisms and kinds of skill and wealth, this need not necessarily be the case, it would seem to me. To repeat, whether in a Lockean culture of private ownership or a socialist one where ownership is centralized and public, a multiplicity

and a fluidity of occupation and a diversity of relations to the land would seem to be practicable, and key components in nurturing senses of identity not derived from particular territorial associations. (The Israeli kibbutz, then, would seem to offer a prime example of a collectivized, command economy where diversity and fluidity of occupations holds a key to an instrumental rather than sentimental attitude to land.) One maintains the value of the land, respects land as a value, but also recognizes how one's position vis-à-vis the land might vary at different times of one's life, or at different moments of social exchange. One recognizes land as a value amongst others and that different values interpenetrate and may be traded.

The model here is not a separation of spheres of valued skill and wealth – say, that of farmers versus merchants or academics – but a fluidity whereby wealth accrued in one sphere can be translated into others. Whether it be in terms of capital or of the intrinsic value of labour, the English liberal and the kibbutznik meet in an ideology that recognizes and celebrates a transition (at least translation) between social identities and the different work they undertake. Having mechanisms that might nurture, teach, promote and celebrate such transition – people and value moving between social and occupational spheres – would seem to me a significant component in fostering the potentially cosmopolitan attitude of global guesthood.

III. Metropoles of Multiplicity

The above history introduces a paradox. If, after Barry and Macfarlane, liberalism gives rise to the citizen as a potentially global actor, answerable to his or her own conscience even to the extent of selecting (and not having ascribed to him or her) membership of a state, able to trade in skills (including the marketing of land, its development and production) on a global scale, then what acts as guarantor of these rights – and of his or her obligations towards the rights of others? The liberaldemocratic nation-state has until now guaranteed the ideal and the practice of good citizenship: globalism now threatens to make that level of political organization redundant – in economic terms but also in terms of people's experience of space and their opportunities for sentimental attachments.

Certainly there are supra-national organizations to assume the role of global guarantor, most prominently the United Nations and its Universal Declaration of Human Rights, but also the regional European Union (and its Convention of Human Rights), the economic World Trade Organization, the juridical International Criminal Court, and the occupational International Parliament of Writers (and its global Network of Refuge Cities) (cf. Rapport 2005). One can also refer to the stand taken by the likes of NATO in opposing localized totalitarian, anti-democratic regimes. But it seems to me that middle-range institutions are also called for, between the individual global citizen and the global organization, and I would propose the city as potentially paramount in this role. The city has certain characteristics of scale that give it an advantageous

213

malleability and multiplicity vis-à-vis the traditional nation-state, and also a capacity to be known, commanded and 'owned' by individual actors: a 'terrain where a multiplicity of global processes assume concrete, localized forms' (Sassen 1996: 210). The longevity of the city as a kind of human social organization attests to this flexibility.

But first, am I right to talk of the redundancy (economic and sentimental) of the nation-state? According to some commentators, it is the Enlightenment project of universal notions of humanity, civility and morality which has fallen prey to renascent nationalisms, militant religions and resurgent ethnicities, not the reverse (Gray 1992: 13). What has a hold over the popular imagination is not the cosmopolitanism of Rousseau and Kant, Jefferson, Voltaire, John Stuart Mill and Matthew Arnold, but rampant neo-tribalism (in the idiom of religious, racial and cultural fundamentalisms) promoting rights to indigeneity, and primordial ties of blood to soil.

Notwithstanding the vehemence and virulence with which such movements are imbued I am persuaded that they are most properly understood as reactions to globalism and to a weakening of the traditional nation-state. In the spaces left by the latter, both global organizations and reactionary localisms come to be represented. As argued by Stuart Hall (1996: 311–13), then, ethnic nationalism and religious fundamentalism are attempts to reconstruct purified identities and restore cultural coherence, closure and tradition, in the face of globalism's openness, hybridity and diversity. This is both a reaction to the perceived forced nature of globalism and to its originally Western provenance, and reaction to a sense of being peripheral to the main centres of globalization and to success in relation to it. Hence the counter-identification of reaffirming cultural roots and a return to orthodoxy.

Gellner (1993) would concur. The reactionary despotisms of fundamentalist regimes (from Iran through North Korea to Sudan) can be seen in the context of an opening up of the world to science and to choice as implemented by Western, liberal and laissez-faire regimes. To expect a peaceful accommodation to Western ideas, however ultimately democratic and beneficial, is, he suggests, to fail to understand global real-politik. There are uncomfortable facts of our global condition today, important amongst which is that the world is not one of balance: Western culture (and rationality and science and technology and liberal democracy) is the predominant meaning-system, and other cultural regimes are either in the process of peacefully accommodating themselves to the West or else, through reactionary measures as diverse as religious fundamentalism and terrorism, suicide-bombing and female circumcision, ethnic militancy and romantic autochthony, making war against the West as best they can. Liberalism wins out, Gellner concludes, where and when it does, because Western capitalism as an industrial-military complex wins out and not because people come to see its idealistic and ideational characteristics and strengths; some will always prefer 'their own' local tyrannies to a freedom gifted by the 'neo-colonizing' West.

What this does draw our attention to, furthermore, is the nature of the security that the era of the nation-state did provide: an institutional security, including the procedural routinization of the rule of law; also a sentimental security, a place to

belong, where the heritage of the community guarantees 'a compelling formulation of self' (Cohen 1996: 802). It seems that global organizations are failing sufficiently to embody a global imaginary as possible repository of institutional or sentimental security, or both.

$$* * *$$

'City air inspires freedom' was a mediaeval European adage; '[the city is] the natural habitat of civilized man' was founder member of the Chicago school of sociology, Robert Park's, modernist rejoinder, surveying the expanding city of the twentieth century (1968: 3). Part of what made the city an ingenious social and moral milieu for Park, and bearing the future within it, was its bringing together of very different kinds of people. However transitory, casual, instable and fortuitous may be their meetings, the contacting and contracting of difference would be instrumental, he felt, in causing a general expanding of the norms of civility.

Commentators since Park's day have drawn attention to aspects of wishful thinking in his depiction of urban futures. The coming together of different people, different social and cultural communities in the city is as likely to give rise to what Robert Paine (1992) has termed 'cultural compression' as to 'civilizing' tendencies. That is, in dialectical relationship with the diversity of the city, its housing migrants from around the world, there emerges a piling up of boundaries, political, ritual, residential, economic, social and cultural, which people feel to be vital to their sense of personal and collective identity. Difference is thus experienced not as a mixing but as an insistence on purity and inheritance, distinction and exclusivity.

Notwithstanding, there does seem to me potential in the city for hope, if for no other reason than its 'incomprehensibility' (Young 1990: 240): 'A place of many places, the city folds over on itself in so many layers and relationships that it is incomprehensible. One cannot "take it in", one never feels as though there is nothing new and interesting to explore, no new and interesting people to meet.'

Verena Conley (2002) would have us know the city as 'chaosmopolis', for its para-doxical proclivity to stage inexhaustible and surprising novelty and difference. It is a matter of education, perhaps, but in the social and cultural multiplicity of the city I would contend there are lessons to be drawn concerning the capacities, the benefits and the rights of all people, every individual, to be themselves multiple. An 'education' would be to comprehend that the multiplicity of the city, its societies and cultures, tra-ditions, cosmologies and moralities, occupations and congregations, materials, moods and styles, is an analogue of the creativity and the potential to construct difference which lie in each individual human being. Thus would I take forward an Enlighten-ment vision of individual freedom, civilitude and cosmopolitanism; social and cultural multiplicity are understood as a consequence of individual creativity.

In seeing the multiplicity around them in the city as *their* multiplicity, as a manifes-tation of a version, a fragment, of the potential for difference that individuals contain

within themselves – as something that they could have thought up, as something that they could enact in life – an attitude of ownership, of generosity, of *irony* is encouraged among all 'citizens'. 'All this difference is a manifestation of me, of the human,' one recognizes. Some parts of 'me' I might like more than others; some parts might seem childish or foreign or undignified. But I should not despise myself or be intolerant. I should not deny what I have been or might become. The multiplicity around me, the otherness, manifests *my* right to construe myself differently in future. The multiplicity fractures any notion that I, humanity, can or should be defined in terms of singularities – except for my singular human capacity to create myself, to become, and to go on doing so. It could be said that I am a guest of myself, of my creative capacities. My body, my life, plays host to a multiplicity of ways in which I choose to define myself and live out an identity over time and place. The generosity I extend to others' choices of how to be is a generosity I extend to myself: a generosity which my body, my life, extends to my creativity to play host to the different 'mes' it construes at different moments. The truth to my identity – the true me – is not this or that manifestation or version but is rather my capacity, my potential, my right to keep on creating.

The multiplicity of the modern city would seem an apposite breeding ground, in short, for an attitude towards one's own identity as ironic, as eschewing the singular, the final and absolute. In seeing in otherness versions of me, and in seeing in me versions of others, the mutualities of playing hosts and guests to one another would also seem to inhere. But how might the above be encouraged, and how guaranteed?

IV. The Narrating of Multiplicity

In his article, 'Reflections on a new ethos for Europe' (1995), Paul Ricoeur argues that in the European Union there is an opportunity to institute a form of polity which does not replicate the structures of the nation-state. The issue is one of imagination: imagining a kind of post-national state which is ineluctably polyglot and eschews homogeneity. The key concept which he offers is 'narrative identity' (1996: 6). We are all entangled in stories, he explains, from those told to us by others, from childhood on, to those we tell about ourselves. There are stories containing stories and stories about stories. Individuals come to know themselves and be known by others, in significant respects, by the stories they know and in which they figure. Their stories become segments of other people's stories (parents', enemies', strangers') and vice versa; social groups may be represented, to significant extents, by the stories shared in their collective traditions. The telling and receiving of stories, forgetting and reviving of stories, mingling and denying of stories, produces important 'narrative identities'.

Narrative identities are of a particular sort, Ricoeur continues. They are not fixed or immutable; and this mobility of identity could be turned to our advantage. If it were recognized that individual and group, cultural, ethnic, religious and national identities were in important respects matters of recounted story – that seemingly fixed social

structures, systems of classification and vocabularies of absoluteness were matters of rhetoric only – then, Ricoeur suggests, we might learn more easily to accept, accommodate, stories that are not our own. The 'inalienable character of life experiences' (Ricoeur 1995: 8) may mean that we cannot directly share the lives of others, but we might imagine our way into their lives through an exchange of life-stories. We might take responsibility, in imagination and sympathy, for the stories of others, of strangers and enemies. (We already do this for fictional characters with whom we provisionally identify in our reading; and we do this for ourselves when we retell our stories and re-configure our pasts for present purposes and contexts.) It is a difficult, ethical labour, calling for generosity and irony, selflessness, as well as an imaginative re-placing of self in other experiences and lives, but it can be socially and morally rewarding, as well as practically vital.

Likewise, we could be generous with our own stories: allow them to be retold by others, not be rigid in our conception of them. We need not allow founding historical events underlying the stories to freeze the identities conjured up in the latter. We do not abandon important historical landmarks in our identities, we go on commemorating and celebrating them in story, but we allow their plural reading. Different narrative recountings of the same historical events – the French Revolution, the Second World War, the creation of the State of Israel – should not be seen as irreverent: a competitive diversity of tellings even honours the richness of the event. As recounted aspects of identity these phenomena are inherently mobile and they should not be allowed to become frozen or embalmed. Ricoeur is calling for a sophisticated as well as generous and ironic self-knowledge here. But the stakes are high. He would have people recognize the storied – recounted, situated, created– nature of their identities, and the pluralities and contrasts inherent in them: between self and other, between self's self and other's self, and self's other and other's other; between story and everyday circumstance; and between story and historical event. The story is not fixed, the story is not singular, the story is not reality (past or present), but the story is vitally significant: it is how reality feels and is approached.

Exchanging stories and partaking in a respectful reciprocity of perspectives, Ricoeur describes as 'narrative hospitality' (1995: 8). It entails imagining the joys and the sufferings of others, and showing compassion, even forgiveness, while not forgetting one's own versions of suffering and joy. By way of narrative hospitality one plays host to others' stories as well as one's own, one gives over one's stories as guests to another's re-telling. One plays guest in one's own life and host in another's.

V. The Anthropology of Movement and Multiplicity

Ricoeur encourages one to recognize that George Steiner's history of Jews being at home in texts and in time is a story. Equally, an existentialist accounting of inherent human capacities for multiplicity and a continual reinhabiting of identity is *my* story of self and world.

The paradox at the heart of Ricoeur's own narrative, however, is that in our capacity for story-telling, our practice of living individual and group lives by way of momentary and mobile narrations of identity, we might see an abiding human truth. The *content* of each of our stories may be an ongoing narrative creation, but the *form* of our storied lives, our continual re-telling, re-figuring of ourselves, our writing and rewriting of personal and collective identities, is a constant aspect of the human condition. *Story-telling manifests a universal capacity and practice.*

Moreover, recognizing the mutual place that stories occupy in our lives, and the multiple nature of those stories, can have large import as regards ethical practice. I recognize my own storied multiplicity: in the multiplicity around me I recognize potential versions of myself; in my stories I see reworkings of others' and vice versa. I remake myself in others' images as well as ones unique to me: I play host and guest to my 'own' identities – as does all humanity. The crux of Ricoeur's narrational hospitality is a mutual respect for one another's diverse notions of good and truth, insiders and apostates, history and economy, while recognizing that what we are fundamentally hosting and guesting in these various storied identities are versions of a universal human capacity continually to construct, own and promote stories (rather than anything more substantive). It is the nature of stories that is deserving of respect not their contents. It is the *form* of storied reality to which one is hospitable and whose reciprocal hosting and guesting suggests a route to a global sociality.

* * *

The anthropological mission could be conceived as promoting an ironic appreciation of cultures, religions, nations, ethnicities, classes and locales (Amit and Rapport 2002). If identity is recognized to be not a fixed, singular or essentialized aspect of reality but a storied one, a matter of particular narrations, then the end-point of anthropology – as well as its ground – is a contextualization of these particular and diverse rhetorical constructions in certain free practices, ontological and political. The capacity to make and inhabit stories is recognized as a human truth: the practice of making and inhabiting stories (multiply and momentarily), of moving through them, is enshrined as a human right. *Contra* the social and political programme of so-called 'identity politics', instilled with its 'final vocabularies' of difference (Rorty 1992: 88–90), it is the practising of storying that is safeguarded and ultimately respected, not particular contents

at any one time. Rhetorical absolutes – of category and community, of God and blood and land – do not translate into (rights to) actual fixities.

The free practice of storying calls for 'just procedures' (Rapport 1997: 180– 201) which ensure that the content of any particular story does not affect the capacity of anyone – those who own particular stories as well as those who are outsiders to them – to inhabit any number of other stories at the same time or go on creating stories anew. The citizen is faced with an array of (possibly conflicting) stories equally to be appreciated as others' and their own; being hospitable to, celebrating and enjoying the ongoing, individual and collective creation of identity, one transcends the claims of any one version to 'the truth'. One's attachment is a cosmopolitan one: to the course of one's life, a time not a space. Contractual arrangements – of ownership, of the distribution of resources – remain in place as vital procedures to regulate the reciprocality of hosting and guesting (Rapport 2005), but the citizen, the global guest, moves between them. It is in the space between, in the air of becoming, that one's global freedom lies.

References

Amit, Vered and Rapport, Nigel. 2002. *The Trouble with Community: Anthropological Reflections on Movement, Identity and Collectivity.* London: Pluto.

Barry, Brian. 2001. *Culture and Equality.* Cambridge: Polity.

Cohen, Anthony P. 1996. Personal Nationalism: A Scottish View of Some Rites, Rights and Wrongs. *American Ethnologist*, 23(4): 802–15.

Conley, Verena. 2002. Chaosmopolis. *Theory, Culture and Society*, 19(1–2): 127– 38.

Frug, Gerald. 1980. The City as Legal Concept. *Harvard Law Review*, 93: 1059– 154.

Gellner, Ernest. 1993. *Postmodernism, Reason and Religion.* London: Routledge.

——. 1995. Anything Goes: The Carnival of Cheap Relativism Which Threatens to Swamp the Coming fin de millenaire. *Times Literary Supplement*, 4811: 6–8.

Gray, John. 1992. Against the New Liberalism: Rawls, Dworkin and the Emptying of Political Life. *Times Literary Supplement*, July 3: 13–15.

Hall, Stuart. 1996. The Question of Cultural Identity, in *Modernity and its Futures*, edited by S. Hall et al. Cambridge: Polity Press, 273–327.

Macfarlane, Alan. 1985. *The Origins of English Individualism.* Oxford: Blackwell.

Marx, Emanuel. 1976. *The Social Context of Violent Behaviour.* London: Routledge and Kegan Paul, 15–28.

——. 1980. On the Anthropological Study of Nations, in *A Composite Portrait of Israel*, edited by E. Marx. London: Academic.

Paine, Robert. 1992. The Marabar Caves, 1920–2020', in *Contemporary Futures*, edited by S. Wallman. London: Routledge, 190–207.

Park, Robert. 1968. The City: Suggestions for the Investigation of Human Behaviour in the Urban Environment, in *The City*, edited by R. Park et al. Chicago, IL: University of Chicago Press, 1–46.

Rapport, Nigel. 1997. *Transcendent Individual: Towards a Literary and Liberal Anthropology*. London: Routledge.

——. 2001. Random Mind: Towards an Appreciation of Openness in Individual, Society and Anthropology. *The Australian Journal of Anthropology*, 12(2): 190–220.

——. 2005. Diaspora, Cosmopolis, Global Refuge: Three Voices of the Supranational City, in *Locating the Field*, edited by S. Coleman and P. Collins. Oxford: Berghahn, 179–97.

Ricoeur, Paul. 1995. Reflections on a New Ethos for Europe. *Philosophy and Social Criticism*, 21: 3–13.

Rorty, Richard. 1992. *Contingency, Irony and Solidarity*. Cambridge: Cambridge University Press.

Sassen, Saskia. 1996. Whose City is it? Globalization and the Formation of New Claims. *Public Culture*, 8(2): 205–23.

Steiner, George. 1997. *No Passion Spent: Essays 1978–1996*. London: Faber.

——. 1998. War, Terrorism and Chaos: God's Idea of Keeping a Promise, in *The Observer Review*, February 22: 15.

Strathern, Marilyn. 1990. *Partial Connections*. Savage, MD: Rowman-Littlefield.

Young, Iris. 1990. *Justice and the Politics of Difference*. Princeton, NJ: Princeton University Press.

Chapter 14: Social Science under Siege: The Middle East

Dale F. Eickelman

In a paper from 1913, Max Weber (1978: 81) argued passionately that value judgments in the social sciences 'presuppose an appreciation, quite simply, of the possibility that ultimate values might diverge, in principle and irreconcilably.' Every independent teacher, Weber (1978: 70) continued, has to repudiate the claim that academic arguments should be dispassionate and avoid issues that stir up 'heated' arguments. Academics should take up such issues, but as analysts and not as propagandists.

Written on the eve of the First World War, Weber's paper has a special poignancy, but his view has been repeatedly challenged by the political and religious left and right outside the university and from within the university itself. Reflecting on long-term changes in the American university, Edward Shils (1997: 166–7) noted the decrease in institutional authority in general in Western societies. In earlier eras, he argued, the greatest infringements came from university administrators and the often 'self-imposed' external custodians of universities. Since the 1960s, however, the greatest threat to the academic ethic of independence from partisan advocacy has come from within the university, as faculty themselves advocate various political causes and make the criteria of 'intellectual achievement and promise of intellectual achievement' and 'the candidates' determination and capacity to pursue and transmit truths about the matters taught, investigated, and studied in universities' only one of many criteria (Shils 1997: 174–5).

Middle Eastern Studies in Perspective

Scholarly research concerning the Middle East is not immune to challenges from states and society, and from within the academy itself. This chapter explores the conditions under which social science can transcend immediate political pressures or at least recognize the price that is paid for succumbing to them. The study of the region requires both intellectual passion and academic discipline to sustain a social science autonomous from both political advocacy and the temptations of research for hire. All social science research is subject to such pressures, but research dealing with the Middle East sustains especially strong passions.

This pressure to deviate from independent and nonpartisan analysis is not a new phenomenon. Over two decades ago the chair of a leading anthropology department in the US said to a mutual colleague (personal communication, 11 March 1989) that he preferred not to hire anthropologists focused on the Arab world because the Middle East was a 'disturbed' region and thus unlikely to generate quality research.

The statement was made in a private conversation and may have had an element of irony lost in transmission, but for over a decade the department in question did not hire anthropologists focused on the Arab world. Almost inevitably, many of the department's faculty had conducted field research in virtually all other regions of the world, including South and Southeast Asia, Africa (including South Africa under *apartheit*), Latin America, Europe, and Oceania. In many of these regions, violent domestic or international conflict was underway or had recently occurred. More recently, two leading political scientists endured sustained concerted attacks, more vehement than those that occur on other issues concerning foreign affairs, for publishing a paper assessing the influence of pro-Israel lobbying groups on US foreign policy (Mearsheimer and Walt 2006).

In other world regions, including the former Soviet bloc, questions proscribed or banned in time of the Soviet Union can now be approached. Whether the subject concerns determining if a given people are Slavic or 'eastern' in origin (Shnirelman 1996) or the origins of the conflict in Chechnya (Tishkov 2004), considerable pressure remains not to attack too significantly the 'official story' of what happened in the past and sometimes to avoid social science research altogether (Kohl and Fawcett 1995; Verdery 1991). These constraints notwithstanding, scholars actually living in the former Soviet Union and other Soviet bloc countries nonetheless often pushed the limits of self-censorship. Leszek Dzięgiel, whose writings included an account of field research in Kurdish villages facing what had been blandly called 'modernization' at the hands of Iraqi authorities in the late 1970s (Dzięgiel 1981), wrote a post-Communist memoir (Dzięgiel 1998) poignantly indicating the prior limits of expression. Yemeni officials of the former People's Democratic Republic of Yemen (PDRY) reportedly obliged Vitaly Naumkin, a foremost Russian anthropologist who worked and taught there from 1972 until 1977, to leave the country because they considered him overly informed about their internal political and tribal struggles (see Naumkin 1984; 2004).

In a similar manner, the Bernstein Israel Research project, initiated in 1963 and directed by Emanuel Marx, offers an exemplar of research carried out under challenging conditions (Marx 1980: 1, 9).

Scholars writing in different national contexts find themselves under intense pressure to conform to prevailing political currents. In spite of calls for boycotts of the first World Congress of Middle East Studies (WOCMES), held in Mainz from 9–13 September 2002, the event was successful because it brought together Israelis and Arabs, among many others. The subsequent boycott by some Arab scholars of WOCMES 2 in Amman in June 2006 served only to isolate the boycotters from a significant scholarly event. In April 2005 a British academic union passed a resolution advocating the

boycott of collaboration with Israeli scholars unless they publicly condemned Israeli government policies toward Palestine (Jaschik 2006), an action that promoted heated discussion on the subject of academic boycotts in general (American Association of University Professors 2006). In the US, pressure comes more from groups seeking to correct the alleged bias against Israel on university campuses, and students are encouraged to report instances of bias by faculty members (www.campus-watch.org). Some individual critics, including Martin Kramer (2001) have additionally argued for external government committees to monitor the activities of university-based Middle Eastern Centres receiving government support.

Such pressures are not confined to universities. In October 2001, US Secretary of State Condoleezza Rice successfully pressured US television networks not to broadcast the videotapes of Osama bin Laden, claiming that they might contain 'secret messages' that would enable further attacks. Others, including myself (Eickelman 2001), argued for the open release of such tapes in order to 'know the enemy'.

The Meaning of Siege

The notion of 'siege' has overlapping connotations. A classic one is the action of investing a locale to cut off communications with the outside world to compel surrender. It can also mean a period of illness, of struggle, of difficulty, and refers metaphorically to a defensive mentality – a state of mind based on the assumption of hostility from others.

Just as ideas of the scope and uses of the social sciences are in flux, so are ideas of the relation of the social sciences to authority. On the one hand there is Shils' perspective: when faculty in the late 1960s saw that there was no effective state sanction against student protests, some faculty saw academic freedom as a license to advocate even 'the destruction of existing society and its cultural traditions,' making the cause of revolution a 'moral obligation' (Shils 1997: 166).

On the other hand is the perspective advocating authority invested in diffuse networks of respected peers. Such is the case, for example, for promotion and tenure committees at most academic institutions. Elaborate rules exist for selecting qualified peers to review candidates for promotion to higher academic ranks. The reviewers are all academic peers, but deans also argue for and against individual cases, and most institutions have a central advisory committee that reviews the assessments of departmental or program committees. The board of trustees usually has the final word in ratifying appointments, but it only intervenes in academic decisions in extraordinary circumstances. Although the US Congress has at times been sceptical of the adequacy of peer review, extensive hearings in the mid-1980s concluded that the alternatives were less likely to be as effective (US Congress 1986).

The line between peer review and outside inducements and constraints is constantly shifting. Thus social science research sponsored by UNESCO on nontraditional forms

of higher education or by the International Labour Organization on pastoral nomadism or labour organization is commissioned research, but usually involves peer review both for commissioning the work and for reviewing the pre-publication results. The US Department of Defense's 'Project Camelot', investigating the likelihood for violent social upheaval in Latin America in the 1960s, was also commissioned research, but it also involved confidentiality and nondisclosure agreements regarding sponsorship and was widely regarded as unethical once deceptive means were used to continue the project (Horowitz 1974).

Since the early nineteenth century, social science has had two competing elements. One element can be traced back to Auguste Comte (1798–1857): social science as social engineering. In this perspective, a 'scientific' understanding of society allows one to manage or manipulate social forms, either to improve the human condition or to produce desired outcomes. An example of this social manipulation is the idea of punishment as a way of 'reforming' or 'correcting' criminals in order either to dispose of them or to reincorporate them into society (Foucault 1977). Economics as a social science is perhaps the most publicly accepted of social sciences in this sense, and thus the actions and statements of the chairman for the US Federal Reserve Board have major public consequences. Social scientists also contribute to development projects, based on the idea that villages and entire countries can be 'managed' to achieve better conditions of economy and life.

The other meaning of the social sciences is thinking systematically about social forms and practices in ways that often are profoundly subversive in the sense that they often criticize or depict the unintended consequences of dominant or official authoritative social forms and practices. Kanan Makiya's *Republic of Fear* (1998), a critique of the exercise of authority in Saddam Husayn's Iraq, is a trenchant example of this style of thought. It is narrower in scope than C. Wright Mills' *The Power Elite* (1956), but the effect is the same – to expose for analysis the hidden sinews of power and privilege.

The Shifting Role of Intellectuals

Shils (1972: 16), a profoundly conservative sociologist, defined an intellectual in the 1960s as someone with a need to penetrate the screen of immediate concrete experience in order to discern more basic and enduring features of social life. The same definition can be adapted to apply to meaningful work in social sciences and social thought, which encompasses the historian's craft.

The French social historian Marc Bloch (1964: 148) wrote that new social forms were not consciously created by successive generations but were created 'in the process of trying to adapt the old'. He had in mind the ties of dependence in feudal Europe when he wrote these lines, yet his observation applies equally to understanding contemporary politics and societies, and the contemporary role of religious belief and practice. In this

sense sociologists, like intellectuals, have a power that is difficult for political authorities to control or regulate.

Abstract
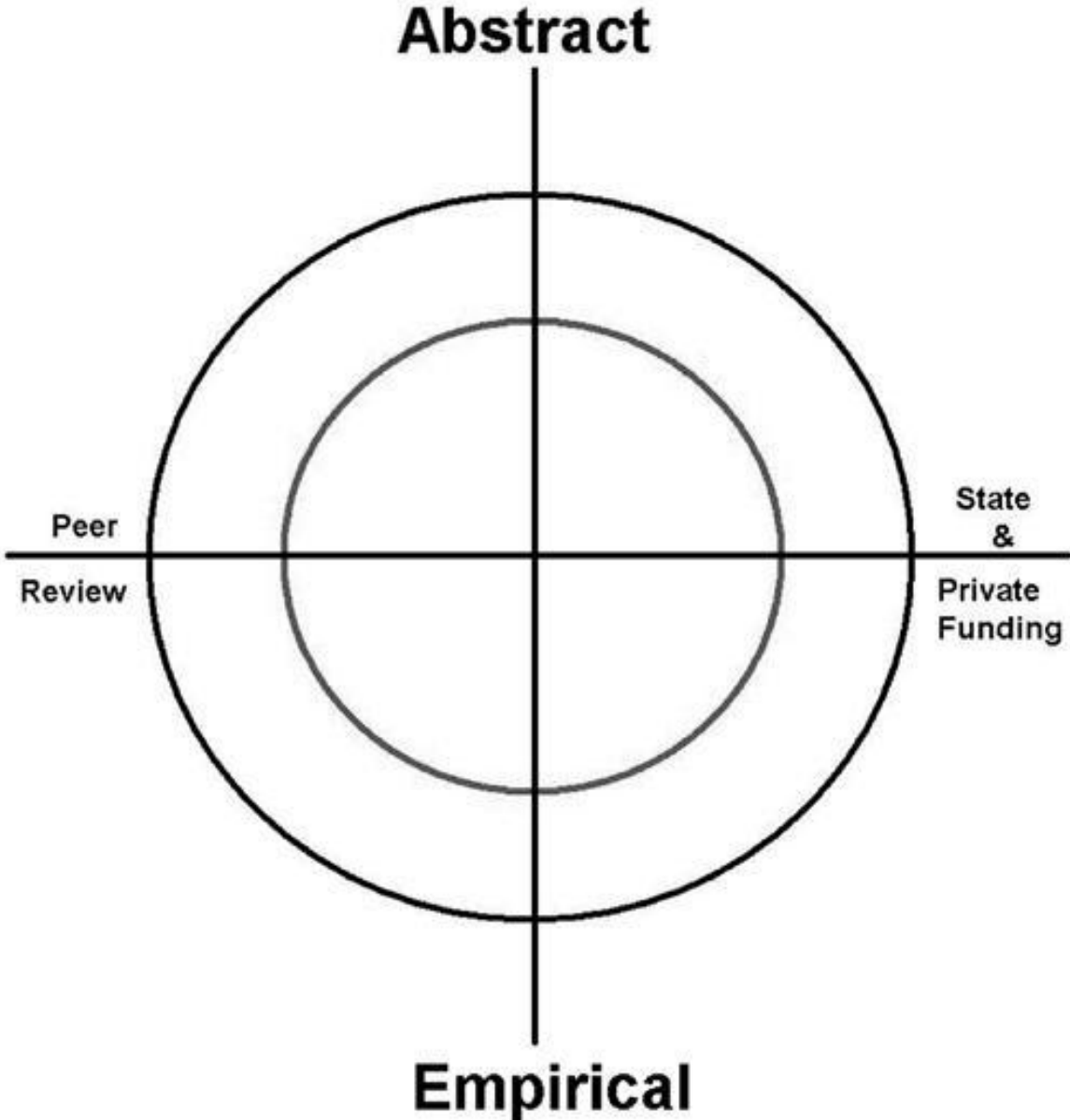
Figure 14.1 Social science as social thought: opportunities and constraints

The fate of many intellectuals and scholars in the United States during the Mc-Carthy era of the early 1950s is a case in point. Writers and scholars were banned from work unless they signed an oath of loyalty to the United States and disclosed to authorities the names of colleagues or fellow students who at any time had belonged to

groups suspected of being subversive. The pressure was so intense that even universities such as Harvard and Yale, while publicly denouncing the 'witch hunts' of scholars and intellectuals, confidentially submitted to the Federal Bureau of Investigation for vetting the names of applicants for jobs as lowly as those of untenured instructors (Diamond 1992). The well-publicized case in recent years of Saad Eddin Ibrahim, a faculty member at the American University in Cairo and head of the Ibn Khaldun Centre, indicates the price that some scholars continue to pay for engaging in activities that call authority into question. At the very least, such cases remind us that scholarship and ideas have value and continue to make a difference – at least in speaking truth to authority.

The ideal field in which social science (and social history) research takes place, can be represented as a double axis (see Figure 14.1).

The vertical axis is defined at one end by a focus on abstract and general conceptual concerns, and at the other end by a focus on immediate and concrete events. Most good social science involves elements of both the abstract (and generalizable) and the concrete and immediate.

The horizontal axis concerns authority – the allocation of resources for social science research. One end of this axis is represented by review and allocation in which one's peers, however they are defined, control the criteria and allocation of resources. The other end is represented by accountability to state authorities or the public. There is a dynamic tension inherent in both poles in the sense that the 'proper' balance along the two poles measure cannot be defined for all time, but must be renegotiated and rethought. Thus a study of women's magazines in the Ottoman empire in the late nineteenth century might not today be seen as subversive, even if it showed an Ottoman concern with allowing women a greater presence in public life (for example, Frierson 2004). But half a century ago, such a study showing the capacity of the Ottoman state for reform and innovation would have challenged Kemalist ideas of the Ottoman past and would have been difficult to pursue within Turkey.

The early work of Pierre Bourdieu (1930–2002) on Algeria during that country's struggle for independence (1954–62) illustrates the balance between social science and charged politics in a time of crisis. His *Sociologie de l'Algérie* was first published in France in the highly popular 'Que sais-je?' series in 1958 and then in a revised edition in 1961, a year before Algeria's independence. An English translation of the revised edition, *The Algerians*, long out of print, was published in 1962. Bourdieu was teaching in Algeria at the time that the book first appeared, so he was an eyewitness to what sociologist Raymond Aron calls the 'horrors' of the late colonial period.

The Algerians claims to go beyond the immediacies of the conflict. In the introduction, Bourdieu invokes Algeria's geographical and linguistic diversity, cautioning that it is easy to exaggerate the opposition between Arabs and Berbers. He also affirms that no society is completely closed and cut off from others. Yet, he argues, groups nonetheless seek to judge themselves in relation to one another (Bourdieu 1962: xiii). He states that his goal is to analyse these differences so as to discover 'above and

beyond them the basic identity that these differences conceal or seek to control'. In a few brief sentences he acknowledges the upheavals caused by colonization and the inequality of relations between groups. The old dialogue between Arabs and Berbers was unequal, he writes, but less so than the greater absoluteness of the French presence. Nonetheless, he says, 'one of the keys to the present drama may be found in the painful debate of a society which is compelled to define itself by reference to another, [but which at the same time] is torn by selfdoubt and the fierce defence of its besieged self' (Bourdieu 1962: xiv).

After these words, however, *The Algerians* reads like a combination of standard French human geography in its descriptions of Algeria's different regions and *modes de vie*. The first chapter concerns Kabyle social structure, and Bourdieu's focus is on the concentric circles of allegiances of the interlocking communities that make up Kabyle society, with the family as the fundamental unit of society. He describes the brutal gender inequality of the Kabyle family, the structure of the Kabyle house, 'typical' village plans, and finally the idea of 'genealogical democracy' (Bourdieu 1962: 16). By this last term he means that the metaphor of belonging is structured by shared genealogies: in one order and kind it concerns domestic organization; in another order and kind it concerns political organization. In every case the 'fiction' of the eponymous ancestor is considered as the archetype of every social tie. He then goes into a distinction between 'our own kind of democracy' – presumably French and involving a conception of the public good, a defined executive power, and a kind of parliamentarianism – and the Kabyle one, in which a council, or *tajmaât*, agrees on decisions by consensus. Excluded from the account is a description of the political economy of the peasant under colonial rule and the implications of sustained colonial rule on ideas of Berber, Arab, and national identity in the context of Algeria's struggle for independence.

Bourdieu's final chapter, 'the revolution within the revolution,' takes as its premise that Algeria's war for independence highlighted a number of transformations already underway within Algerian society. Bourdieu sometimes engages in awkward generalization, as when he writes (1962: 156) that in a society where self-knowledge is obtained exclusively by reference to itself alone, 'the cultural models, although conventional and therefore arbitrary, are yet considered normal and natural. With the discovery of a foreign cultural system, however, their hidden essence is suddenly revealed.' The discovery of the existence of another tradition leads to a new understanding of one's own.

Even given the sociological ahistoricism of much ethnographic reporting of the 1960s, this assumption of Kabyle self-containment is astonishing, given the contributions of Kabyle religious scholars and poets to Muslim and Arab society, as well as sustained economic interaction with the rest of the world. This shortcoming notwithstanding, Bourdieu succeeds in raising his monograph above the immediate political conflicts.

In substance and approach, Bourdieu writes almost in the fashion of a standard encyclopedia entry, a major accomplishment for the charged circumstances of the time. He was close enough to his ethnographic subject to convey a sense of ethnographic

authority, as in the description of the Kabyle house. Parenthetically, it is only in the last five years that I learned that his field survey was conducted in fortified hamlets (*camps de regroupement*) at the height of the Algerian conflict, meaning that the village structure that he describes is at best an idealized image of the past. This context is not made clear, for the overall image projected in *The Algerians* is that of a seemingly timeless village and domestic social organization.

Some of Bourdieu's generalizations on the nature of Algerian society have not aged well, but in structure the book maintains a balance between the specific and the abstract. As a state employee and university teacher, he nonetheless had the space needed to confront the French public with his argument – at least those attracted to reading sociology, even if the criticism of French action in Algeria is muted.

In a similar fashion, Ernest Gellner's *Saints of the Atlas* (1969) distances itself from the tumultuous years preceding Morocco's struggle for independence in 1956 and the ensuing years, although much of Gellner's field research was conducted in the late 1950s and early 1960s, although elsewhere Gellner sought to place the tribal uprisings of the time in a wider sociological context. As with Bourdieu's early sociology, Gellner's polemic is consistently strong, but within the context of academic discussion and debate.

Another example of the balance between partisanship and scholarship is Abdellah Hammoudi's *Master and Disciple* (1998). In Isiah Berlin's distinction between the hedgehog and the fox, the hedgehog knows one thing well, holds onto it, and explores it in depth. The fox, on the other hand, knows a little about many things and links them with skill. In *Master and Disciple*, Hammoudi combines the two approaches. At one level, his book concerns the cultural foundations of Moroccan authoritarianism, specifically the sources of 'vitality and continuity' of monarchical authority since 1956 in the 'post-colonial Moroccan nation-state' (Hammoudi 1998: 11). At another level, he argues that Moroccan authoritarianism – 'a mixture of coercion and a transfer of a powerful sentimental motif of community to newly unfolded spheres of the political (constructed as a human association with limited space for argument)' (Hammoudi 1998: xviii) – sheds light on certain types of authoritarianism elsewhere, and on Arab societies in particular. He writes that the 'underlying mechanism' of all Arab states, whether monarchical or single-party systems, is 'a central figure's claim to single-handed decision-making regarding the aspirations and future of the society governed' (Hammoudi 1998: 150).

One can debate the merits of his central image of master and disciple, but it is good to think with and stays away from the direct criticism of Moroccan or other Arab styles of rule. Other social science studies that achieve the balance between strong argument and partisanship include Susan Slyomovics, *The Object of Memory: Arab and Jew Narrate the Palestinian Village* (1998), achieving a delicate balance between generational changes in narratives of Jewish and Arab dispossession, and Laetitia Bucaille, *Growing Up Palestinian* (2004), which narrates through the eyes of youth of different political factions the daily struggle of the *shabab* to sustain a sense of

work in a precarious society. In a similar way, Fariba Adelkhah's *Being Modern in Iran* (2000) describes the ebullience of Iranian society, sociologically portraying underlying shifts in patterns of responsibility, gender relations, person and society, and religious practice and expression.

Scale and Subject in Middle Eastern Anthropology: The Past and the Present

There is also a change in conception of the nature of the 'object' studied in the social sciences in the Middle East that tends to draw such work more into public policy debates. At the time when I became an anthropologist in the 1960s, the majority of available studies concerned pastoral societies and the rest concerned villages. Although German geographers studied cities and markets in this earlier era, such as Gaube and Wirth on Aleppo (1984), I did not become aware of their work until the late 1990s, and anthropologists were noted mostly by their absence. In this respect, Kenneth Brown's social history, *People of Salé* (1976), stood out as one of the first anthropologically informed studies, together of course with the work of Marx and other Israeli scholars. Indeed, the distance between Marx's first monograph, *Bedouin of the Negev* (1967), which fits the mould of classical anthropological studies of the era, and the different approach epitomized in *A Composite Portrait of Israel* (1980) suggests the incremental disciplinary shift that was underway. Now there is a multitude of studies both *in* cities, concerning the economics of crafts, micro credits, labour unions, and schooling, and *of* them, involving the cultural nature of urban forms themselves. The older ethnographic sense of delimited areas, which could be caricatured as beginning with a village and in a brief concluding chapter reaching out to the world, and commenting on 'change,' has given way to more satisfactory and diverse ways of conceiving themes and issues.

A personal recollection gives a sense of this shift. In 1992 I published an article in the *American Ethnologist* on the implications of the rise of mass higher education for Arab societies. In accepting the article, the journal's editor at the time, Don Brenneis, wrote me that this was the first article the journal had accepted that was ethnographic yet not based on any specific place. Much of what we study today also involves sustained relations between communities in the Middle East and what used to be called diasporas, although the greater speed and ease of travel and communications changes relations between homelands and the communities of migrants or like-minded people overseas into potentially intimate and immediate ties.

All contemporary work on the region must deal with a range of core issues that shape virtually all understandings of the region. In spite of success in some countries to reduce the rate of population growth, it remains high throughout the region. It is highest in Gaza, but the population growth rates of many Arabian Peninsula countries are not far behind. In general, the region is also one of declining economic opportunity.

Official rates of unemployment for youth hover around 30 per cent, but the actual rate is much higher. Thirty per cent was the unemployment rate during the worst of the US depression in the 1930s. The region's population is now overwhelmingly urban, but in settings where state authorities, even those of oil-rich countries, are less and less able to provide adequate infrastructure.

On a slightly brighter note, educational opportunities are now more widespread than they were in the middle of the twentieth century for both women and men. Since the 1950s, country after country in the region has entered the era of mass higher education. Its rise, coupled in recent years with the growth of regionwide satellite television, has facilitated a wide audience able to follow debates that in earlier generations were confined to a minority. Paradoxically, however, this rise of mass higher education has also seen a decline in general standards and an educational system increasingly disconnected from employment and the possibilities of raising standards of living. The 2003 Arab Human Development Report (AHDR 2003) courageously spells out the deficiencies of most Arab governments in promoting the knowledge needs of Arab societies.

Publicly shared ideas of community, identity, and leadership take new shape in such engagements, even as many communities and authorities claim an unchanged continuity with the past. Mass education, so important in the development of nationalism in an earlier era, and a proliferation of media and means of communication have multiplied the possibilities for creating communities and networks among them, dissolving prior barriers of space and distance and opening new grounds for interaction and mutual recognition. In both politics and religion, many assert continuing strong links with the past, but the practice of religion and politics now conveys significantly different ideas of person, authority, and responsibility.

Some of the major consequences of this rise of mass education, greater ease of travel, and greater ease of communication include a greater recognition that governments are increasingly unable or unconcerned to deliver goods and services, although they demand loyalty and compel obedience. Another consequence is the increasingly free flow of information across frontiers. As Sadik al-Azm wrote (2000), in spite of the total broadcast and print ban on mention of negotiations between Israel and Syria regarding a peace treaty in the late 1990s, the *salons* of Damascus had a remarkably accurate contemporary view on what was taking place, a view later confirmed by the foreign press.

As a result of what Jon Anderson and I (Eickelman and Anderson 2003) have called new people and new forms of communication, social distance and hierarchy become eroded. Now it is not just the views of the political and religious elite that count, but also the views of everyone else. Although Anderson and I focused on the implications of the new media, Magnus Marsden's study of the Chitral region of north-west Pakistan (2005) reminds us that it is not just the educated middle class who contribute to these debates, but ordinary villagers exposed not only to doctrinal militants but also to a shared local Sufi tradition of disciplined knowledge that recognizes not only doctrine and reason but also the passions and emotions of experience.

Globalized Scholarship and Peer Review

Thirty years ago one could still discern distinct national traditions in the social sciences. Today more than in the past social scientists work in a global environment in which there is a much greater awareness of complementary national traditions of scholarship. German, Moroccan, Japanese, French, American, Russian, Bulgarian, and British studies of Sufi tradition are no longer as separate or distinct as they were in earlier eras. For the Middle East, contemporary French political science combines anthropological techniques with more conventional styles of analysis.

In part this merging of forms builds on deliberate institutional action. In 1983 the US Social Science Research Council established a Joint Committee for the Comparative Study of Muslim Societies. For the duration of its existence – it was disbanded in 1991 – it innovated the sponsorship of dissertation workshops open to doctoral candidates drawn from throughout the world. They met with leading scholars from various disciplines, and in principle could experience complementary approaches to common themes at an earlier point in their career than was possible at an earlier time. Although the SSRC committee has ceased to exist, subsequent efforts by the Institute for the Study of Islam in the Modern World (Leiden, 1998–2008), the German von Humboldt Foundation, and the European University Institute in Florence provide similar opportunities today.

The blurring of disciplinary and national traditions in scholarship is a fact, but a combination of changing patterns of state and transnational funding (as with certain foundations and projects sponsored by the European Union) and efforts to shape the support of scholarship to narrow partisan or national interests remains strong. I have already mentioned efforts in the US to tie federal support for scholarship to a narrowly construed national interest, a threat that has counterparts in other countries of Europe, Asia, and the Middle East.

Peer review offers no facile answers to situating scholarship in terms of the national interest or the public good, but such issues must be kept in mind in responding to the key issues that any peer reviewer must ask: What will be learned as a result of this research? Why is it worth knowing? How do we know that its results are correct? And is it likely to influence work in other disciplines?

The above questions build on a brilliantly incisive pamphlet (Przeworski and Salomon 1988, revised 1995) once hard-to-obtain in print form but now readily available online. Of all that has been written on peer review, this pamphlet remains the most succinct pragmatic statement for the social sciences. But these questions, seemingly self-contained, are encompassed in a subtly shifting new environment. Founded in 1923 with foundation support to professionalize the social sciences, in recent years the SSRC website (www.ssrc.org) presents its mission as 'bringing *necessary knowledge* to public issues'. The SSRC italics may appear defensive, but shortly after September 2001 the SSRC, based in New York, opened its first office in Washington DC. In a world of

shifting funding opportunities, it is reasonable to suggest a concerted effort to remain in the public policy eye.

Social Science and Public Responsibility

The role of the social scientist in society is coterminous with that of the intellectual, and from the other end of the political spectrum in a remarkably parallel view, Edward Said (1994: 120) writes:

> However much intellectuals pretend that their representations are of higher things or ultimate values, morality begins with their activity in this secular world of ours – where it takes place, whose interests it serves, how it jibes with a consistent and universalist ethic, how it discriminates between power and justice, what it reveals of one's choices and priorities.

Thus an inevitable, tough chore for the social sciences anywhere is to conduct searching debate about the stage and setting for the shifting context in which social science can reach its full potential, sustaining a balance between social thought and public responsibility.

Much like the classical French *leçon* as described by Bourdieu (1967), this chapter has argued by contrasting opposites. Thus the approach of Emanuel Marx and the Bernstein Project contrasts implicitly with that of Martin Kramer. Marx's colleagues made better sociology by distancing themselves from policy objectives and the territorial state as their object of study, an approach that would have tied them to the then current Israeli nationalism. Kramer, on the other hand, would undo such autonomy in American Middle Eastern studies by tying academic funding to the support of immediate policy objectives. Likewise Shils, a conservative advocate of academic meritocracy, is in many respects the opposite of Said, an advocate of deconstructive criticism. Yet implicitly they both agree that one role of social scientists is to speak truth to authority, a responsibility that demands that social scientists engage in public issues as well as in basic research, explaining to shifting constituencies what it is that they do that makes even seemingly arcane knowledge necessary and producing outcomes that are not shaped or tailored to anticipate what sponsors or policy makers want to hear. Maintaining the balance among these competing factors is a responsibility shared by all social scientists, but it is one especially incumbent on those engaged in the study of the Middle East.

Acknowledgements

Anthropology, Tokyo Metropolitan University, 13 October 2002; at the Department of Anthropology and Sociology, Ben-Gurion University of the Negev, Beersheba, 5 December 2002; as the 20th annual Kaller lecture at the Moshe Dayan Centre for Middle Eastern and African Studies, Tel Aviv University, 15 December 2003; and at the Centre for Intercultural Studies, St Kliment Ohridski University, Sofia, 8 March 2005. I thank these audiences for valuable comments, as well as Jon W.

Anderson and Simon O'Meara, who commented on the penultimate version.

References

Adelkhah, Fariba. 2000. *Being Modern in Iran*, trans. from the French by Jonathan Derrick. New York: Columbia University Press.

ADHR. 2003. *Arab Human Development Report: Building a Knowledge Society.* (www.undp.org/sa/reports).

al-Azm, Sadik J. 2000. The View from Damascus, in *New York Review of Books*, 47(10): 70–77.

American Association of University Professors. 2006. Academic Boycotts: Conference Papers. *Academe*, 92(5): 35–83.

Bloch, Marc. 1964. *Feudal Society*, trans. L.A. Manion. Chicago, IL: University of Chicago Press.

Bourdieu, Pierre. 1962. *The Algerians*, trans. Alan C.M. Ross. Boston, MA: Beacon Press.

Brown, Kenneth. 1976. *People of Salé: Tradition and Change in a Moroccan City, 1830–1930.* Cambridge, MA: Harvard University Press.

Bucaille, Laetitia. 2004. *Growing Up Palestinian: Israeli Occupation and the Intifada Generation*, trans. Anthony Roberts. Princeton, NJ: Princeton University Press.

Diamond, Sigmund. 1992. *Compromised Campus: The Collaboration of Campuses with the Intelligence Community, 1945–1955.* New York: Oxford University Press.

Dzięgiel, Leszek. 1981. *Rural Community of Contemporary Iraqi Kurdistan Facing Modernization.* Kraków: Agricultural Academy.

——. 1998. *Paradise in a Concrete Cage: Daily Life in Communist Poland, an Ethnologist's View.* Kraków: Wydawn Arcana.

Eickelman, Dale F. 1992. Mass Higher Education and the Religious Imagination in Contemporary Arab Societies. *American Ethnologist,* 19(4): 643–55.

——. 2001. The West Should Speak to the Arab in the Street, in *Daily Telegraph* (London) 27 October. (http://news.telegraph.co.uk/news/main.jhtml?xml=%2Fnews%2F2001%2F10%2F27%2Fweick27.xml).

Eickelman, Dale F. and Anderson, Jon W. 2003. Redefining Muslim Publics, in *New Media in the Muslim World: The Emerging Public Sphere*, 2nd edition, edited by D.F. Eickelman and J.W. Anderson. Bloomington: Indiana University Press, 1–18.

Foucault, Michel. 1977. *Discipline and Punish: The Birth of the Prison*, translated into English by Alan Sheridan. New York: Pantheon.

Frierson, Elizabeth. 2004. Gender, Consumption, and Patriotism: The emergence of an Ottoman Public Sphere, in *Public Islam and the Common Good*, edited by A. Salvatore and D.F. Eickelman. Leiden: Brill, 99–125.

Gaube, Heinz and Wirth, Eugen. 1984. *Aleppo: Historische und geographische Beiträge zur baulichen Gestaltung, zur sozialen Organisation und zur wirtschaftlichen Dynamik einer vorderasiatischen Fernhandelsmetropole.*

Weisbaden: L. Reichert.

Gellner, Ernest. 1962. Patterns of Rural Rebellion in Morocco: Tribes and Minorities. *European Journal of Sociology,* 3(2): 297–311.

——. 1969. *Saints of the Atlas.* Chicago, IL: University of Chicago Press.

Hammoudi, Abdellah. 1998. *Master and Disciple: The Cultural Foundations of Moroccan Authoritarianism.* Chicago, IL: University of Chicago Press.

Horowitz, Irving Louis (ed.) 1974. *The Rise and Fall of Project Camelot: Studies in the Relationship between Social Science and Practical Politics*, revised edition. Cambridge: MIT Press.

Jaschik, Scott. 2006. British Scholars Seek to Cut Ties to Israeli Scholars. *Inside Higher Ed.* 30 May (http://www.insidehighered.com/news/2006/05/30/ boycott).

Kohl, Philip L. and Fawcett, Clare (eds). 1995. *Nationalism, Politics, and the Practice of Archaeology.* New York: Cambridge University Press.

Kramer, Martin. 2001. *Ivory Towers on Sand: The Failure of Middle Eastern Studies in America.* Washington DC: Washington Institute for Near East Policy.

Makiya, Kanan. 1998. *Republic of Fear: The Politics of Modern Iraq*, updated edition. Berkeley: University of California Press.

Marsden, Magnus. 2005. *Living Islam: Muslim Religious Experience in Pakistan's North-West Frontier.* Cambridge: Cambridge University Press.

Marx, Emanuel. 1967. *Bedouin of the Negev.* Manchester: Manchester University Press.

——. (ed.) 1980. *A Composite Portrait of Israel.* London and New York: Academic Press.

Mearsheimer, John, and Walt, Stephen. 2006. The Israel Lobby. *London Review of Books,* 28(6), http://www.lrb.co.uk/v28/n06/print/mear01.html,

Mills, C. Wright. 1956. *The Power Elite.* New York: Oxford University Press.

Naumkin, Vitaly V. 1984. *al-Jabha al-qawmiyya fi al-kifah min ajl istiqlal alyaman al-janubiyyah, wa-l-dimuqratiyya al-wataniyya* (The National Front in the Struggle for the Independence of South Yemen and National Democracy). Moscow: Dar Taqaddum.

——. 2004. *Red Wolves of Yemen: The Struggle for Independence.* New York: The Oleander Press.

Przeworski, Adam and Salomon, Frank. 1995. *The Art of Writing Proposals*, revised. New York: Social Science Research Council. (www.ssrc.org/ fellowships/ art_of_writing_proposals.page).

Said, Edward W. 1994. *Representations of the Intellectual.* New York: Vintage Books.

Shils, Edward. 1972. *The Intellectuals and the Powers.* Chicago, IL: University of Chicago Press.

——. 1997. *The Calling of Education.* Chicago, IL: University of Chicago Press.

Shnirelman, Victor A. 1996. *Who gets the Past? Competition for Ancestors among Non-Russian Intellectuals in Russia.* Washington, DC: Woodrow Wilson Center Press.

Tishkov, Valery. 2004. *Chechnya: Life in a War-torn Society.* Berkeley: University of California Press.

US Congress. 1986. *Science and the Regulatory Environment.* House of Representatives, Committee on Science and Technology, Task Force on Science Policy, 99th Congress, 2nd Session, 26 February and 5–6 March, Washington DC: US Government Printing Office.

Verdery, Katherine. 1991. *National Ideology under Socialism: Identity and Cultural Politics in Ceauşescu's Romania.* Berkeley: University of California Press.

Weber, Max. 1978. *Weber: Selections in Translation*, edited by Walter G. Runciman. Cambridge: Cambridge University Press.

Chapter 15: The Suspicious Anthropologist: Documenting my Mother's Holocaust[1]

Esther Hertzog

Introduction

This chapter was written with love and gratitude for two people, Eva Ben-Tal, my mother and feminine role-model, and Emanuel Marx, my friend and professional role model.

While telling me her Holocaust experiences, interwoven in her present-life narrative, daring to say things that 'should not be said', I was overtaken by admiration and compassion for my mother. Her guts and fascinating insights struck me with the understanding that fundamental human ethics lies in free, unbounded elaboration on human situations and conduct. In this context Emanuel became a spiritual and intellectual guide for me, in encouraging my nonconforming insights. Had he not encouraged and urged me to write down my mother's testimony and to publish my interpretation of it I would have never done it. Moreover, Emanuel's unique and subtle perceptiveness, which makes him so skilful in pinpointing hidden meanings of human expressions, helped me to realize that suspecting my mother's story and interpretations is fine as long as I take this instinctive, 'natural', scepticism further and accept what she says as true, valid, sincere descriptions, when trying to make sense of it, as I put on my Anthropologist hat. As puzzling, contradicting and even absurd as they may seem, they all are part of an intriguing conduct of a human being, in this case a woman who survived the Holocaust.

In my chapter I deal with the flickering sensations, insights, comparisons, jumpily changing scenes and periods, in which I've become an active-passive participant, through listening, wondering, hesitating, doubting and sometimes laughing over my mother's wandering and sometimes contradicting associations. My mother's narrative is embedded in travelling among conflicting and ambiguous perceptions. She meanders between extreme bad and virtuous good, between dates and periods in

---

[1] A different version of this chapter was published in 2008 (by Gefen).

the past and various events in the more and less immediate present, between faraway places on the globe and Israel, between collectives and individuals, between seemingly unconnected situations. Listening to my mother, talking with her and documenting these conversations, made me a companion to her emotional, verbal and intellectual journey between places, periods and ethical positions. This journey of ours is at the core of the chapter.

Another issue with which this chapter deals is my puzzled reaction, or rather my latent suspicion, regarding my mother's descriptions and views concerning people and events that were part of her past. I examine the annoying doubts that crept into my thoughts all along my interviews with my mother, namely her unexpected, inconsistent and inconceivable accounts concerning her experiences during the Holocaust. Elaborating on my recorded reactions to my mother's descriptions and connotations suggests that I, sometimes openly, question the sincerity of her expressed sensations and views that she offers while telling me about her past experiences.

Attempting to comprehend the nature and implications of past recollections, the far past in particular, for people, raises basic issues of validity and reliability. There is a vast literature that discusses memory in the context of social catastrophes, which is quite probably the most relevant to Holocaust survivors' memories. Some of the more recent research suggests that memory has little to do with historical facts (an inextricable concept by itself, as it emerges, for instance from Jacques Le Goff's preface to his book *History and Memory*, 1992: ix–xxiii, 'history-asproblem'. Febvre's notion, cited by Le Goff, xvii, expresses this point clearly). Memory researchers Elizabeth Loftus and Katherine Ketcham (1994) have pushed ahead by far arguments about memory's fragility, inventiveness, and its tendency to easily decay over time. They claim that 'memory fades with time, losing detail and accuracy; as time goes by, the weakened memories are increasingly vulnerable to "post-event information" – facts, ideas, inferences, and opinions that become available to a witness after an event is completely over'. According to Loftus and Ketcham the boundary between truth and lie, reality and fantasy, sanity and madness, is

> Permeable and unguarded, and we cross it all the time in our dreams, desires, and imaginations. Memory is the vehicle by which we transport ourselves from reality to fantasy and back again, as many times as it takes to spin coherent and colorful stories from the dry straw of real life ... Memory, our most loyal and faithful servant, complies with our wishes.

Holocaust researchers (e.g. Moshe Zuckermann, 1993; Marianne Hirsch, 1997; James Young, 2000) support these claims clearly, arguing that memory is inaccessible and representations of the past bring about the erasure of the past from our memory, rather than its commemoration. It is argued, therefore, that the representations of the Holocaust cannot be perceived as a concrete reality, but rather as part of subjective, flexible and pragmatic contemplation.

Giorgio Agamben (1999), argues that although we can enumerate and describe the atrocities of the Holocaust yet 'they remain singularly opaque when we truly seek to understand them ... The aporia of Auschwitz is, indeed, the very aporia of historical knowledge: a non-coincidence between facts and truth, between verification and comprehension' (12).

Historians and Holocaust researchers Martin L. Davies and Claus-Christian W. Szejnmann (2007) claim that 'Memory is not History. Moreover, if the Holocaust has tested the limits of representation, it has also made memory itself insecure' (xxxiv). They further contend that

> The history of reactions to the Holocaust reveals a basic reality: that history, though it keeps the past present, manages a world that is constantly changing. It is a basic truth: the human world is by definition temporal, hence transient. The 'truly stated' facts history preserves are – to use classical, Aristotelian terminology – accidents in time as a process of degeneration (ibid.).

Introducing the contributors' main arguments in the introduction to their book the editors of *How the Holocaust Looks Now* suggest that, 'far from being a safe repository of truth, history itself is subject to fierce political contestation and instrumentalization (as Benz, Cohen, Davies, Hertzog and Wolf show)' (xxxii). Similar claims were raised by, for instance, Moshe Zuckermann (1993), Idith Zertal (2002), Ronit Lentin (2004), and the author (Hertzog 2007): all are stressing the pragmatic, instrumental use of Holocaust recollections for personal, collective, public, political and other purposes.

Sociologists and anthropologists emphasize the cultural and social background of the individual that effects, to a large extent, what and how events are remembered. Thus, for instance, sociologist Jeffrey Prager (1998) suggests that memory is 'intersubjectively constituted. While memory is produced by an individual, what is remembered is always influenced by the cultural world in which the individual is embedded' (89). Maurice Halbwachs, cited and followed by Paul Connerton (1989), argued that it is 'through their membership of a social group – particularly kinship, religious and class affiliations – that individuals are able to acquire, to localize and to recall their memories' (27). Perceiving memory as 'relational and social rather than as the product of an isolated individual' (30), serves Diane L. Wolf (2007) in her study on memories of hidden children in the Holocaust.

To sum up, the efforts to unveil the 'truth' or 'real facts' behind survivals' testimonies and narratives are, apparently, impossible. Remembering is discussed, in this chapter, as an instrumental form of human activity and as connected to social-cultural contexts. Apart from making sense of past experiences, recollections serve as a means of adjusting one's hardships to given conditions, comforting and relieving suffering, etc. Remembering is perceived in this context as a way of affording life after death to those whose physical existence was annihilated, as a way of memorializing the experiences

of the survivors, as perceived by them, and as adjusted by them to fit their needs at present. Documenting survivals' experiences, raised from memory and processed within a context distant from the historical events, reaffirms, vitalizes and actualizes them. This is what I do in my chapter based on my mother's memories, as she recounted them to me. The importance of this analysis is in actualizing things that took place in her far past, even if it is not clear what 'really' happened. 'In reality', argues Le Goff 'the interest of the past is that it illuminates the present. The past is reached by starting out from the present' (ibid.: xx).

Following this line of argumentation I treat my mother's recollection as part of her present, and as such the historical 'true' facts have little significance. What is interesting for me is how she constructs her past and uses it for her needs at present. As for my own part in this journey it is the challenge of understanding how my thinking changes from disbelieving and doubting my informant's (although a trusted person) story and statements to accepting them as relevant, real, social truth. François Châtelet's discussion of the 'historical Spirit' (cited by Le Goff: 10) can serve to stress this argument. He claims: 'it is not in any way acceptable to treat what has happened as fictive, as unreal, and that the non-presence of the past (and of the future) cannot in any manner be identified with its nonreality'. From this point of view there is truth beyond the eyes of the beholder and the anthropologist's duty is to go along with it and, more importantly, to follow its inner, subtle, connections with the whole plot and changing situations. Thus, in line with Connerton's argument, to remember is 'to become capable of forming meaningful narrative sequences. In the name of a particular narrative commitment, an attempt is being made to integrate isolated or alien phenomena into a single unified process' (ibid.: 26).

The Oppressed Presence of Memory

Repeated reading of things my mother said in conversations with her makes me wonder about details she notes. Sometimes they sound superficial, as if flicking through the events with a distant touch; sometimes they contain contradictions and lack clarity. Time and again, I found myself wondering, while talking to her and when I read her words, whether she is evading me. My mother often ended her words with comments such as 'That's it' or 'It's hard', and I wondered if she wanted to avoid the exposure of the pain and the hurt.

I asked my mother whether she thinks about things after our time together and she replied: 'I think regardless ... This accompanies you throughout life ... all the time. Sometimes you don't fall asleep at night, no way out. That's it.'

Mention of her grandmother, with whom she stayed at home for three more weeks, after her parents and her sister were taken from the village, arises in the traumatic context of the two-day journey to Auschwitz in a cargo train. She recalls:

> They put them into cargo trains, in sort of carriages, one on top of the other, thousands in one train. and old grandma ... something ghastly. It's not good to talk about it. I'm surprised at myself for talking at all. It's hard to describe, a woman of 84, like this, without being able to get off, to drink or something ... They did their body functions in a vessel placed in the corner of the carriage ... It's impossible to think about it.

Repeated reference to her grandmother taken from her in Auschwitz made me wonder whether this recollection involves special suffering, or whether mentioning her serves as a stimulus or confirms the memory, a promise that she will not be forgotten. In other words, retaining this memory may work as a 'trigger' that assures fulfilling the obligation of those who did survive to remember those who did not.

I kept asking about her nights, and she replied: 'It's night and that's it. So what do I think? It goes through one's head all the time.' Thus she goes over, repeats and secures the meaning of the suffering and the difficulty involved in remembering throughout her life, but does not want or is incapable of going into details. Being unsatisfied with her stubbornness over that which is obvious in the constant presence of the painful memory I challenge her: 'But you never spoke about it.' 'No. It's enough that I think. Even now. Have I got anything to say? I've already said it a million times, so it's already totally superfluous to tell. You want to know, you're writing about it, but for me it's already ... that's it.'

But nonetheless, I don't give up and continue: 'But that's not it. According to what you say it's with you constantly ...'

> Well, of course it is. If you had some sort of trauma, wouldn't you think about it all the time? Not all the time, but mainly when you're like this, when you rest, when you wake up at night and can't fall asleep again, so you sometimes have to really exorcise all these thoughts and because of that who wants to talk about it at all?

Here I put on my psychobabble hat, hoping to help the client by making him/ her conscious of past experiences in order to achieve release from burdensome experiences. 'Don't you feel the need to talk about it?' I ask. 'No, no', she replies, and I persist, 'Is it better to be alone with such thoughts at night?' And on another occasion I again ask: 'But perhaps talking about it with someone makes it easier? You involve someone, you're not alone with your thoughts?!' And she replies determinedly: 'No, no, who could be interested in it? Everyone has his own package to bear.'

My mother may have wanted to say that the experience was so terrible that she has no words to describe it. If this is the case, her words express the great difficulty in remembering the experience in all its intensity. My mother may have also forgotten and finds it hard to admit that the details have escaped her and only a dull memory of pain remains. If it is hard for her to talk about the painful details she may prefer, perhaps,

to talk about them superficially, vaguely, to avoid the pain. Is this 'post-traumatic' behavior (as she herself suggests) that channels the attempt to interpret and explain towards the domain of mental health? Or is this 'simple', unavoidable forgetfulness over the years, that prevents remembering details of things? For the details are that which affords meaning to the event, make it real, present. If this is forgetfulness, then why does my mother insist on talking about 'bitter memories' that make her nights miserable? Could it be that the feeling of obligation towards the dead, and even the feeling of guilt over remaining alive explain her insistence on her version of suffering at night? From this point of view, mention of the grandmother from whom she was separated in Auschwitz is the most tangible way to enable my mother to hold onto, and to fulfil, her 'obligation' to remember and to be connected to the dead who did not survive.

Later, she openly admits her difficulty in remembering events for the prosaic reason of the distance of the years. She says, 'It's almost sixty years. Exactly fiftynine years ago. My entire mother's family was expelled to the Ukraine in 1941, so it's more than sixty years. One lives with it. It was grandmother and aunts with children I was just a young girl.' She explains that she can remember things through associative recollection, but 'much of it is also imagination ... and also what people have already set themselves.'

The question of memory continues to trouble me and I ask her: 'And do you remember them? Do you remember what they looked like?' And she replies, 'No, even my father, for I have no photo of him, so it's hard now, even to think. Sometimes I imagine him like your father, you know? It's already ...' 'Confusing?' 'Yes.' 'Do you have a picture of your mother?' 'Yes, of mother I have. The picture with the aunt from Romania. They brought me the photo with grandmother, mother and myself on her arm. The photo of my mother alone has vanished.'

On the one hand my mother admits her memory is very fuzzy and on the other hand she insists on her claim of the emotional difficulty involved in remembering. She explains to me that it was particularly difficult for her in the past to deal with memories and says, 'Father [my father, who passed away at the age of 48–E.H.], poor thing, would talk, but I shut him up. I couldn't stand it. I really suffered. Now I can tell something here and there but it's not good for me.' 'Is it hard for you to talk about it?' I ask. 'It doesn't do me any good, both to talk and to think.' In order to further reinforce the meaning of the suffering involved in remembering, she employs proof of the damage this causes anyone who makes a point of exploring these memories: 'People are really going crazy now, at an older age. Really. This is the reason I don't want to discuss it at all.' And when I continue to push her and wonder about her reluctance to remember, I almost preach to her and say, 'But there are those ... who like to talk.' She continues my words and criticizes those who do talk, 'Those who talked and talked obsessively ...' Then she celebrates the proof of her claim, regarding the difficulty and its dangers, 'They really went crazy. Those who are constantly involved in this really go crazy. You know? It's really an obsession for them. All the time dealing with it and it's not good. It's forbidden. One should be busy with other things, with one's children,

with grandchildren.' Thus, enforcing the present on her life seems to be a means my mother employs in order to distance the threat of remembering the past.

My mother avoided the threatening recollection in the past, and would also like to avoid it in the present, but she was also sure in the past and is absolutely convinced now that discussion of what occurred should be withheld from others, especially children. She criticizes those parents who told their children what happened to them in the Holocaust and claims:

> There were parents who destroyed their children's lives. For this reason I told Father, 'They're still young. Why do you wreck their lives? They aren't guilty. How can they help?' Now it's something else. You already see they're mature adults ... He really had it tough. But we all went through it. Why should we tell the children? We suffered enough. I wanted to forget and didn't want to talk about it.

When I pester her further, saying, 'But you see that you haven't forgotten, it's with you all the time', she says 'It's impossible to forget. Mother was 43. Cremating my mother at such an age in Auschwitz, and father, aged 44. Grandmother was 84, but she was healthy. And to think that they sent one as if to bathe, and opened the gas on one.'

When I asked my mother if she had any particularly strong recurring memory, she replied, 'No, not any memory concerning myself, I was young, I was 19. Only in connection to my parents and family.' It seems that, even today when she is clearly interested in documenting and publishing her story, my mother restricts herself in discussing her experiences of suffering during that period. At the same time she is allowing (and perhaps even forcing) herself to elaborate on the horror related to memories of her relatives.

My mother's descriptions of her efforts to erase, to silence and to 'bury' the bitter memories, expose the oppressed presence of memory in her life from the Holocaust till now. Nevertheless, it appears that my mother holds on to the horrendous memories of her past experiences, as it is most important for her to transmit them, at this later stage of her life, to the world.

The Subjectivity and Relativity of Recalled Humiliation

Assuming that the victims' degrading experiences as women were devastating when they took place and are intolerable for the survivors, when thinking about them at present, aroused my scepticism toward my mother's stories. Being forced to walk naked in front of the Nazis and having their hair shaved, could clearly have a tremendous impact on the women's feminine identity. Yet, my repetitive questions, concerning

the harsh humiliation I assumed my mother experienced, invoked mostly an adamant denial. Her replies were unanticipated by me and made me suspect that her descriptions of how she felt in the past could not truly reflect what she felt at that time. She did not express hatred or rage, which I expected. I suspected that while recounting the past events she was not really aware of the immense degradation of the experiences she went through and was telling about.

When I persisted on raising 'difficult' questions about humiliating experiences she replied with a vehement negative. 'Did you erase it?' I ask, doubting her statement, and she replies: 'No, no, they did not humiliate me particularly.' 'Did they shave your head?' I insist and she raises her voice slightly and says, 'That was humiliating? That was something done to millions. To me too. If you stayed alive, that is the least awful thing they could do.' And I don't let up: 'You stood there naked, naked in front of the ...' 'That was already nothing.' I wonder: 'Really?' And she continues: 'Nothing. Right.' 'How could that be? They stripped you naked and you walked around there like some sort of ...'

> Yes, like a what? Like everyone. That's it. You get used to it. So I walked around. So what? It passed. Look, it's one thing when they strip you, a man takes a woman and does it. But when you're in a group, so is it exactly the one standing there who matters? He's looking exactly at you? He doesn't see anyone. He sees women as animals ... no, it shouldn't be taken to heart. I didn't give a damn about him. As far as I'm concerned he humiliated himself.

'Yes? Have you thought about it then?' I wondered whether this was her view now of the past or whether it was how she perceived the situation at that time. Her reply was: 'Yes, Always. This is how I thought then.' Obviously, it is impossible to get a valid reply to such question, for my mother certainly spoke from her current thinking. However, she answered confidently that her memory was reliable.

My mother's words and resolute reactions to my doubting questions, regarding remembering the humiliating situations, indicate her determination to now perceive her past experiences as bearable rather than exceptional, unconceivable events. She stubbornly interprets things in a manner that diminishes the significance of the humiliation when thinking about events. She does this by denying essentiality and humanness from the situations and the perpetrators, who perceived the naked women before them as non-human. Thus, while the Germans perceived the naked women in front of them as animals lacking human consequence, their conduct in that situation turned them into creatures lacking human consequence, beasts, and so their deeds had no impact on the human value of the women, and their nakedness is like nakedness before non-humans: animals or objects. In other words, humiliation is experienced and perceived subjectively and should be understood in relation to specific contexts and situations.

Similarly, my mother presents the memory of the situation in terms of relativity: Baldness and nakedness lose their de-humanizing, humiliating impact and their relevance to the female identity relative to 'what happened', in other words to death. Furthermore, the public humiliation, its collectivized context, is a 'trouble shared'. The individual has no independent existence and is absorbed in the masses. All are affected and thus the hurt is not individual, and accordingly less severe. The 'togetherness' of the humiliation reduces and mitigates its weight and impact. A psycho-social dynamic is manifested here, in retrospect, of processing the experience of nakedness, converting it into something that was 'not awful' when there were many others who shared the event, as something that happened to 'everyone'. The situation in which these things occurred affords them their reduced meaning in my mother's perceptions at present. Nevertheless, it is possible that this cognitive mechanism served the women in the past as a means of adjusting to the devastating situations.

However, the ambivalence in remembering filters through from time to time. At one point my mother almost admits the feeling of humiliation regarding the baldness and nakedness. But then, too, the humiliation is not personal but rather collective: 'They took everything from us, our hair, everything. You know, there, wandering around like that, girls did not know anything about undressing before someone.' And she immediately says something contradictory, and returns to grasp onto the observation that reduces and softens the bitter meaning of the events:

> They shaved, and sometimes had no razor, so they used scissors. With me, for example, they cut my hair with scissors and a little bit more was left. But what we looked like ... Anyway, who had a mirror to see ourselves. But there were girls who found themselves some rags [to cover their bald heads].

In other words, the situation diminishes the import of losing feminine appearance (as there is no mirror in which to see the distortion) and, moreover, the women quickly found a means to hide it, thereby also 'pulling a fast one' on the Germans. However, in my mother's recollections of nakedness before the Germans something, more compatible with familiar descriptions of traumatic reactions, was revealed in connection to the social-specific context, mainly of old and religious women. I asked: 'What happened to someone who did not want to get undressed, and tried to object?' My mother answered, raising her voice,

> There was no such thing as not wanting to get undressed. My old grandmother, poor thing, an 84-year-old woman, so religious all her life. She got undressed there, what shame, it was something-ghastly, you cannot imagine, even though we already saw in what trouble we were, thus how could anyone who stayed alive care.

244

It appears that there is a contradiction in the attitude to memory: on the one hand relative to herself – it wasn't so bad, she 'got used' to it and allegedly didn't give a damn, but for her grandmother and religious women, it was 'A total disgrace, horrific.'

When she talks about herself, for example, telling me about Mengele separating the old women from the young, forced to parade naked in front of him, the shame vanishes. She says:

> When we arrived in Auschwitz, stripped of everything, they of course took us to be examined and to shower and he stood in the middle, and we were naked, I was no longer embarrassed in the least, you can laugh ... we went past as if there was no-one, he also looked at us as if we were sheep and not women.

Several researchers (such as Rochelle Saidel, 2004) describe this experience of women going naked before the Nazis as one of the worst experiences women remember from Auschwitz. According to this approach, religious women suffered particularly since they came from a social world that views nakedness as an absolute taboo. In other words, the perception of nakedness is influenced by the women's cultural background and thus, the enforced walk in the nude would not carry the same meaning for all women, nor would it be remembered similarly by them. The interpretive direction, arising from analysis of my mother's words, offers a perspective that stresses adaptive coping that drafts the ability to rationalize and come to terms with remembering the 'inhuman' experiences that are extremely anomalous in comparison to 'regular' life prior to the Holocaust. Furthermore, the women interviewed by Saidel may have perceived marching naked before Nazi eyes as a suitable symbol and expression, *post factum*, of a situation they wished to present in their cultural-social context, as an experience of total humiliation that they remember.

Recollecting the past in mitigating, and even positive, terms stands out in my mother's comments on the cessation of menstruation. Today, she perceives this result of the Nazi damage to the female physiological functioning as a positive thing, and even as 'lucky', in a reality lacking any suitable sanitary conditions. The issue of cessation of menstruation arises in our conversations following my question, 'How did one manage while menstruating without underwear, if this was not provided after they came out of the showers and remained with the shoes and received a striped nightdress?' Her sharp response was: 'One didn't menstruate. They gave us bread with bromide to eat that burned our stomachs. It caused me such heartburn that I couldn't bear. Or they gave us potatoes with their peels to eat. That was the food. It was bitter ...' I cut into her and ask: 'So did it immediately stop all women menstruating?' and she replied: 'Yes, of course. They thought of that. It's such a satanic thing. And yet, that was luck.' And I wonder, 'Why was that lucky?' And she replies 'Well, what would we have done there? There was nothing there. A piece of rag for one's head. If I already had one, which someone gave me, someone would have stolen it from under your head.' The deed was, in fact, satanic, but remembering the results is connected to 'luck'.

Instrumental Recalling – Humanizing the Demonic Evil

When I studied the transcripts of my conversations with my mother I found myself surprised not only by her words that mitigate the significance of the humiliation by the Nazis in the camps, particularly in Auschwitz. I was also surprised by the way in which my mother humanized the devil – that which was perceived by me selfevidently as such. Time and again, I discovered her forgiving, understanding and even compassionate attitude towards the Germans. Consistently and stubbornly she insisted that there is no 'they' which comprises all the Germans. In her descriptions there are different people and changing situations. There is no hatred in my mother. I ask her about this: 'Look, you don't talk out of hatred.' And she answered: 'There is no hatred in me.' And when I ponder that 'There is no hatred in you?' she explains, 'No, no. Because I know, they also suffered, the Germans, OK? They followed the Führer like idiots. Didn't they try to kill him several times? Amongst the officers there were some who tried. It was impossible. They were always caught and killed.'

I insist on understanding the absence of hate: 'In any case, it's hard to understand that there is no hate. You know, after all they did to you, such horrific things...' And she insists, 'Right, but they were given orders. That's what they had to do.' And I object her position: 'When they took people to the crematoria they could have refused to take them, being ready to die too.' She again refrains from condemning all Germans collectively. She says to me, 'Look, the Germans were not all animals. Hitler only made them ... out of the youngest in particular ... they got excited. But there were those who had no choice.'

I argue with her and wonder about her certainty and self-confidence regarding the probability that the Germans were not all the same, and that not all were evil, but were rather trapped without much choice. However, she is ready to admit that amongst the youth were many more that were swept away by Hitler. I ask several times whence she draws her assessment and conviction that 'They were forced.' She says resolutely, 'Absolutely 100 per cent. It was known. They were forced.' and when I ask if this was stated clearly, she answers, 'No, why on earth? But there were some, those from the SS who said it. They had no choice. They were forced to go – to guard. There were some who were a bit older, who viewed us as their children. You could really see ...' And I ask again if these things were said. In response she tells me about one of their guards who showed interest in her. He told her how he was sent there. She adds: 'They were very humane and didn't lift a finger on us. Straight, like that.' Then she tells me about the guards' kindness: 'They took us one day, on Sunday, to cut ice and load it on the truck and afterwards we took the ice down to the cellar. After that they prepared food, gave us some in the factory kitchen. Did we eat! We were sick. We ate cabbage.'

And she brings another story of good Germans. She talks of German workers who threw apples from a passing truck full of apples to women who were in another truck

taking them from the brick factory back to the camp in Praust (near Danzig). Those people, she believed, provide an example for the existence of good Germans who helped them.

> I try to remember those things ... so it's not awful. Otherwise it's impossible to live with it ... There were decent people. What do you think, all the Germans were evil? Some of them suffered. They had no choice. Of course some were bad, really ... I don't envy anyone who does bad to another. I don't believe that he can ever be at peace with himself.

My mother is partially aware, so it seems, of her self-contradicting positions and of her unperceivable understanding (by me at least, of which she is, apparently, well aware). She almost apologizes for speaking positively about the Germans and for seeing them as people rather than as 'animals', weak, admittedly, but themselves controlled by a cruel master, who probably answers the demonic description.

My mother's perception includes all types of Germans, and there, are, of course, bad and even vicious Germans, as, for example, a Ukrainian SS female soldier who was most vicious. She walked around with a stick 'Like this in one hand ... and with it she was a queen there. Heaven help her victims, poor things. I still remember her, just like that with the stick in her hand walking around in the barracks where we lived ... I think they killed her after the war.'

At the same time she expresses understanding and even compassion for women recruited by the Nazis to serve as SS. She says,

> They were poor things. Suddenly you see her with such a belly, Hitler wanted them to bring children into the world ... so they made children with every soldier, ... And he took the children, so there would be future generations since maybe some 20 million Germans died in the war. So they would be soldiers. He thought of that, that Satan ... they would wander around there pregnant and suddenly vanished. Went to give birth and returned a few weeks later. I remember them like that, little they were; 'Loss', 'Loss', 'move, quickly' ... and they were sometimes worse than the men.

The memory of their evilness mingles with the understanding and forgiveness. They were 'poor things', they were forced and had no choice.

My mother would also give credit to some for being considerate. When she suffered from terrible pains in her legs, after laying railway tracks in the sweltering sun near Danzig, Peppy, her Kapo friend, obtained medication from a member of the Czech SS who worked in the pharmacy. She most forgivingly explains how unlucky the female SS were: 'There were young girls, most of whom left their children with their mothers ... they went because this was their source of income, there was nothing to live off,

after all they were in a crisis situation. They received a wage, it's not like real soldiers. They became wardens.'

When I ask, 'Do you feel sorry for them?' she answers unhesitatingly, 'There were many who suffered, those who didn't do anything. Those who did things got out of there, escaped. Where did they catch Eichmann? In Argentina.'

Terming the Germans, both male and female, as 'poor things', 'innocent' and exploited by Hitler, as needing an income and so on, makes them 'normal' people, according to my mother. At the same time this 'positive thinking' also bolsters her personality and humanness in her self-perception in the present. Thinking about the past in this way empowers her and strengthens her moral positions.

The forgiving approach, interwoven as a consistent thread in her memories, even includes appreciation for those who were supposed, according to my assumptions, to be the incarnation of evil for her. She tells me, 'They also knew how to respect people, if they saw that someone was not lazy and worked, didn't exploit.' I try to direct her attention to the absurdity in her statements about the survivors, as if forced female labourers could take advantage of their German employers, but she insists, 'Whoever did not waste time, you know, who worked and didn't shirk (remaining in the hut when the others went out to work) ...' My reservations do not deter her and she adds positive descriptions of her taskmasters, of their 'consideration', that there were some who allowed them to take pots of water from the barracks in the morning, and when they worked at the airport allowed them to collect potatoes from field close by. The women cooked the potatoes over a fire in the work area and divided them up between themselves. 'Some girls managed to get hold of some salt and even if we ate without salt it kept us going.' She is swept away in her considerate and understanding contemplation of the taskmasters' distress.

> There were also Romanians. There was this young guy, poor thing. Was he guilty? They made him into an SS, to guard the girls. In the beginning he was OK, but towards the end when they were going to lose, they became nervous and began to take out their nervousness on the girls. Then it was very tough.

In other words, she perceives the SS as those caught in a no choice situation, and even then, when they were forced to participate in the death system, some of them managed to preserve some sort of humanness. Only when their situation was under existential threat did the good ones amongst them become cruel. Humanizing the Germans, thinking about them in human terms and as individuals with bearable and even positive nature, helps my mother to remember, apparently, while ignoring threatening, painful memories. And perhaps this helps her to forget what she prefers to wipe out of her memory. This analysis does not deny, of course, the humanness, the consideration, the help and even the good-heartedness of some of the Germans and of others, as arises from my mother's descriptions. My claim hones the central place positive

thinking holds in my mother's memories as a means of alleviating recollection and its implications for her life.

Summary – Moulding and Losing Memories

The main point arising from the conversations with my mother of her Holocaust memories and experiences is her instrumentalization of memories that have been lost, forgotten and/or erased. Prominent in her comments are the inconsistency and self-contradictions in the details of her recollections, in the way she perceives events, people and thoughts from her past. These are connected apparently, to the instrumental use that she makes of her memories, so that they serve her in a mode relevant and necessary to the particular context of our conversation. My mother's use of her recollection is, from this point of view, a practical means. It serves and is adapted in a flexible manner to the way she now conducts herself, during our conversations and apparently at other times too, when she touches on her memories.

My mother's recollection of the events, as emerges from her remarks, replies and explanations, presents them in a more bearable, mellow manner. There is forgiveness, understanding, compassion and even gratitude to the Germans (who helped). They are not a collective in her eyes, and not the absolute symbol of evil, bar the satanic image of Hitler. They are people who react in a different and varying way in situations that present a challenge to the human essence of their personalities. This point supports the argument of instrumental recollection. The expressions connected to the past employ tools that enable my mother to cope with it, whether as a need to cope with the recollection of the unbearable suffering or whether to face the commitment to remember those who did not survive. In the positive statements about the Germans and in her words, 'They were also human beings' my mother finds, perhaps, a way to alleviate the memory of the past as a total horror, inhuman and impossible to grasp. Thus she humanizes the Holocaust and the perpetrators. Her insistence on remembering the events through positive thinking (of good-heartedness, self-sacrifice, help with the weak, luck and so on) indicates the way in which she copes with memories, organizes and processes them in the present.

The anthropologist, at long last, should be satisfied as the journey that passed through unpleasant, undesired, suspicions regarding beloved informant's accounts, ended well. Ambiguous, self-contradicting pieces of information gained sense and coherence by drawing a complex portrait of the person's life story. Thus, every informant becomes, through suspicious observation and loving analysis, a hero/ heroine of his/her narrative.

References

Agamben, Giorgio. 1999. *Remnants of Auschwitz: The Witness and the Archive*, trans., D. Heller-Roazen. New York: Zone Books.

Connerton, Paul. 1989. *How Societies Remember*. Cambridge: Cambridge University Press.

Davies, Martin L. and Szejnmann, Claus-Christian W. 2007. *How the Holocaust Looks Now, International Perspectives.* New York: Palgrave-Macmillan.

Hertzog, Esther. 2007. The Presence of the Shoah in Daily Discourse in Israel, in *How the Holocaust Looks Now, International Perspectives*, edited by M.L. Davies and C.C.W. Szejnmann. New York: Palgrave-Macmillan, 60–71.

——. 2008. Past and Present in my Mother's Holocaust Memories, in *Life, Death and Sacrifice: Women and Family in the Holocaust*, edited by E. Hertzog. Jerusalem: Gefen, 261–86.

Hirsch, Marianne. 1997. *Family Frames: Photography, Narrative and Postmemory.* Cambridge, MA: Harvard University Press.

Le Goff, Jacques. 1992/1977. *History and Memory*, trans., S. Rendall and E. Claman. New York, Oxford: Columbia University Press.

Lentin, Ronit. 2004. Introduction: Postmemory, Unsayability and the Return of the Auschwitz Code, in *Re-Presenting the Shoah for the 21st Century,* edited by R. Lentin. Oxford and NY: Berghahn, 1–24.

Loftus, Elizabeth F. and Ketcham, Katherine. 1994. *The Myth of Repressed Memory.* New York: St Martin's Press.

Prager, Jeffrey. 1998. *Presenting the Past: Psychoanalysis and the Sociology of Misremembering.* Cambridge, MA: Harvard University Press.

Saidel, Rochelle G. 2004. *The Jewish Women of Ravensbrück Concentration Camp.* Madison: University of Wisconsin Press.

Wolf, Diane L. 2007. *Beyond Anne Frank: Hidden children and Postwar Families in Holland.* Berkeley and Los Angeles: University of California Press.

Young, James. E. 2000. *At Memory's Edge: After-Images of the Holocaust in Contemporary Art and Architecture.* New Haven, CT: Yale University Press.

Zertal, Idith. 2002. *Death and the Nation: History, Memory Politics.* Or Yehuda: Dvir publishing house (Hebrew).

Zuckermann, Moshe. 1993. *Shoah in the Sealed Room: The 'Holocaust' in Israeli Press During the Gulf War.* Tel Aviv: Author's publication (Hebrew).

Part V: Oases

Chapter 16: The Significance of Colours in Pastoral Bedouin Society

Aref Abu-Rabi'a

In this chapter I describe the symbolic significance of colours, and their use in medical therapy and rituals, in pastoral Bedouin society. I focus on Bedouin in the Negev Desert, especially those who continue to raise livestock. I do not discuss the linguistic aspects of colour terms.

Methodology

During the decade 1990–2000 I studied folk beliefs and traditional medicine among Bedouin in the Negev, the Gaza Strip, Sinai and Jordan. This chapter deals with one facet of that study, namely the symbolic significance of colours, and their use in medical therapy and rituals, among Bedouin in the Negev who continue to raise livestock. Data were collected in unstructured interviews with traditional healers and other Bedouin, as well as by field observation. The interviews were carried out in the informants' homes. All the informants were married and over the age of thirty. Most of the healers were between forty to eighty years old. Most of the material was recorded in field logs, and some of it was taped.

It is usually difficult to gain more than a superficial knowledge of intimate life in other cultures, as every anthropologist knows. However, since the fieldwork for my Ph.D. thesis in 1983–5 I have maintained close personal ties with many Bedouin in the region. Many have known me since childhood and feel at ease with me, and most of them were glad to share information with me.

Background

Badawi is Arabic for Bedouin. It is derived from *badia*, the Arabic word for desert. Today (2011) some 60 per cent of the 180,000 Bedouin in the Negev live in seven towns, and most of the remaining 40 per cent live in tribal settlements of varying sizes, in clusters of tents, in wooden or metal huts, or in cement block or stone houses. The

Bedouin population is divided into two main groups according to type of settlement: First, those residing in seven towns established by the state authorities: Rahat, Tel-Sheva [Tel al-Sab'a], Kesifa [Kuseife], 'Aro'er ['Ar'ara], Segev Shalom [Shgib al-Salam], Hura and Laqiya. Second, those residing in dispersed, unauthorized settlements outside the seven towns, in concentrations of varying size (Abu-Rabi'a 2006: 865–82). After the establishment of Israel in 1948, most of the 75,000 Negev Bedouin were driven off their land and many were expelled from the country and became refugees. Those who remained in the country were concentrated in a reservation governed by a military administration. There they had to rely on the local resources, and engaged in dry farming and pastoralism. In the early 1960s the military administration was dismantled, the Bedouin gradually re-entered the labour market, and their economy diversified rapidly. Still many of them continued to raise sheep and goats, as well as a few camels. Bedouin often believe that there is a division of roles between the sexes. The main duties of the husband concern affairs outside the home and politics. The wife deals with household duties and care of the children. However some tasks, such as caring for livestock, are carried out by both husband and wife (Abu-Rabi'a 1994: 60; Marx 1967: 83; Marx 1987: 156–79).

Livestock

The flock is considered the property of all members of the nuclear family. It is a source of pride and profit, of wealth and social status. The sale of animals is a major source of income, and only a small number of animals are consumed at home. Still, the flock supplies dairy products and wool, and some meat to the household. The joint ownership of the flock is an important factor in engendering co-operation between all family members. Caring for livestock requires a great deal of interaction between family members, and serves as an expression of solidarity between them. Moreover, the flock plays a central role in all rites of passage in the life of a Bedouin: when he is born, circumcised, betrothed, gets married, makes a pilgrimage to Mecca, and when he dies.

The ceremonial slaughter of sheep, in which the whole family participates, must take place on the occasion of each of these milestones. The gathering of the family, in joy or mourning, strengthens the Bedouin's belief in the importance of such ceremonies. Sheep and goats have become carriers of a great amount of symbolism. It is around them that ties are wrought between man and God, the prophets and the righteous: both people and flocks make pilgrimages to the tombs of saints. Thus the flock is central to Bedouin life on several levels: it is a source of income, a focus of social ties, and a depository of symbols which are manifest in modes of thought and certain beliefs.

Colours Associated with Livestock

The colour white in Bedouin culture bears an emotional and ideational charge of gratitude and esteem, happiness, good will, peace, purity, and fertility. It is also a charm against the evil eye. It embodies the idea of blessing, as it is the colour of milk, esteemed by the Bedouin as the most important dairy product.

This is shown, for example, in the Bedouin ceremony of Bleating Friday (*aljim'aih al-khawwara*), held on the Friday of the third week of the month of *alkhimis*, which roughly coincides with April. This particular Friday is considered a livestock festival which herd owners mark by not churning milk. They drink only a portion of it, and eat a piece of bread and butter. They give the rest of the milk to relatives and the poor, who are allowed to churn it. On the morning of the feast, the backs of the sheep are painted bright red, to defeat the evil eye. The festival comes near the conclusion of the year's milking cycle, and marks the commencement of the animal cycle of the succeeding year – rutting and mating, impregnation, and birth.

The marking of each woman's milk-ewes is performed by various ways of rubbing paint on the horns, thigh, head, or on a leg. If paint is not available, coloured threads are tied on different parts of an animal's body. The preferred colour is red, the colour of blood. The belief is that when the evil eye approaches to attack the flock, it will think that the flock has already been harmed and will leave it alone. Sick animals wear amulets, curative beads and pieces of metal with medicinal properties, just like people. For instance, silver is used to cure eye infections of both persons and animals.

Colours Associated with Saints

The tombs of saints play an important role in Bedouin life. When a Bedouin travels to the tomb of a saint, he or his wife ties a white scarf (*rayih baydha*) in a conspicuous place at the front of their car, so that the scarf flutters in the wind and can be seen from a distance. In the course of the visit itself, a white cloth is hung on the tomb, and incense (*bakhkhur*) is placed in a special niche and burnt. The white cloth on the car is a token of engendering a link to God, as well as a reminder to all who see it. It is a token of esteem and thanks to God and the saints. The tying of the cloth to the tomb of the saint symbolizes joy and good will, as well as the connection with the saint and the contracting of a covenant of eternal allegiance to him. The link with a saint enables a Bedouin to have a sense of control over his existence.

By means of the white cloth, which was originally part of his clothing, the Bedouin beseeches the patronage of the saint. The white cloth symbolizes several pieces of clothing he wears in the course of life: from the diaper, the first piece of clothing in which a Bedouin is swathed as an infant, through the white pieces of cloth he wears on his pilgrimage to Mecca, to the white shroud that covers him at death.

Upon their return home, the Bedouin slaughter a sheep in commemoration of the saint. The meat is boiled and put into bowls with pita bread and rice and passed around. Men and women eat in separate groups. Some of the male children join the women and some eat with the men. At the end of the meal, at the request of the host, all those sitting around the feast extend their arms, palms upward, and say the *al-fatiha* (the first chapter in the Quran), at the end of which all present say*, 'amin, zwaritkum magboula'* (Amen, your pilgrimage to the tomb of the saint has been accepted). Parallel ceremonies are conducted in the men's and women's sections. The still half-full bowls are then returned to the kitchen, where the remaining food is distributed. Before the feast, the hostess prepares sweets (*fraha*) for each of the women present, and as she distributes them, she explains that they are for the children. The sweets symbolize joy, good luck, blessing, and hopes for a sweet life, both for giver and recipient.

The ceremonial visits to the tombs of saints certainly engender a psychological and therapeutic inter-dependency between the Bedouin and his saints. This interdependency is firmly rooted in faith and strengthened and legitimized by Bedouin culture (Abu-Rabi'a 1983). In the desert, there are trees that grow near the tombs of saints. It is believed that a saint's spirit dwells in their shadow. Hanging white strips of cloth on these trees or on a saint's tomb serves as an expression of recognizing a saint's holiness.

Gender and Fertility: The Colour of Clothing

Muslim religion advocates full public separation between men and women. It has also established a clear code of conduct for meetings between members of opposite sexes. Under certain circumstances, a woman may not have any visual or physical contact with a man. These rules also define which clothing fashions are acceptable or unacceptable, in accordance with current moral codes. Rahaman (1986) attributed the modesty of women's dress to Muslim law and custom. The requirement for modesty demands the complete covering of women, from head to toe, in a wide, full style which blurs the contours of a woman's body, and obscures such changes as pregnancy (Flugel 1930). The code of modesty, which applies more strictly to women's clothing than it does to men's, is based on two contradictory assumptions. On the one hand, women are thought of as weak and in need of male help to defend their honour and maintain their safety. On the other hand, women are considered to have strong sexual urges, which threaten the honour of men and the integrity and homogeneity of the group (Rugh 1986). The concept of 'woman's honour' (*'ard/'ird*) has deep roots in Bedouin culture (Kressel 1992: 160–216; Stewart 1994). A dishonoured, even if guiltless, woman brings shame on the whole clan.

Clothing characterizes the social identity of members of each sex, and determines the position of the individual in the sub-group and the limits of social interaction (Vener and Hoffer 1965). According to Mass and al-Krenawi (1994: 357–67), the Bedouin woman passes through three stages in her life. These stages are identified by the type

and the colour of clothes she wears. At each stage, she has a different status, and the prohibitions on her social relationships change accordingly. From early childhood until marriage, she is separated from boys. The characteristic clothes for this stage are a traditional dress, with blue embroidery and a blue belt. She is not allowed to wear makeup.

The second stage commences with marriage. At the wedding ceremony, she wears red embroidered clothes and a red belt (*sufiyih*), a symbol of fertility, and everyone who sees her knows she has become a married woman.

In general, the colour red (*ahmar*) symbolizes joy, love, fertility (above all, the birth of a male child), and the virgin's hymen. There is a clear connection between the colour red, and fertility and sanctity. The use of the colour red enables a Bedouin woman to have a sense of fulfilment: she is ready to become a mother of children and thus to ensure the future of the household and the clan. Her status rises considerably; she is treated with respect when she bears many male children. On the other hand, the position of a sterile woman, or one who bears only daughters, is very low. In such cases, the husband's parents and relatives encourage him to marry a second or even a third wife, who can bring him sons. In the Bedouin belief system, failure to bear children, or to bear sons, is ascribed to the wife, and not to the husband.

During the third stage of a woman's life, after menopause, she wears blue clothes and a blue belt, symbolizing non-fertility and non-sexuality. Her social position rises, mainly because she has by then become the mother of grown up sons, and in certain cases she may sit with the men of her agnatic group. Blue or azure (*azrak, ashhab*) symbolizes, aside from old age, a state of being worn out, sadness, or mourning; women in mourning, like elderly women, tend to wear these colours.

The duality in the meaning of colours and the duality of sexuality should be stressed here. Both young women and old women wear blue. Young women before the age of fertility, and elder women after fertility ends, are both classified as not having a sexual identity. However, at the age of fertility, the distinction between the sexes is highly emphasized.

Married Bedouin women wear a plaited woollen girdle decorated with cowrie shells. The girdle has two parts: a broad striped grey and black under-girdle, and worn over it a narrower black sash with coloured chevron patterns, and fringed and tasselled ends (Weir 1990). The importance of the cowrie shell need not always be solely based on gender. Bedouin from Southern Sinai could wear them on a braided thread around their waist under their clothes, just as a more discreet way of wearing an evil eye pendent – rather than as a necklace, like the girls (Gardner 2005).

The girdle (*sufiyih*) worn by young married Bedouin women, symbolizes a woman's being at the height of her fertility and sexual activity. The way the girdle is worn indicates that the breast and womb are not separate. It also shows that the woman's body is 'closed' in the same way as a sack (*kiys*), by firmly tying a rope (*habil*) around it. The sack symbolizes the woman's vagina, and the rope symbolizes the man's penis. It is worth noting that the girdle is untied in public, except when making a pilgrimage

(*zwarah*) to the tomb of a saint (*wily*). The woman lies down and stretches out on the tomb and beseeches the saint to help her become pregnant, or to appeal for a private request. This ritual is called *traffil 'alih*, which means 'she prostrates herself over him'.

Bedouin women adorn their hair with strings of cowrie shells called *wada'*. From the side, with their openings facing outwards, the cowrie looks like a woman's vagina, while the colour of the cowrie is white, as is a man's sperm, symbolizing fertility. When a woman wears the cowrie string, she intimates that she is at the peak of her fertility. If a Bedouin woman does not have access to cowrie shells, she will collect the bulbs of the Colchicum (*wada'*) as she herds her livestock. The bulbs of the *wada'*, incidentally, are used as a medicine to treat sexual diseases of both men and women, among the Bedouin of the Middle East.

Women are considered especially vulnerable while giving birth and breastfeeding. Hence, a whole range of charms intended for decoration and defence are designed to be placed on their chest. A large charm which forms part of the dress wards off evil spirits and bad luck.

When Bedouin women are in mourning, the *sufiyih* (girdle) is removed and replaced by a black or grey sash. They cover whatever gold they are wearing with black or green cloth. They intertwine black threads and small black or blue beads in their jewellery. All women in mourning cover their regular clothes with a dark or a green fabric (Bar-Zvi, Abu-Rabi'a and Kressel 1998). It should be noted that gold is a favoured colour of many Bedouin women in the Sinai, because of the properties associated with the metal itself, not just the colour (Gardner 2005). In the Negev, gold is associated with acquiring property and symbolizes fertility as a good omen.

The range of colours of the dress of Bedouin women in the Negev is of course dynamic. New trends are continually developing (Tal 1995).

Purity and Colours

Traditional Bedouin women will shake hands with males of their agnatic group only after covering their hands with a white handkerchiefs (*mandil/shash abyadh*), as during her menstrual period. This is to deliver the message that the 'barrier' is preserved, as she is considered then to be 'impure'. At the same time, the woman is also 'protecting' herself, and not just the man, as she does not know the condition of the man shaking her hand. In Bedouin belief, the colour white saves from the influence of impurity, pollution, sorcery, evil eye, and evil thoughts.

If a breast-feeding mother has little milk, she is fed spurge (Euphorbia: *lbina*, *lubbina*), a plant with white sap, that has the consistency of mother's milk. If her nipple is infected or swollen, preventing her from feeding her infant, she will wear a white coral necklace, called breasts' bead (*kharazat al-dioud*) which, according to folk wisdom, will cure her. The coral is white and perforated, as are her nipples. Therefore, Bedouin women sprinkle ground coral on their breasts, to ensure plenty of milk and

cure any swellings and pollution. These practices are based on a belief in the reciprocal action of like on like. Some of these practices also occur in other cultures (Turner 1967: 19–47 and 59–91; Frazer 1950: 11–48).

Black (*asmar*) symbolizes modesty and making do with what little one has. Thus, for example, Levi notes (1987: 200) that Bedouin women in the Sinai wear black clothes to make themselves visible in the desert; it is critical that constant eye contact be maintained between the women who go out with the flocks and those who remain in the tents. Such contact is necessary to maintain a feeling of security and well-being in the empty expanses of the desert. The woman must stand out against the background of the bright-coloured hills and plains, so that she may be noticed and get help, should the need arise. Furthermore, Bedouin believe that black clothing does not show up dirt, which is especially useful in desert conditions.

The Bedouin woman does not decorate her dresses to any great extent, and in any case of such adornment, she uses the colours red, yellow, or green. Bedouin women favour these bright colours as they contrast with the black material of their dress. The Bedouin tent is made of black goat's hair, as are most of the rugs, and even some of the clothing. Unmarried young virgins, elderly women, and widows dress in black and grey, to emphasize their personal status and sex. The Bedouin drink bitter black coffee on festive days, to symbolize hospitality and generosity in the desert. However, during mourning, bitter black coffee symbolizes sadness. Thus, the particular meaning of a symbol depends on the context. In mourning, both men and women dress in black or grey. The black colour also symbolizes the dark clouds which bring crucial rain, making possible the growth of grass and grains. Rain helps the pastoral Bedouin to pass from a state of pollution and impurity, to one of purity. Rain is an important source of water to the pastoral Bedouin, in which context black has a positive connotation.

Health, Illness, and Misfortune

When a woman gives birth to a boy, a white flag is hung on her tent, to symbolize new life and the group's continuity. The Bedouin treat jaundice (*safar*) by wearing a yellow chain of beads called an *anbar* (amber). The *anbar* is made either of yellow wood or yellow beads, the belief being that it will cure jaundice, since both are yellow. Likewise, people with measles (*hasbih*) wear red. Here, again, the practice is based on a belief in the reciprocal action of like on like, or 'sympathetic magic' as described by Frazer (1950: 11–48).

Nature has given the Bedouin flora and fauna with curative powers, of which he has learned to make use for hundreds of years. In mediaeval Europe, the fifteenth-century Swiss physician Philippus Paracelsus developed a system of plant medicine that came to be known as the Doctrine of Signatures. Paracelsus contended that plants were created for man's use, and were 'signed' in their individual shapes by the Maker, in accordance with the purpose He had intended them to serve man. The form of a plant, its colour,

and its habitat provide hints as to its medicinal traits. For example, yellow plants, or those with yellow juice, were deemed effective against jaundice. Unique qualities were ascribed to red plants, qualities that made them effective cures for disorders of the bloodstream. Plants with white sap were believed to be conducive to the production of milk in breastfeeding women (Palevitch and Yaniv 1991: 12).

Clark (1984: 43–9) notes that there are colours that have a calming effect on people, and help them to get into a good mood, while other colours make them angry, and may even engender physical discomfort. There are people who wear quiet colours when they are full of energy, but white when they are in poor spirits. According to Shinar (1982: 29–42), colours are commonly claimed to have a certain magic significance. For example, in Morocco, white is claimed to have the ability to dazzle the evil eye.

Among different societies in Africa, colours and combinations of colours are used for different purposes, e.g. medicine, cosmetics, and social rituals (JacobsonWidding 1979: 220–72). According to Laman (1968: 68), all colour terms among Congo people designate shades of red, white and black, so that only these three colours are identified separately. The palette of the Negev Bedouin is richer. According to Borg (1999: 122), the most ancient version of the Negev colour paradigm comprises the first six basic categories in the Berlin and Kay (1969: 19–23) evolutionary sequence: white, black, red, green, yellow, and blue. Borg (1999: 123) claims that the stability of the Negev Bedouin's colour system over time is also plausibly ascribable to cultural factors specific to the Bedouin way of life, in particular the ecology.

Out of the six colours, three are dominant among the pastoral Bedouin: white, black, and red. It should be noted that Turner (1967: 80–82) found that these three colours are common to all human beings because they are deeply rooted in their life cycle. Buckley's (1985) study of Yoruba medicine too finds three symbolic colour schemes: black, white, and red. These colours are related in a symbolic way to soil, sky, and clay; and by extension, to skin, semen and blood. The model for fertility is explained by the agricultural model of the sky penetrating the topsoil and fertilizing the seeds in the clay. Symbolically, this is interpreted as white penetrating black and mixing with red to create life. Likewise, semen entering the black skin and mixing with the red blood inside produces offspring. Further, it explains why sex during menstruation is considered un-fertile, if not taboo. In this scenario white mixes with red as it seeps out from black. If the clay is turned out over the soil, the land is no longer fertile.

Summary and Conclusions

A pastoral Bedouin's life is centred on his family and his livestock, which are both the source of his livelihood and the focal point of his extended family's activities. He also has a unique faith in the rapport between man and his animals and his Maker, through the agency of saints. These basic elements constitute the background for understanding the symbolic significance of colours and their therapeutic effects for the

Bedouin. The symbolic significance of a colour often depends on a particular context: whether a man wants the public to know that he is visiting the tomb of a saint, to protect his livestock from the evil eye, whether he is planning revenge or cares for his prosperity, or whether he is in mourning. Sometimes a colour has a purely functional purpose, as when womenfolk wear black for safety, or to be clearly outlined against the desert landscape. Therefore the symbolic significance colours for the pastoral Bedouin reflects his environmental and social circumstances, economic situation, religious and folk beliefs, and the exigencies of situations. The range of meanings can only be deciphered if the context is taken into consideration.

References

Abu-Rabi'a, Aref. 1983. *Folk Medicine among the Bedouin Tribes in the Negev.* Sde-Boker: Jacob Blaustein Institute for Desert Research, Ben-Gurion University of the Negev.

——. 1994. *The Negev Bedouin and Livestock Rearing: Social, Economic and Political Aspects.* Oxford: Berg.

——. 2006. A Century of Education: Bedouin Contestation with Formal Education in Israel, in *Nomadic Societies in the Middle East and North Africa: Entering the 21st Century*, edited by D. Chatty. Leiden: Brill, 865–82.

Bar-Zvi, Sasson, Abu-Rabi'a, Aref and Kressel, Gideon. 1998. *The Charm of Graves: Mourning Rituals and Tomb Worshipping Among the Negev Bedouin.* Tel Aviv: Ministry of Defense and Ben-Gurion University of the Negev (Hebrew).

Berlin, Brent, and Kay, Paul. 1969. *Basic Color Terms: Their Universality and Evolution.* Berkeley: University of California Press.

Borg, Alexander. 1999. Linguistic and Ethnographic Observations on the Color Categories of the Negev Bedouin, in *The Language of Color in the Mediterranean*, edited by A. Borg. Stockholm: Almqvist and Wiksell International, 121–47.

Buckley, Anthony D. 1985. *Yoruba Medicine.* New York: Oxford University Press.

Clark, Linda. 1984. *The Ancient Art of Colour Therapy.* Tel Aviv: Or-Teva.

Flugel, John. 1930. *The Psychology of Clothes.* New York: International Press.

Frazer, James. 1950. *The Golden Bough: A Study in Magic and Religion.* London: Macmillan.

Gardner, Ann. 2005. Personal communication, 29 April.

Jacobson-Widding. Anita. 1979. *Red-White-Black as a Mode of Thought: A Study of Triadic Classification by Colors in the Ritual Symbolism and Cognitive Thought of the Peoples of the Lower Congo.* Uppsala: Almqvist and Wiksell.

Kressel, Gideon. 1992. *Descent Through Males: An Anthropological Investigation into the Patterns Underlying Social Hierarchy, Kinship, and Marriage among Former Bedouin in the Ramla-Lod Area, Israel.* Wiesbaden: Harrassowitz.

Laman, Karl E. 1968. *The Kongo III*. Uppsala: Studia Ethnographica Upsaliensia XII.

Levi, Shabtai. 1987. *The Bedouin in the Sinai Desert.* Tel Aviv: Schocken (Hebrew).

Marx, Emanuel. 1967. *Bedouin of the Negev.* Manchester: Manchester University Press.

——. 1987. Relations between Spouses among the Negev Bedouin. *Ethnos*, 52(1–2): 156–79.

Mass, Mili, and al-Krenawi, Alean. 1994. When a Man Encounters a Woman, Satan is Also Present: Clinical Relationship in Bedouin Society. *American Journal of Orthopsychiatry*, 64(3): 357–67.

Palevitch, Dan and Yaniv, Zohara. 1991. *Medicinal Plants in the Holy Land*, vol. 1. Tel Aviv: Tammuz.

Rahaman, Afzalur. 1986. *Role of Muslim Women in Society.* London: Seerah Foundation.

Rugh, Andrea. 1986. *Reveal and Conceal: Dress in Contemporary Egypt.* Syracuse: Syracuse University Press.

Shinar, Pessah. 1982. Elements of Magic in Modern North African Jewelry. *Pe'amim*, 11: 29–42 (Hebrew).

Stewart, Frank. 1994. *Honor.* Chicago, IL: University of Chicago Press.

Tal, Sigal. 1995. *The Negev Bedouin Woman in an Era of Changes.* Lahav: Joe Alon Centre (Hebrew).

Turner, Victor W. 1967. *The Forest of Symbols: Aspects of Ndembu Ritual.* Ithaca, NY: Cornell University Press.

Vener, Arthur M. and Hoffer, Charles R. 1965. Adolescent Orientation to Clothing, in *Dress, Adornment, and the Social Order*, edited by M.E. Roach and J.B. Eicher. New York: Wiley, 76–81.

Weir, Shelagh. 1990. *The Bedouin.* London: British Museum Publications.

Chapter 17: The Structure of Bedouin Society in the Negev: Emanuel Marx's Bedouin of the Negev Revisited

Frank H. Stewart

I. Introduction[1]

The Bedouin have played a major role in world history, and the literature dealing with them is correspondingly large. But in spite of earlier contributions by some outstanding authors, it was only in 1967, with the publication of Emanuel Marx's Bedouin of the Negev, that we had for the first time an analysis of a Bedouin society that came up to the standards set by modern social anthropology. In recent decades a number of other professional anthropologists have published books on the Bedouin, but Marx's work retains—and will continue to retain—a central place in the field.

The great merits of the book are its determination to penetrate beyond the superficialities with which many earlier investigators were satisfied, its insistence on argument rather than the mere formless accumulation of data, and its extensive presentation of the rich factual material on which it is based. To this day, no other study of the Bedouin has given us anything like the detailed information that Marx offers about such subjects as marriage patterns, social structure and land distribution. *Bedouin of the Negev* is not, however, an easy book to read, and I hope that this chapter will make it a little more accessible.

One of the difficulties of Marx's book is that his terminology is liable to cause confusion. He uses a large number of technical terms, and this chapter, in an attempt

---

to clarify his analysis, introduces even more. Some general remarks about terminology may therefore be helpful. The technical terms that appear here fall into three classes.

(i.) Those used by me, but not by Marx. These are defined in due course (notably in section III), and will, I hope, present no problems to the reader.

(ii.) Those used both by Marx and by me, e.g., tribe, sub-tribe, co-liable group. In every case I have at some point explained both how Marx uses such a term and how I use it. Almost always the sense that I attach to such a term is rather different from the one that Marx attaches to it. Unfortunately it has been necessary on occasion to use certain of these terms before they are fully explained. In such cases, the reader can sometimes tell from the context whether the term is being used in my sense or in Marx's sense; where the context does not make this clear, then it does not matter which way the term is understood.

(iii.) Those used by Marx, but not by me, e.g., core-group, section. I have noted and tried to clarify these terms in their place. What Marx calls a core-group I call a sub-tribe. This is explained in section V.2, though the term core-group appears several times before then.

I have only touched on those topics about which I had something to say; most of what is in Marx's book is not discussed here. I shall draw my data largely from the book itself, but my understanding of them is conditioned by the fact that I myself spent six years (1976–82) studying the Bedouin of central Sinai, whose social structure is similar to that of the Negev Bedouin.

Marx's book was originally written in English (1967), and was later translated into Hebrew (1974). The Hebrew version differs only slightly from the English one.[2] In what follows, I shall give references to the book in the form (111/222) where 111 is the page of the English version on which the relevant passage appears, and 222 the page of the Hebrew version.

Bedouin names mentioned by Marx are generally given here in his transcription, though without diacritical signs. The letter "g" in Marx's transcription represents a sound like that represented by the letter "j" in English.

II. The Tribes and their Land

II.1

Marx's book describes the Negev Bedouin as they were in the years 1960–63. At that time they numbered about 16,000 souls (12/18).[3] They were divided into two main classes: those I shall call the *True Bedouin*, and those I shall refer to as the

---

[2] For details of the differences, see Stewart 1991a: 132–3.

[3] According to official figures. In a personal communication, Uri Mintzker notes that the real number of Bedouin in the Negev was probably higher, since many of them were not registered with the authorities.

Fallahin, i.e., people of peasant origin whose ancestors had left their villages and come to live in tents among the True Bedouin. Marx was the first to investigate in detail the distinction between Fallahin and True Bedouin, but he does not tell us in the book what proportion of the total population were True Bedouin, and what proportion Fallahin. It appears, however, that the True Bedouin were a majority.[4] The two groups virtually never intermarried (67, 80, 114/61, 71, 99), nor were True Bedouin and Fallahin ever to be found camped together (179/148), except in the camp of a sheikh (88/78). Even the Arabic dialects spoken by the two groups were different (Henkin 2010).

Both these main groups had connected with them people of sub-Saharan African origin. I shall refer to them as the Blacks. Their number is not recorded, but was evidently not large. Some Blacks were once slaves of the Bedouin, or were the descendants of such slaves. Other Blacks were Fallahin.[5] The Blacks married only among themselves.

In what follows, I shall, when writing of the Negev, use the term *Bedouin* to cover not only the True Bedouin, but also the Fallahin and others who lived in tents among the True Bedouin.[6]

II.2

Of some 65,000 or more Bedouin who lived in the Negev before 1948, only about 11,000 remained in 1953 (12/18). Like the other Israeli Arabs, they were at that time (and were still during the period when Marx was in the field) under Military Administration. In 1954 there was a "complete registration" of the Negev Bedouin, every one of whom "had to have a tribal affiliation, which was entered on his identity card" (40/40–41). What this meant in practice was that each Bedouin was registered as attached to a particular sheikh. There were eighteen (44/43) or nineteen (12/18) such sheikhs, all but one of whom appear to have been True Bedouin.[7] It seems that the government also intervened at the level of the subtribe: Marx mentions two sub-tribal chiefs being appointed or recognized by the authorities,[8] and also implies that membership of the sub-tribe was registered with the government (69, 78/63, 70).

For a variety of reasons, the eighteen or nineteen tribes formed by government registration bore only a very rough relationship to the real tribal structure of the

---

[4] In another publication Marx suggests that the Fallahin made up half the population (1977: 354), and estimates by other well-informed observers also suggest something of the sort. In contrast, the only detailed figures that I know of, which refer to the year 1998, indicate that the Fallahin made up only about 22 percent of the total Bedouin population (Ben-David 2004: 126–7).

[5] Randolph 1963: 36–7. In this work Randolph gives the results of his fieldwork among the Negev Bedouin in 1961–62.

[6] Marx himself sometimes uses the term Bedouin in the fashion proposed here, and sometimes uses it to refer specifically to what I call the True Bedouin.

[7] The exception is the head of the Abu al-Qi'an group; he and his followers were Fallahin (13, 72/19, 65).

[8] Suliman Abu Msa'ad and 'Iyadeh Abu 'Aginah of the Ma'abdah (70, 154–5/63, 115).

population;[9] and subsequently groups often moved from one tribe (or sub-tribe) to another without this change being registered with the government (68–70, 78/62–3, 70).

These facts must always be borne in mind when looking at statistical data which Marx draws from official sources. So, for example, the population figures in the figure headed "Bedouin tribes of the Negev" (13/19) simply reflect the government's records. The situation on the ground was certainly different, in some instances perhaps radically different, from what the figure indicates.

II.3

One of the aims of this chapter is to try to clarify, on the basis of Marx's data, what I have called "the real tribal structure of the population." That is to say, I shall attempt to describe the types of autochthonous political group that existed among the Negev Bedouin in the early 1960s and the relationships between those groups. The question of who controls the land is of central importance in this context, and here we are concerned with two distinct legal systems. One is the law of the State of Israel, the other is the customary law of the Bedouin. Marx was able to gather only limited information about the latter, but what he says can be supplemented from the fuller information that is available about the land law of the Sinai Bedouin.

This procedure is easily justified. Al-'Arif's notable work on the customary law of the Negev Bedouin deals fairly briefly with the land law (1933: 168–75, 235–40), but both here and over the wide range of other topics covered, the information is more than enough to show that Negev Bedouin customary law was for all practical purposes the same as that of the Sinai Bedouin. Marx himself has emphasized that up to the foundation of the State of Israel in 1948, "one dealt with a single Bedouin population ranging over Sinai and the Negev" (7/omitted in the Hebrew). He also noted that in the 1960s the Negev Bedouin still looked to Sinai "for advice on customary law" (5/14).

Let me present, then, a brief sketch of the customary land law as I found it among the Ahaywat tribe of Central Sinai in the late 1970s. At that time the tribespeople were still entirely nomadic. In Bedouin law, the Ahaywat tribe constituted a *corporation*, that is to say an entity that had legal rights and duties. As such, it was represented by the sheikh of the tribe. The tribe had a territory with well-defined (though not always undisputed) boundaries, and any negotiations or litigation about these boundaries would be carried on between the sheikh of the Ahaywat and the sheikh of whatever neighboring tribe was involved (Stewart 1986).

Land in the tribal territory fell into two categories, claimed and unclaimed. The claimed land was privately owned, heritable and (with certain restrictions) alienable;

---

[9] Randolph (1963: 132–4) gives details of how the component sections of the Qderat tribal group were distributed among the officially recognized tribes, and remarks that "In many cases the descent groups are displeased with this forced affiliation into new ashayir [tribes]."

it consisted of what might charitably be called arable, though in most years there was not enough rainfall to allow cultivation. Virtually all the arable in the tribal territory belonged to male members of the tribe. The unclaimed land was what I shall refer to as pasture (though large areas were in fact entirely barren). All men of the tribe had equal rights in this land.

Access to the territory was controlled by the tribe. Generally speaking, Bedouin from neighboring tribes were allowed to graze on it as freely as the Ahaywat themselves, and the Ahaywat enjoyed reciprocal rights in the territories of their neighbors.[10] But other rights in the land were reserved to the Ahaywat alone. For instance, any Ahaywiy man, but only an Ahaywiy, could acquire ownership of a piece of unclaimed land simply by demarcating the area and announcing that he thereby appropriated it.[11]

In what follows I shall use the term *territorial rights* to refer to rights of the kind that the Ahaywat tribe claims over its territory, and the term *territorial group* to refer to a group that claims territorial rights.

In the nineteenth century (and earlier) the whole of the Negev was divided between various Bedouin groups.[12] At that time the Bedouin—among them the Ahaywat, who until 1948 occupied not only a large part of central Sinai but also the southern Negev— dominated the region. Until the end of that century their customary land law was the only one that mattered, and while land registration, and the accompanying state law, made some inroads in the late Ottoman period and during the British mandate, most Negev Bedouin held only customary law titles to their arable land (Abu-Rabia 1994: 15). Now Marx, writing of the early 1960s, says that "they are particularly anxious about everything relating to the ownership of land" (57/53), and that they "make every effort to keep the authorities in the dark about matters of land ownership" (177/ 146). It is evident, then, that the customary land law was still a living force at that time. This argument applies with more force to the law that relates to arable than it does to the law that relates to pasture, but Marx leaves no doubt that in the early 1960s the notion of rights in pasture land being vested in a group also continued to be strongly maintained among those whom he studied (134–6/122–4).

---

[10] See Abu-Rabia 1994: 49–50 on similar arrangements in the Negev.

[11] The Bedouin use the transitive verb *hajar* to refer to this action (al-‘Arif 1933: 235; cf. Stewart 1988–90, vol. 2: 231). The word appears in a sentence quoted by Marx (1973: 412).

[12] A number of European maps from the late Ottoman period indicate roughly the locations of the tribes in the Negev, but Marx tells me that he was mistaken in referring to a map of tribal territories prepared by the Ottoman authorities in 1917 (9/16, cf. 95/84). The map he saw was merely an enlarged copy of the well-known one that appears in al-‘Arif 1934. Others also write of an official Ottoman map showing tribal territories: Gal-Pe’er (1979: 275, citing Marx as his authority), Ben-David (1989: 188; 2004: 32, 40, 120, 269), Meir (1996: 196) and Kressel (2003: 63 n. 23). Inquiries to the last three reveal that none of them has seen the purported map, or even documentary evidence of its existence.

II.4

Let me turn now to Israeli law as it affected the Bedouin in the early 1960s. At that time they were largely confined to a reservation which lay to the east of Beersheba. The area of the reservation was about 1,000 sq km, of which about 400 sq km was arable (14, 19, 54 n. 3/19, 23, 56 n. 42). The Bedouin derived a large part of their livelihood from agriculture. According to the customary law of the Bedouin almost all the arable land in the Negev belonged to the True Bedouin, and up to 1948 this claim was not in practice disputed. After 1948 the State of Israel claimed ownership of most of this land. Within the reservation, the authorities had, by 1960, gained control of 38 per cent of the 400 sq km of arable (54/50).

The reservation included all or most of the arable land belonging to members of five of the eighteen (or nineteen) tribes, and part of the arable belonging to members of two others (35, 55, 61, 244/37, 51, 57, 224). I shall refer to the former group as the *landed tribes*, the latter as the *partially landed tribes*. The members of the remaining tribes owned only arable that was outside the reservation, and that they were forbidden to cultivate. They therefore had to lease arable land on the reservation,[13] either from the 62 per cent that was controlled by other Bedouin or from the 38 per cent that was controlled by the State. Members of the partially landed tribes also had to lease such land in order to make ends meet. Since the Fallahin owned only small amounts of land, they too were forced to rely largely on leasing. Where the land belonged to True Bedouin, Fallahin (and presumably also True Bedouin) leased it on the basis of share-cropping (75, 197 n.1/68, 162 n. 38).

Marx's research concentrated on the five landed tribes. Three of them (Abu Rbe'ah, Abu Qrenat and Abu Gwe'id) belonged to the Zullam, an entity of a type that Marx calls a *tribal group*.[14] The official figures indicate that each of these three, together with its attached groups (mostly Fallahin), numbered on average roughly 1,500 souls (13/19). The Zullam also had a fourth member, a small tribe called the Kashkhar (or Ganabib), numbering about 150 souls.[15] Its members lived in the Negev, but not on the reservation,[16] and they were not officially recognized as being resident in Israel

---

[13] Only exceptionally were they able to lease such land outside the reservation (53/50).

[14] Some of the other tribes in the Negev were also, according to Marx, organized in tribal groups (13/19).

[15] The figure for the Kashkhar (13/19) is not an official one, and (if accurate) indicates the true size of the tribe. They did not have any dependent Fallahin (Uri Mintzker, personal communication; Mintzker is currently completing a doctoral dissertation on the Kashkhar).

[16] Some of them were moved to the reservation in 1960 (61, 123/57, 106).

(Marx 1973: 411 n. 2). Of all the Negev tribes, the Abu Gwe'id is the one that Marx describes in most detail.[17]

II.5

It is virtually certain that territorial groups existed in the Negev in the early twentieth century, but the question as to which groups these were is not easily answered. Even the best sources from the nineteenth and early twentieth centuries will usually only tell us where the Bedouin were camped or where they owned arable; like Marx himself, they do not give us many details of the territorial rights claimed by this or that group. In some places there seem to have been sharp boundaries between the territories, as in central Sinai.[18] Elsewhere, though the concept of territorial rights evidently existed, the situation was not so simple. Uri Mintzker, in a personal communication that is based on both information collected in the field and archival evidence, writes that the Kashkhar tribe of the Zullam (whose territory was outside the reservation) had a well-defined boundary with the Subhiyin tribe of the 'Azazmih, with whom they had a tense relationship. In contrast, he continues, no such boundary existed between the Kashkhar and the Sarahin (another 'Azazmih tribe). These two were on good terms with each other, and arable land and cisterns belonging to members of both tribes were (and still are) to be found mingled with each other in the same area.[19]

Marx distinguishes four levels of tribal organization: the *confederation*, the *tribal group* (or *group of tribes*), the *tribe* and the *sub-tribe*. In what follows I shall refer to these as *tribal units*. In asking which of the tribal units claimed territorial rights, I will deal with each type of tribal unit in turn. The discussion will be confined essentially to the Zullam.

The Zullam are affiliated to a larger unit, the Tiaha, which is classified by Marx as a confederation (13/19). The Tiaha are numerous and dispersed, not only in the Negev but also in Sinai, and we can be fairly confident that the Tiaha as such did not possess territorial rights.

The Zullam group has consisted of the same four tribes, living in the same region, since at least the late nineteenth century.[20] In the 1960s the tribal group did not have a leader, nor does it appear to have acted as a body, whatever may have been the

---

[17] The name Abu Gwe'id applies both to a tribe and to a sub-tribe of that tribe. In order to help the reader keep track, I shall in what follows use the name Abu Gwe'id only for the tribe (and its associated clan), reserving the term Abu Gwe'id-Rahahlah for the sub-tribe (and its associated sub-clan, core-group and co-liable group). On the relationship between the Abu Gwe'id and the Rahahlah, see 236/198, and cf. also n. 48 below.

[18] Aref Abu-Rabia tells me in a personal communication that tribal boundaries in the Negev were no less sharp than in Sinai; and the Ahaywat certainly saw their boundaries in the Negev as being just as clearly defined as those in the Sinai (Stewart 1986).

[19] Canaan, in his invaluable article on the 'Azazmih, stresses that this confederation (as Marx calls it) did not have sharply defined boundaries (1928: 90, 118).

[20] For details and references to the older literature, see Oppenheim 1939–68, vol. 2: 129–31.

case in earlier times. Yet it remained a significant entity. Almost all marriages took place within the group; the few exceptions (3 out of 130 in the sample) were marriages with True Bedouin who belonged to a neighboring group of tribes but had settled in Zullam territory (124/107). The Zullam "consider themselves as inhabiting a common territory" (123/106; cf. 61–2/57), and "pasture is free to all members of the tribal group, and may even be made accessible to other groups of tribes" (88/79).[21]

"The tribe," writes Marx (63/58) "is not a landholding group and its boundaries are not as clear-cut as those of the confederation,[22] but in general its constituent groups hold farming land in roughly contiguous areas, and exploit the same pastures." He believes that in earlier times "each tribe's farm land constitut[ed] a separate sub-territory. Through sale and transfer of farm land, the tribal boundaries of the three large tribes became somewhat blurred" (62/57; cf. 136/124, and the map on 135/123). Each tribe had a leader, its sheikh, and Marx tells us that it is "the widest political association of Bedouin today" (62/58). The Israeli Military Administration functioned in a way that strengthened the tribal sheikhs (41–6/41–5), and the same was evidently true of the Mandatory regime. But it would be wrong to conclude from this that the tribal sheikhs of the Zullam are a creation of twentieth century governments. The sources from the early twentieth century all agree that each of the four tribes had a sheikh, while the Zullam as a whole did not. There can be no doubt that the tribe was at that time an important political unit, and Marx indeed refers to the tribes as "the groups which formerly waged war" (62/58).

Nothing in Marx's material suggests that the sub-tribes were territorial groups, but mention must be made here of Randolph's description of the Qderat, a tribal group very similar to the Zullam. He states several times that the "named corporate descent group," an entity corresponding roughly to Marx's sub-tribe,[23] is the territorial group.[24] Randolph offers no evidence in support of this assertion, but it might be true of the Qderat without being true of the Zullam.

No clear conclusions can be reached from these data. If territorial rights were vested in just one of the tribal units, then that unit may have been the Zullam as a whole. The fact that the arable of each of its constituent tribes formed a fairly well-defined bloc is in no way incompatible with this; among the Ahaywat the arable of each constituent clan forms such a bloc.[25] On the other hand, the Zullam as a group differ from the Ahaywat in that the Zullam do not have a permanent leader, and there is no evidence that they

---

[21] Similarly 91/80, though here the Hebrew translation has "confederation" instead of "group of tribes." This is obviously a mistake.

[22] Here and elsewhere on the same page Marx writes "confederation" when he evidently means to refer to what he usually calls a "tribal group" or a "group of tribes."

[23] More precisely, Randolph's term refers to what I call a sub-clan, on which see below, sec. III.1.

[24] Randolph 1963: 116 (it "owns land as unit"), 118 ("Members of corporate named descent groups own grazing land"), 136 ("Members hold a territory"), 137 ("the named descent group, which holds a territory").

[25] As will become clear in what follows, what I call the clan and what Marx calls the tribe are closely related.

had such a leader at some earlier period. Without some kind of representative it would not be possible for the group to act as a corporation in its relations with neighboring territorial groups. Possibly some kind of ad hoc representative, or representative body, was established when the need arose.[26]

It is also possible that territorial rights were not vested in a single tribal unit. If so, then they may have been in some way divided between the tribal group and its constituent tribes.

II.6

The territorial groups in the Negev, whatever they were, obviously consisted only of True Bedouin. So whether we say that territorial rights among the Zullam were vested in the tribal group, or in the tribe, or were distributed between them, this much at least is clear: we are using the terms "tribal group," "tribe," and by implication "sub-tribe," in a sense that excludes the Fallahin and Blacks. And, unless otherwise noted, it is in this sense that I shall use these terms in what follows.

In contrast to this view, Marx agreed with the Military Administration in viewing *all* Bedouin as belonging to one or other of the tribes. For any given tribe there might, however, be disagreement between Marx and the Military Administration as to who these members were. This is because the Military Administration determined an individual's affiliation according to the tribal name entered on the individual's identity card, whereas Marx often determined an individual's affiliation according to which tribe the Bedouin themselves considered that individual to be associated with (68, 78/ 62, 70).

II.7

There is an interesting exception to the rule that the Fallahin were not organized in tribes. As was mentioned in n. 7 above, the Abu al-Qiʻan enjoyed the official status of a tribe, and at the beginning of the twenty-first century its members were trying to present themselves as True Bedouin descended from the ʻAyaydah tribe.[27] I do not know whether they claimed a territory of their own in the Negev. Abu al-Qiʻan have a reputation for courage and daring that extends well beyond the Negev.[28] If they have succeeded in organizing themselves as a Bedouin tribe (in addition to being officially recognized as one), then they will not be the first people of peasant origin to have done

---

[26] See Stewart 2006: 251 for an example of a North African Bedouin tribe with arrangements of this sort.

[27] Personal communication from Yosef Ben-David in April 2007.

[28] I myself heard of their reputation in central Sinai.

so.[29] Another Fallah group also attempted to follow this course, but without success (121, 144–5/105, 135). In the days when governments had little or no authority in the Negev, a Fallah group, if it had succeeded in setting itself up as a tribe, would no doubt at the same time have acquired territorial rights, either by joining an existing territorial group, or simply by seizing a territory.

II.8

Marx writes at some length about a contrast, which seemed to him paradoxical, between the behavior of the landed True Bedouin and that of the Fallahin. The landed Bedouin owned enough land to meet their needs, yet instead of devoting themselves primarily to agriculture, invested much energy in stock-raising; the Fallahin, in contrast, had to lease most of the land that they cultivated, and yet devoted themselves primarily to agriculture—they had only about half as many stock per head as the True Bedouin (94/83). Marx suggests that this apparently irrational behavior was (at least in part) the result of "diverging cultural orientations, deeply rooted in their specific historical backgrounds" (94/83).

I am not in a position either to confirm or to dispute Marx's explanation, but I should like to point out what I believe to be a false assumption in his argument, namely that "anybody can dig a cistern" in the pasture areas (98/86). The pasture areas were, as I have indicated, controlled by True Bedouin groups to which the Fallahin did not belong; and one of the rights in the territory that the territorial group reserves to itself is the right to dig cisterns. This was noted by Shmueli (1970: 46) with reference to the Judaean Desert Bedouin, and while I was in Sinai an armed conflict almost broke out between two territorial groups because a member of one had begun digging a cistern in territory claimed by the other.

Now in order to maintain a substantial herd in the Bedouin reservation, it was necessary to have access both to the pasture land in the east of the reservation (and beyond it), and to a cistern in that pasture land (22, 88–9/26, 79). A man who did not own such a cistern, and did not have the right to dig one, acquired access to a cistern only by permission of its owner. Such permission was given, no doubt, to close agnates, and it was also given to relatives by marriage. Bedouin from other groups were therefore careful to marry into the groups whose members had cisterns in the pasture areas (134/122). Now a Fallah could not be an agnate of a Bedouin, and, as will be remembered, there was also virtually no intermarriage.

Forbidden to dig a cistern and unable to gain access to one by marriage, how could a Fallah maintain a herd in the pasture area? It may be that Bedouin would have been prepared to sell water from their cisterns, so perhaps the possibility of raising

---

[29] The Huwetat tribe, for instance, which is represented in both Transjordan and the Sinai, seems to be of peasant origin (Oppenheim 1939–68, vol. 2: 291). The Zullam nevertheless accept them as True Bedouin (n. 63 below).

stock on a large scale was not completely closed to the Fallahin; but at any rate, Marx underrated the difficulties facing a Fallah with ambitions of this kind.

Table 17.1 Population of the Abu Gwe'id tribe and its attached groups in mid-1961

Sub-tribe name	Core-group	Fallahin	Other attached	Total
Abu Gwe'id-Rahahlah	145	404	90	639
Sarai'ah	123	55	21	199
Abu Msa'ad	37	75	0	112
Ma'abdah	133	0	57	190
Totals	438	534	168	1,140

Source: Tables 8 and 9 of *Bedouin of the Negev*. The figure for the Abu Msa'ad core-group is given wrongly in the English version of Table 9, but is corrected in the Hebrew.

III. Descent Groups

In order to analyze the social structure of the Negev Bedouin we need to distinguish sharply between descent groups and what Marx calls co-liable groups.

It will be best to begin with a rough definition of a *(patrilineal) descent group*. For present purposes we can say that it is a group[30] in which membership is acquired by being the child (male or female) of a man who is himself a member; and that the members of the group believe (a) that they are all descended in male line from a single male ancestor and (b) that they constitute all the descendants in male line of that ancestor (the *apical ancestor* of the group).

In this section I shall deal only with the formal characteristics of the descent groups. Their functions will be discussed later (section IV.5).

III.1. True Bedouin

Among the True Bedouin I shall distinguish descent groups at four different levels: sib, clan, sub-clan and lineage. None of these terms is used by Marx, but I shall explain how these terms are related to the ones used by him.

Sib I shall refer to a descent group that is at the center of a tribal group as a sib, and to the constituent descent groups of the sib as clans. Each of the four Zullam

---

[30] I shall not here attempt to define the notion of a social group, but will refer the reader to the profound and rigorous analysis of this concept that has been developed by Margaret Gilbert. See Gilbert 2006: 91–181, which also gives references to her earlier publications.
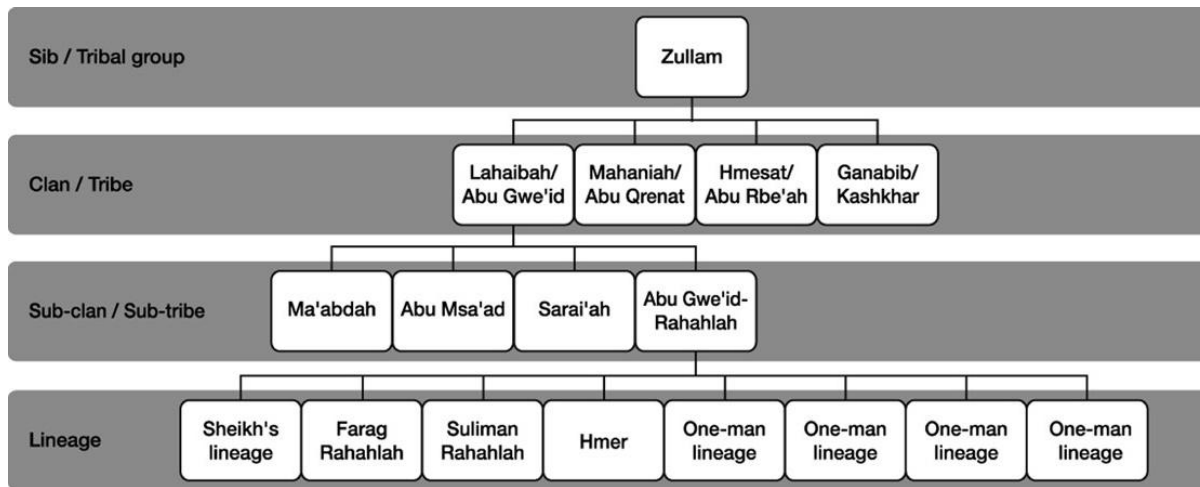
Figure 17.1 Zullam descent groups

Source: *Bedouin of the Negev*, pp. 66, 123/60, 107 and the references given in note 48.

tribes is centered round a particular clan. The Zullam all agree that these four clans originated from the Bili tribe.[31] There is, however, disagreement among the Zullam about the genealogy that links the four Zullam clans to each other. Some versions represent the Zullam clans as constituting a single descent group,[32] but others assert that the Abu Gwe'id clan, though also of Bili origin, was linked to the other three clans by the marriage of a daughter of the Abu Gwe'id apical ancestor either to Zalem (the apical ancestor of the other three Zullam tribes) or to a descendant of his.[33] In this version, then, the sib does not include Abu Gwe'id.

It seems likely the Negev Bedouin did not attach great importance to the exact nature of the relationship between the apical ancestors of the central descent groups of a tribal group, though it may be that they always assumed that the ancestors of these groups were in some way kin to each other. The Qderat tribal group resembled the Zullam in that it consisted of four tribes, each centered on a particular clan. Here too there are differing versions of the relationship between the apical ancestors of the four clans; one ethnographer says indeed that on this subject there existed "a state of utter confusion."[34]

---

[31] Bili (Baliyy in literary Arabic) is a tribe whose history can be traced back to before Islam. Those of the tribe who remain in Arabia are to be found in the Hijaz (Oppenheim 1939–68, vol. 2: 352–7).

[32] Musil 1907–08, vol. 3: 44; al-'Arif 1934: 125 ("The Zullam are all the offspring of one man", sc. Zalim), 126 n. ("Some say that Muhanna [son of Zalim] is the ancestor of all the Zullam").

[33] Al-'Arif 1934: 125 (it is noteworthy that the informant whom he quotes begins his account by saying that all the Zullam are the offspring of Zalim); Ben-David 2004: 81; Mintzker, personal communication; and the same seems to be implied by the Abu Rbe'ah version of the Zullam genealogy given by Marx (123/106).

[34] Randolph 1963: 130. Unfortunately he does not give much detail about this confusion. Cf. Musil 1907–08, vol. 3: 38–9; al-'Arif 1934: 121–5; Oppenheim 1939–68, vol. 2: 116–17.

Clan I shall use this term to refer to a large descent group, and particularly (though not only) the descent group that constitutes the main body of a tribe. It appears that the Bedouin view each of the four founding fathers of the Zullam as the apical ancestor of such a descent group.[35] As will become clear in what follows, the tribe may include some people who are not members of the clan,[36] and there may be members of the clan who are not members of the tribe.[37]

The names that Marx uses for the four Zullam tribes—Abu Gwe'id, Abu Qrenat, Abu Rbe'ah and Kashkhar—are also to be found in sources from the first part of the twentieth century. They are based on the names of the sheikhs of these tribes. Thus the leadership of the Abu Gwe'id tribe has for many generations been in the hands of a group whose members use the teknonym Abu Gwe'id 'father of Gwe'id', and the tribe is commonly referred to as *'ashirat Abu Gwe'id* 'the tribe of (sheikh) Abu Gwe'id'.[38] But Marx tells us that "the Zullam among themselves" used different names for the tribes, which he also gives: Lahaibah, Mahaniah,[39] Hmesat and Ganabib (123/107). These too are recorded in the earlier literature, and they are names of a kind commonly used among the Bedouin to refer to descent groups: each one is the plural form of the name of an ancestor. Thus the Lahaibah (i.e., Abu Gwe'id) are the descendants of Lahaib,[40] as one might say "the Lahaib people." Marx says firmly that these names are used "to refer to the whole of a tribe," but I suspect that they refer specifically to the clans. On this view, the name Lahaibah, for instance, refers to what I shall call here the Abu Gwe'id clan rather than to the Abu Gwe'id tribe. If this guess is correct, it is a further indication of the need to distinguish between clan and tribe among the Zullam (see also section V.3 of this chapter).

Marx does not give an exact membership figure for any clan, but it can be inferred that Abu Gwe'id numbered a bit less than four hundred souls.[41] Other information that he provides suggests that the Negev clans differed considerably in size, and that Abu Gwe'id was somewhat larger than average.[42]

---

[35] This seems to follow from the rather obscure statement at 124/107, and is in any case implied throughout Marx's book, and indeed in other publications on the subjects.

[36] See, for an example, n. 63 below.

[37] This would be the case if a member of the clan had left the co-liable group associated with that clan and joined a co-liable group belonging to a different tribe. (Co-liable groups are dealt with in section IV of this chapter.)

[38] Personal communication from Uri Mintzker. Cf. 59/59.

[39] A more plausible form, Mahaynah, appears in al-'Arif 1934: 126 et passim. The singular is Muhanna.

[40] This is the form used by Marx; the corresponding literary form must be Luhayb, pronounced Lheb in the local dialect.

[41] Marx gives 438 as the total for the four core-groups in the Abu Gwe'id tribe (66, 75/60, 67; and see Table 17.1 of this chapter). Two of these four core-groups (Abu Gwe'idRahahlah and the Ma'abdah) contained some people who were not members of the Abu Gwe'id clan (see nn. 61 and 63 below). We can therefore infer that the size of the clan was less than 438 souls. See further section V.5 below.

[42] See Marx's Table 2 (13/19). The Hebrew version of the table, in contrast to the English one, does not give the names of the sheikhs, but it makes good an omission in the English text: there should

Sub-clan According to Marx's account, each of the three large Zullam tribes was in turn divided into four smaller groups that he calls sub-tribes.[43] Each subtribe is centered on a descent group that I shall refer to as a sub-clan.[44] Often what I call a sub-clan is the same as what Marx calls a core-group; but sometimes the entity Marx calls a core-group includes some people who are not members of the sub-clan (nn. 61 and 63 of this chapter). I shall also use the term sub-clan to refer to other descent groups that are of roughly the same size as the groups that Marx calls core-groups, even when such descent groups are not associated with a sub-tribe. The four sub-clans of the Abu Gwe'id clan (listed in Figure 17.1 of this chapter) ranged in size from 37 to something approaching 145 members (66/60, and cf. Table 17.1 of this chapter).

Lineage I shall use this term to refer to a descent group within a sub-clan, or to a very small descent group that is not in a sub-clan. Marx refers to such a descent group as a *section*, though as he himself notes, and as we shall see soon, he also uses the term section in another sense.

Marx says that a lineage should contain "an appreciable number of adult males" (114/100), but he records a number of lineages with only one or two men in them.[45] In fact the lineages of which he gives details range in size from one to 17, or perhaps 19, adult men.[46] Multiplying the number of adult men by four will give a rough idea of the total number of members of a lineage.[47]

The Abu Gwe'id-Rahahlah sub-clan had (at least) eight lineages,[48] as may be seen from Figure 17.1 of this chapter. Another of the True Bedouin sub-clans had five lineages (200/165).

---

be four Tarabin tribal groups, and not three (as in the English version). The fourth one is the Naba'at, which consists of the Abu Blal tribe, whose population is 260.

[43] Marx remarks that the fact that each of the three large Zullam tribes is divided "into four sub-tribes is probably pure coincidence, and the number of sub-tribes in other tribes varies" (65/59–60.). What he says is confirmed by the fact that by the beginning of the twenty-first century the number of sub-tribes in Abu Rbe'ah was only three (Ben-David 2004: 91, 93).

[44] Marx writes that each of the four core-groups of the Abu Gwe'id-Rahahlah subtribe consists of people "who claim direct descent in the male line from the ancestor of the tribe" (65/60). I have generalized on the basis of this statement.

[45] The lineage mentioned at 129/112 as having one man in it is mentioned as having two men in it at 182/151. Other one and two man lineages are mentioned on 130/112.

[46] Marx refers to this group as having seventeen men (214/180), but the accompanying genealogy shows nineteen adult males (215/181).

[47] I suggest four on the basis of the data given for the Sarai'ah sub-clan, which had 123 members (66, 75/60, 67), of whom 33 were adult men (200/165); the Abu Gwe'idRahahlah sub-clan, which had 145 members (66, 75/60, 67), of whom 36 were adult men (236/198); and a Fallah group with about 110 members (114/99) of whom 22 were men (208/172).

[48] They are: 1. The sheikh's lineage (128, 152, 214/111, 114, 180). 2. and 3. The Farag Rahahlah and Suliman Rahahlah lineages (152/114, where the Hebrew is abridged). 4. Hmer (130/112). 5. through 8. Four lineages, each containing only one man; only one of these men was married, his wife was not a member of his lineage, and it is not stated whether he had children (130/112; in the English text, for "four members of the co-liable groups" read "four members of the co-liable group").

The Bedouin had no clear ideas about the number of generations that separated the apical ancestors of the various sibs, clans and sub-clans from the living members of these groups.[49] They did, however, often have information of this kind with regard to the lineage (see section IV.6 of this chapter).

III.2. Fallahin

The Fallahin (except for the Abu al-Qi'an) were not organized in tribes, but they had sub-clans and lineages much like those of the True Bedouin. One sub-clan contained 98 adults, of whom 39 were men, belonging to four lineages that ranged in size from 5 to 17 men (77, 202/69, 166–7); another sub-clan is recorded as having about 110 souls (114/99), of which 44 were adults (77/69), 22 of these being men (208/172); a third is said to number 50 tents (72/65), but to have only 23 adult members (77/69)—clearly something is amiss here (I would guess the number of adults to be around three times the number of tents); a fourth had 62 adults (77/69); a fifth had 30 tents (72/65).

IV. Co-liable Groups

IV.1

The *co-liable group*[50] is the most characteristic feature of Bedouin social organization; it is probably universal among the Asiatic Bedouin, and it is at least widespread among the African ones. It consists of a group of adult men, usually each other's closest agnates, who are bound to co-operate in certain legalpolitical matters. The central commitment is that they pay blood-money together, and distribute any blood-money received by one member to all the members. The outside world looks on the group as a unit—for example, if a member of the group commits a blood offense, then the group as a whole is liable for blood-money. It is for this reason that Marx uses the term "co-liable group."

Women and children belong to the descent group, but not to the co-liable group; and just as the co-liable group defends the property of its members, so also it defends their dependents. A man's *dependents*, in the sense in which I am using the term, are his minor children and those adult females for whom he has primary legal responsibility. These will, generally speaking, include at least his daughters and sisters, whether or not they are married.[51] They will not, generally speaking, include his wife.

---

[49] "Bedouin fully realize that when they discuss the genealogical positions of persons not contemporaneous with themselves, their information becomes imprecise" (205/169).

[50] This useful term was invented by Marx. For further details about co-liable groups, see Stewart 2006: 253–7, 2010.

[51] It will be evident that the term *dependent*, as used here, does not always imply economic dependence. Thus a man is legally responsible for his married sister, though as long as she is living with her husband he is not economically responsible for her. See Stewart 1991b.

The True Bedouin co-liable groups in the Negev generally have a chief,[52] who acts as its representatives in dealing with the outside world; but the leadership of a Fallah co-liable group tends to be weak or non-existent (201, 203, 206/166, 168, 171).

IV.2

As Marx emphasizes—and this is entirely in accordance with my own material from Sinai—the True Bedouin co-liable group, if it is of any considerable size, has a well-marked internal structure: it is made up of a number of what I shall call *co-liable sub-groups*, each with its own leader (or leaders) and its own corporate existence (191, 201/157–8, 166 et passim). Marx notes that among the Fallahin, in contrast, co-liable groups usually lack a clear internal structure of this kind (125, 177, 191, 201/108, 146, 158, 166).

The word that Marx uses in order to refer to a co-liable sub-group is *section*, a term that in his usage has to bear a particularly heavy burden of different meanings. Sometimes a section is a whole co-liable group,[53] and as was mentioned above, he uses the same word to refer to a fairly small descent group of the kind I have called a lineage (114, 185/99, 153). On occasion Marx explicitly or implicitly makes a distinction between the section as a lineage, and the section as a co-liable group or a part thereof. Thus he remarks that "a section in the political field is not always completely identical with the domestic group of that name" (177/146), and that "only men become members of sections (of the *qom* and the *khams*[54]) in the political sense" (186/153). Evidently a "section in the political sense," also referred to as a "corporate section,"[55] is a co-liable group or a part thereof, as opposed to a "domestic" section that includes women and children (114/99–100).

IV.3

Marx's material leaves no doubt that the Negev Bedouin distinguished between the descent group and the co-liable group, but he never quite makes this fact explicit, and indeed uses a terminology that obscures it. Let us see what he has to say about three

---

[52] In the Negev, as in Sinai, he is referred to as a *kibir* "elder," and just how commonly (and in what contexts) the term is used may be seen from Stewart 1988–90, vol. 2: 240. A group may have more than one elder. In the Hebrew translation of his book Marx uses the word *zaken* "elder" where the English version has "chief."

[53] "A section may sometimes constitute a co-liable group for its members, but in other cases several sections share in co-liability" (115/100), and "in some cases a co-liable group is composed of a single section, but often it comprises several sections" (177/146; similarly 160/194).

[54] The word that the Bedouin use is actually *khamsih* (Stewart 2010). Marx (194/160) quotes al-ʿArif 1933: 78 as using the term *khams*; but in fact of the three places where the word *khams* appears in Marx's translation, al-ʿArif uses the word *khamsah* in the first, and *damawiyyah* (the Bedouin word for co-liable group) in the second and third.

[55] 7; the expression does not appear in the Hebrew translation.

cardinal features of the co-liable group: that it contains only adult men, that there is an agreement between its members, and that it closely linked to a particular descent group.

Marx was aware—as one cannot fail to be—that all public legal and political activity among the Bedouin is carried on by men alone, but he does not explain the way in which this finds clear formal expression in the distinction between the co-liable group and its associated descent group. When Marx gives figures for the membership of various co-liable groups those figures include men, women, and children (e.g., 66/60); yet his first extensive discussion of the co-liable group (63–4/58–9), though it contains no outright statement to the effect that the group includes only men, nevertheless offers a number of strong hints to that effect. These hints become even more clear later in the book[56] and in its Hebrew translation.[57]

Marx notes correctly that "the obligations incumbent on individuals as members of an association are usually precisely and jurally defined" (183/151). What Marx does not state is that the method used to define the obligations of the members of the co-liable group to each other is a contract between the members.[58] But his general account of the obligations themselves is accurate, and in particular he notes that there is a distinction between what might be called the *blood-money group* and the *vengeance group* (Stewart 2006: 257). The former consists of those who share "in payment and receipt of blood-money," while the latter are those "who are exposed to vengeance or have to exact it" (193/159). The vengeance group consists, at least in theory, of the co-liable sub-group of the victim or perpetrator, while the blood-money group is the whole of his co-liable group (including, of course, the vengeance group).[59]

Marx writes at one point that "membership of the ... co-liable group is determined by the principle of descent" (185/153). But he himself provides evidence (quite apart from his hints that only men belong to the co-liable group) that this statement is not strictly correct. Thus he notes that a man may be expelled from his co-liable group (171, 199, 240/145, 164–5, 202), or join one other than the one to which he belongs by birth (64, 73, 130, 186/59, 65, 113, 153), and that the co-liable group is not composed

---

[56] See, for instance, the remark about the membership of the *qom* and the *khams* from 186/153 that is quoted at the end of section IV.2 above.

[57] The translation says of the members of a group that "their sons become full-fledged members of their father's group" (59). This statement is correct, in contrast to the original English (64), which has the words "their children" in place of "their sons."

[58] Occasionally a co-liable group may consist of a small group of very close agnates, e.g., three brothers. In such cases there will not normally be a formal agreement between the members.

[59] , 183/150, 151. Marx adds that "vengeance is still the responsibility of all members of the co-liable group, in the sense that failing a close agnate any other member would hold himself responsible" (195/161).

exclusively of agnates (73, 129–30/65, 112–3).[60] None of these things could be so if membership were determined exclusively by descent.

Taken all in all, Marx's account of the co-liable group and of the section gives the impression that he has sensed much of the truth, even though he has not brought it clearly into the light.

IV.4

Having clarified, as far as I can, Marx's view of the relationship between descent group and co-liable group, let me now deal with the relationship itself in slightly more detail. Broadly speaking, we may say that the sub-clan is the descent group associated with a co-liable group, while the lineage is the descent group associated with a co-liable sub-group. If we want to be precise, however, we must add a number of modifications.

The first concerns the Fallahin: as will be evident from what was said in section IV.2 above, their lineages do not give rise to clear co-liable sub-groups within the co-liable group.

Second, a sub-clan among the True Bedouin may give rise, not to a single co-liable group, but to a number of such groups. An example among the landed tribes is found in the Ma'abdah sub-clan of the Abu Gwe'id tribe: a total of about a hundred souls produces no less than five co-liable groups (71, 154/64, 114).[61] This suggests that the average number of men in each of these co-liable groups was around five or six, though there was a wide range of variation: one of the groups only had three men in it (153/114) while another (the Far'aunah) had nineteen (245/225). It appears that four of the five co-liable groups were each made up of men of a single lineage, but that the fifth one—the largest, the one with nineteen men—was made up of men from two lineages. I take it that in this case the men of each of the two lineages constituted a co-liable sub-group within the group (154/114).

Third, there are True Bedouin descent groups which give rise to co-liable groups, but which are so small that it is appropriate to call them lineages rather than sub-clans. Such are the Zagharna and the Zbedi each of which has only five adult men (245/225, cf. 71/64).

Fourth, the fact that men may leave the co-liable group associated with their descent group and join a different co-liable group means that the mapping from descent groups

---

[60] In a sentence that is omitted from the Hebrew translation, Marx writes, quite correctly, that "Even when a man is allowed to join such a co-liable group, he does not partake of agnation" (73). As Marx suggests elsewhere (171/145), if a man does not belong to the co-liable group associated with his own descent group, there arises a distinct tension between his continuing status as an agnate and the fact that he no longer belongs to his original co-liable group.

[61] The Ma'abdah core-group numbered 133 souls (66/60), and consisted of six coliable groups (on p. 71 of the English version Marx says "at least six," but in the Hebrew, as elsewhere in the English, he writes simply of six). The men of one of these groups belong to the Abu Wadi lineage, which is not part of the Ma'abdah sub-clan (73–4/66). They numbered seven men (245/255), from which one can infer perhaps about thirty members in all (see n. 48 above).

to co-liable groups is not always precise. So, for instance, the Abu Gwe'id-Rahahlah co-liable group consisted of eleven sub-groups; eight of these were members of the Abu Gwe'id-Rahahlah sub-clan,[62] and three were not.[63]

IV.5

At this point something should be said about the nature and functions of descent groups among the True Bedouin in general. The remarks that follow probably apply not only to the Sinai and the Negev but also much more widely among the Asiatic Bedouin.

The importance of the descent group arises from the significance that the Bedouin attach to agnatic proximity. Quite apart from the deep loyalties to which it may give rise, such proximity is also important in the law, for instance in the contexts of succession and pre-emption.

A man therefore needs to know how close he is agnatically to others. Now a Bedouin can say exactly how he is related to his closer agnates, but he will not necessarily be able to trace his precise relationship to every other person whom he recognizes as an agnate. He will, however, generally be able to tell you, if you name any two descent groups that he recognizes as consisting of his agnates, which of them is agnatically closer to his own group (unless they are equally close). Descent groups are thus, among other things, a convenient way of categorizing people.

We can take any male ancestor and refer to his descendants in male line as constituting a descent group. What we find in practice is that the Bedouin pick out certain ancestors, frequently referring to their descendants as a group, while neglecting other ancestors, and rarely referring to their descendants as a group. The question of why one ancestor is picked out among others cannot usually be answered. But if we look at a group formed by the living descendants of one of these *significant (apical) ancestors*, we will find that its members have a sense of mutual solidarity, though the strength of that solidarity may vary greatly from group to group, from person to person within a group, and from time to time.

If the descent group's solidarity finds expression in some kind of joint political action, then of course it is only the men of the group who will take an active part in planning and executing the action. When such a group exists, other Bedouin will look for someone who is authorized to act as its representative. If such a man exists, then

---

[62] See n. 48 above.

[63] They were: 1. The Halaileh, a large sub-group originating from the Beni 'Atiah tribe, and which joined the Abu Gwe'id-Rahahlah co-liable group a generation ago (130/112). 2. Another sub-group originating from the Beni 'Atiah; this sub-group had only one (129/112) or two (73, 182/66, 151) members; apparently it was formed out of a lineage all of whose members were descended from one man, the father's father of the one (?) surviving adult male (129/112). 3. A sub-group originating from the Huwetat tribe. It consisted of two men whose father's father had joined the Abu Gwe'id-Rahahlah co-liable group (130/112).

the group is well on the way to becoming a corporation. I take the corporation to be a legal concept (cf. section II.3 of this chapter). Now in Bedouin law only men have full legal personality,[64] and only men can be the members of a corporation. Women and children will be affected if they are dependents of members, but they are not themselves members. There is no harm in referring to a man as being the leader of a descent group as long as we bear the legal position of women and children in mind.

The corporation tends to be relatively long-lasting. It may take the form of a co-liable group, or it may (like the core-group of the Ma'abdah) be made up of coliable groups without itself being one. Once a political group is born in this way, it is no longer necessarily limited to members of the original descent group, for the highly developed contract law of the Bedouin makes it possible also for outsiders to join. As one would expect in a nomadic society with no central authority, the political structures of the Bedouin are flexible to a degree.

A large political group among the Bedouin will be made up of a number of smaller units. If the large group has a leader, then he will also be the leader of successively smaller groups within the large group. Thus the leader of the Abu Gwe'id tribe is also the leader of one of its constituent sub-tribes (Abu Gwe'idRahahlah), and of one of the constituent lineages of the latter, which I have followed Marx in referring to as the sheikh's lineage (see Figure 17.1 of this chapter). We can also say that the sheikh of the Abu Gwe'id tribe is leader of the Lahaibah clan, of the Abu Gwe'id-Rahahlah co-liable group and sub-tribe (which are both centered on the Abu Gwe'id-Rahahlah sub-clan, though both also include outsiders), and of the sheikh's sub-group of that co-liable group (which is made up exclusively of men from the sheikh's lineage).

IV.6

Marx offers a great deal of genealogical material in his book, and though it is rather unsystematic, it allows us to gain a general impression of how the genealogies of the various descent groups are related to the political structure. I shall deal with the descent groups in descending order.

The Zullam sib has already been discussed (section III.1 of this chapter). Marx nowhere gives details of the genealogy of a clan, but going down another level, we can find a discussion of the genealogy of a True Bedouin sub-clan, the Sarai'ah, one of the four that make up the Abu Gwe'id clan.[65] He tells us that this genealogy is "typical of the larger Bedouin groups" (186/154). The information comes from a single informant, the head of the corresponding co-liable group, and the genealogy is only a partial

---

[64] Infant males have limited legal personality. For instance, they own property, even if they do not manage it. Women, at least in Sinai and the Negev, do not have legal personality.

[65] 186–8/154–5; at 200/165 there is a representation of all the sub-groups in the relevant co-liable group, and at 126/110 the genealogy of one of the lineages. The co-liable sub-group is said to contain eight men, while the genealogy shows eleven living males; presumably three of them (E5, E7 and E8) were children.

one—it includes the apical ancestors only of three of the five lineages in the sub-clan. Even for these three, the informant makes no claim to have produced a full agnatic genealogy: he is uncertain about some of the names, and he is well aware that he has left out a number of generations in his sketch. The genealogy, in fact, conveys only two important points: that the apical ancestors of the three lineages are all descendants in male line of one man, and that the apical ancestors of two of the lineages are agnatically closer to each other than either is to the apical ancestor of the third one.

Marx also tells us something about the genealogy of the Abu Gwe'id-Rahahlah sub-clan (236/198), but the information is in various ways problematic.[66]

As was noted above (section III.1), it is only at the level of the lineage that the Zullam have detailed genealogies—and not always then. Most of members of the lineage "are connected through an ancestor one or two generations removed," but the lineage "in some cases also contains more distantly related agnates" (114/100). Much the same is stated elsewhere (220/184–5); and the genealogy of one large lineage (seventeen or nineteen men) shows their apical ancestor to be the father of their (very old) oldest living member. In contrast, however, the genealogies of three other lineages of a single sub-clan (having eight, nine and ten adult males respectively) show the apical ancestor of each one to be not less than three generations above the oldest living generation—so far back, in fact, that the exact distance is no longer exactly known (187, 190, 200/154, 157, 165).

In sum, we can say that except as regards the recent past, the Zullam remember little more than their significant ancestors and the relative agnatic proximity of those ancestors to each other.[67] A significant ancestor, in the Negev, is one around whose descendants a political group has formed. It is because of these facts that I have defined the various types of descent group (sib, clan, sub-clan, lineage) mainly in terms of the type of political group to which they are connected. It is not profitable among the Bedouin, as it may be in some other societies, to define descent groups in terms of generational depth, and probably not even possible. With regard to the two lowest levels (sub-clan and lineage) I have also introduced the criterion of size, but only in a secondary fashion. The smallest Abu Gwe'id sub-clan (Abu Msa'ad) has thirty-seven members, which is almost certainly less than the number of members of the sheikh's lineage of the Abu Gwe'id-Rahahla which has seventeen (or nineteen) adult men in it. Despite this, I call the former a sub-clan because its menfolk constitute a sub-tribe (and a co-liable group), and the latter a lineage because its menfolk constitute only a sub-group of a co-liable group. Generally, any True Bedouin descent group which is at

---

[66] In contrast to what Marx found among the Zullam, Randolph (1963: 116–7.) writes of the Qderat that "Within the named descent group [i.e., the sub-clan] each individual can trace patrilineal connections in known ways to the common ancestor and to all other individuals in the group."

[67] Randolph (1963: 127–30) agrees that detailed genealogies do not stretch further back than the recent past, and also takes the view that "In the Negev all temporally remote levels of the genealogies are also ambiguous." By this he means that there is considerable disagreement about the agnatic proximity of remoter ancestors to each other.

the center of a coliable group that has sub-groups is referred to as a sub-clan. I have only applied the criterion of size where the descent groups in question are not part of the Zullam sib (including under that term the Abu Gwe'id clan), and particularly in the case of the Fallahin descent groups.

V. The Tribal Units

V.1

I have suggested that the social structure of the Negev Bedouin should be analyzed in terms of four types of group.

(i.) The groups recognized by the authorities (section II.2 above). Because these do not in themselves present conceptual problems, I have only mentioned them briefly here, though they were of great practical importance to the Bedouin in the early 1960s. Marx deals fully with this subject in the second chapter of his book.

(ii.) The tribal units, i.e., the confederation, tribal group, tribe, and sub-tribe. The confederation I have ignored, since it was evidently of little or no importance to the Zullam in the 1960s. At least one of the other three units was the territorial group. If the tribal group was the territorial group, or even if it merely enjoyed certain territorial rights, then it was by definition corporate, though it had no single leader and we do not know how it was represented in its dealings with the others. Tribe and sub-tribe both have leaders, both are politically significant units, and both are corporate.

(iii.) The co-liable groups, which are corporations that include only men. Each tribe contains a number of co-liable groups, but there are also co-liable groups (notably those of the Fallahin) that do not belong to any tribe.

(iv.) The descent groups, i.e., the sib, clan, sub-clan and lineage. They include men, women and children, and are not corporations. The central descent group of a large co-liable group is a sub-clan, and the central descent groups of its constituent co-liable sub-groups (if any) are lineages. The central descent group of a small co-liable group is a lineage.

The relationship between the descent groups and the co-liable groups was, I hope, made clear in section IV. In this section I shall try to clarify the relationship between these two types of group and the tribal units.

V.2

The co-liable groups are units from which larger political structures can be built. One possibility is to merge existing co-liable groups, or to incorporate into an existing co-liable group individual men or small groups of agnates who have left their original groups.[68] The Abu Gwe'id-Rahahlah co-liable group mentioned in section IV.4 was

---

[68] For technical details of the various ways in which this can be done, see Stewart 2003: 256–60.

no doubt created in this fashion: the men of the Abu Gwe'idRahahlah clan always constituted a single co-liable group (236/198), but the other three sub-groups in the group must have been incorporated into the Abu Gwe'idRahahlah co-liable group.

Marx's material, which has been confirmed by subsequent research, shows that there are also other ways in which co-liable groups can be united into a single structure. These involve what Marx calls a *core-group*. Marx's definition of this term is unsatisfactory,[69] but we can see what he means by the term from the detailed information he provides about the four core-groups of the Abu Gwe'id tribe.

In Marx's view the *core-group* (all of whose members are True Bedouin) is one of the two main components of the *sub-tribe*, the other being any *attached groups* that it may have.[70] Now my view, as will be remembered, is that a sub-tribe consists only of True Bedouin. This means that what I call a sub-tribe is what Marx calls a core-group, so that in my analysis the term core-group is superfluous. In what follows I shall therefore refer to the attached groups as being attached to a sub-tribe, not to a core-group.

V.3

Three of the four Zullam core-groups, in Marx's account, consist each of a single co-liable group. If the fourth were also like this, then we might doubt the need for the concept of a sub-tribe (in my sense of the term). We would arguably need to speak only of the co-liable group, or of the co-liable group and its dependent women and children.

In fact, however, the fourth Zullam core-group, the Ma'abdah, is different. The men who belong to the sub-clan of this name are organized in five co-liable groups, and the Ma'abdah core-group also includes a sixth group, which is called the Abu Wadi. They do not belong to the Zullam sib, though they too are of Bili origin. They appear to have joined the Zullam around the year 1900 (74, 154/66, 114). Unlike the outsiders who attached themselves to the Abu Gwe'id-Rahahlah (n. 63 above), the Abu Wadi established their own co-liable group. Marx tells us that they are part of the Ma'abdah core-group, not an attached group.

We have, therefore, a political grouping that consists of the five co-liable groups formed from the Ma'abdah sub-clan, plus the Abu Wadi. This grouping (in my terminology a sub-tribe, in Marx's a core-group), has a chief, who comes from one of the Ma'abdah lineages (154/115). In my usage, then, a *sub-tribe* consists of one or more

---

[69] Marx defines the core-group as "a group of people who form a co-liable group, and who claim direct descent in the male line from the ancestor of the tribe" (65/60). This definition is inconsistent in at least two ways with what Marx himself tells us about the core-groups. First, the core-group may consist not of one but of half-a-dozen co-liable groups (as does that of the Ma'abdah). Second, as we have seen, the co-liable group may contain members who do not claim to be descended from the ancestor of the tribe.

[70] The attached groups, mainly Fallahin, are dealt with in section VI of this chapter.

co-liable groups that recognize one (or several) of their members as the leader (or leaders) of the whole group.

Marx gives some information about the marriage links between the Abu Wadi and other groups of the Abu Gwe'id clan (153–4/114–5), but he offers no details of how the Abu Wadi are tied to the Ma'abdah. Since this tie goes back several generations, it does not seem to be something that was produced merely by the demands of the authorities that the Abu Wadi be registered under the name of a particular existing group or sheikh. We might speculate that the Abu Wadi originally joined the Ma'abdah by virtue of an agreement with the leader of the Ma'abdah.

The remaining five Ma'abdah co-liable groups may simply be tied to each other by the fact that they belong to a single sub-clan and recognize a single leader; in other words, there may be no contractual ties between them.

Given that each of the other three Abu Gwe'id sub-clans gives rise to only a single co-liable group, it might be argued that though we perhaps need the notion of a sub-tribe for the Ma'abdah, we can do without it for these other three groups. There are two reasons for rejecting this line of thought. The first is that the other three groups are viewed by the Zullam as being groups of the same kind as the Ma'abdah. This we know not only from Marx's account, but also from data collected before World War II.[71] The second is that if Abu Wadi could join the Ma'abdah, then presumably other non-Zullam True Bedouin might join any of the other three groups in a similar fashion.

One of the great merits of Marx's work is that it does not schematize the material. The Ma'abdah sub-tribe is presented as different from the other three not only in its formal structure (six co-liable groups rather than one), but also in its behavior. The Ma'abdah chief, for instance, "is completely immersed in his own economic affairs, and does not hurry around from one camp to another as do the other two *ruba'* [i.e., sub-tribal] chiefs" (70/63), and the Ma'abdah sub-tribe, as one would expect, is less likely to unite for political action than are the other three. But these differences do not mean that from a legal point of view it is a group of a different type from the others.

Once we have determined that the notion of a sub-tribe is necessary, then we are forced also to accept the notion of a tribe, since the four sub-tribes are united under a single leader into the *'ashirat Abu Gwe'id* 'the Abu Gwe'id tribe'. By similar logic we must accept the notion of a tribal group. Another reason for bringing in the notion of a tribal group is the version of the Zullam genealogy according to which Abu Gwe'id is not a member of the Zullam sib.

V.4

I stated above that the tribal units (ignoring the confederation) are corporations, and that among the Bedouin, only adult males are members of corporations. Strictly

---

[71] See the convenient table in Oppenheim 1939–68, vol. 2: 130. The Abu Gwe'id tribe is there represented as being made up of four sub-tribes: Abu Gwe'id (the sheikh's group), the Rahahlah, the Sarai'ah and the Ma'abdah.

speaking, it follows from this that only men are members of the tribal units. But this is not, I believe, the way that the Bedouin view the matter. They see these units as including also women and children. The tribal units are, in fact, communities, and as such often have each a distinct character, and attract strong loyalties. Perhaps the clearest evidence that the Zullam constitute a community is the fact, already mentioned, that they are virtually endogamous. The same is true of the Qderat, the only other tribal group whose marriage pattern has been analyzed in detailed (Randolph 1963: 133–4). That the smaller tribal units constitute communities is so evident as to demand no demonstration.

It would be possible to invent for each tribal unit two terms, one referring to the unit as a community, the other referring to it as a corporation. But this hardly seems necessary. The context will usually make clear, if necessary, what is being referred to, and if not, then some additional words of explanation can be added.

V.5

Marx gives detailed population figures for the Abu Gwe'id tribe, which I have summarized in Table 17.1. He does not state whether he collected these figures himself, or whether they are derived from official sources. If the latter, then they are not likely to be reliable, for the reasons indicated in section II.2 above.

Table 17.1 reflects Marx's own conceptual scheme. What he calls the coregroup is what I call the sub-tribe. In his view, the population of the Abu Gwe'id tribe is 1,140 souls.[72] In my view, however, none of the attached groups (534 + 168 = 702 souls) belong to the tribe; this leaves it with a population of only 438.

Even this figure, however, is not quite certain, for it is possible that the criteria Marx used in deciding who the members are of the various core-groups do not coincide precisely with those that I would use in deciding who the members are of the various sub-tribes. (In this case, of course, earlier statements in this chapter to the effect that Marx's core-group is identical with my sub-tribe would need to be modified.) In my view the tribe consists of the men of its constituent co-liable groups plus their dependents (as defined in section IV.1 of this chapter). Marx undoubtedly included in his figure of 438 the men in the co-liable groups, their unmarried children, and women of the Abu Gwe'id tribe who married men of the Abu Gwe'id tribe. All this is in accordance with my own view of who belongs to the tribe. One may ask, however, which other married women Marx counted as members of the tribe, and to this question he provides no answer. A significant proportion of the women whose fathers were members of the Abu Gwe'id tribe were married to men of other tribes, and a significant proportion of the men of the Abu Gwe'id tribe had taken wives whose fathers were

---

[72] Marx's Table 2, which is explicitly based on official sources, gives the population of the Abu Gwe'id tribe as 1,148 in September 1960.

members of other tribes.[73] The Bedouin undoubtedly viewed the first group as members of the Abu Gwe'id tribe, even though they lived with men of other tribes, and the second group as members of other tribes, even though they lived with Abu Gwe'id men.[74] My guess is that Marx did indeed include in his figures members of the first group and not members of the second.[75] If so, then the tribe did have 438 members (assuming that the count was correct). But if, on the other hand, Marx included in his figures members of the second group and not members of the first, then this would be indicative of yet another difference between his view of Bedouin social structure and my own. It would probably not, however, make a great deal of difference to the figure given for the total number of souls in the tribe.

VI. Attached Groups.

VI.1

Marx classifies the *attached groups*[76] according to their origins, and he lists the following types:

Related Bedouin These are landed True Bedouin descent groups, each giving rise to its own co-liable group. They are believed to be more or less closely agnatically related to the sub-tribe.[77] They enjoy considerable independence (71/174).

External Bedouin Again, these are True Bedouin descent groups, each with its own co-liable group.[78] Marx writes that they originate "from outside the tribe" (66/60), but I suspect that this is equally true of the Related Bedouin. Ben-David (2004: 93), in his

---

[73] What proportion we do not know, but Marx discovered that 70 percent of True Bedouin men took wives who did not belong to their own co-liable group, or, as I would say, wives who were not dependents of men in their own co-liable group (112/70).

[74] The same considerations apply, *mutatis mutandis*, to the members of co-liable group sub-groups mentioned in nn. 61 and 63 above.

[75] Cf. his remark that "Women who marry away do not sever their connections with the group" (64/59).

[76] Marx himself sometimes refers these groups as "attached" and sometimes as "accreted," but seems to use the two terms synonymously. According to Kressel (2003: 33) the Bedouin refer to such groups as lumuma (*lamuma* in Ben-David 2004: 93) or *mahmiyyat* (protectees).

[77] 71/64. At one point Marx writes of the Bdur group, which was especially close agnatically to the Abu Gwe'id-Rahahlah sub-clan (174/119), that it "is considered to be descended of the tribal ancestor" (131/116). If this means that the Bdur are believed to be descendants in male line of Lahaib, the apical ancestor of the Abu Gwe'id clan, then the Bdur must surely be looked on by the Bedouin as members of the tribe by birth, and not by virtue of their relationship of dependence with the Abu Gwe'id-Rahahlah sub-tribe.

[78] I mention here a small puzzle concerning the Abu Rafi'a group. On p. 69/62 it appears as an External Bedouin group linked to Abu Gwe'id-Rahahlah core-group; and this fits in well enough with the fact that the whole group—only three tents—is mentioned as forming a longstanding camp together with three other tents from the Abu Gwe'id tribe (179–80/148–9). Yet elsewhere we are told that Abu Rafi'a belongs to the Abu Qraynat tribe (230/193).

account of another of the Zullam tribes (Abu Rbeʻah), defines the Related Bedouin as groups which originally belonged to other Zullam tribes, and the External Bedouin as groups who are also True Bedouin, but not of Zullam origin. It may well be that Marx used the same criteria.[79] It seems that the External Bedouin own land.[80]

Fallahin The link between the Fallahin groups and the sub-tribe seems to be based mainly on the fact that the Fallahin cultivated land owned by members of the sub-tribe. But there was also some sort of political relationship, quite apart from any that may have been created as a result of the Israeli law demanding that each Fallah be registered as belonging to a tribe. The Fallahin owned some land, all of which had been bought from the True Bedouin (Marx 1973: 412). Such a purchase was not a purely economic transaction, for "the peasants ... remained under the protection of the tribe from which the land had been bought" (77/68).[81] Marx notes that a single Fallah co-liable group might be divided into different parts, each linked to a different sub-tribe.[82]

Blacks The link between the Blacks and the sub-tribe is in most cases probably based on the fact that the ancestors of the Blacks were slaves of the ancestors of the members of the sub-tribe (though see 70/63). The Blacks were organized in co-liable groups (67/61), but we know nothing of their structure. It is not stated whether the Blacks owned any land.

Isolated individuals and families The data under this heading are obscure (67–8/61–2, cf. 88–9/78–9). The Hebrew version represents a revision of the English, and is in some ways clearer. At the same time it contributes a new problem of its own: the cross-reference in n. 12 on p. 72 of the Hebrew implies that the Abu Dghem and Abu Rumanah groups fall under this heading, whereas in the diagram on p. 69/62 they

---

[79] Ben-David's usage of the term External Bedouin is, however, in at least one respect different from Marx's. Ben-David, in his account of the Abu Rbeʻah sub-tribe of the Abu Rbeʻah tribe of the Zullam, tells us that one of the External Bedouin groups, the Mtayrat (which claims for itself Bili origin), joined the Abu Rbeʻah co-liable group (Ben David 2004: 94). In other words, the Mtayrat had become part of what I call the sub-tribe and what Marx calls the core-group. Now Marx was aware of the possibility that non-members of the Abu Gweʻid-Rahahlah sub-clan might join the Abu Gweʻid-Rahahlah co-liable group, and hence (in my analysis) become members of the sub-tribe (see n. 63 above). In contrast to Ben-David he would not therefore have classed the Mtayrat as External Bedouin.

[80] "The attached and related Bedouin groups own land" (74). I take it that the phrase "attached Bedouin groups" in this statement refers to what are elsewhere referred to as the "external Bedouin groups."

[81] See also Jaussen's excellent account, dating back to the early twentieth century, of how Palestinian peasants came to cultivate the land of the Bedouin in Transjordan (Jaussen 1948: 242–3, and for the Negev, 246–7).

[82] 77/69, where the figures present some problems, Abu ʻArar is recorded as having 24 adults affiliated with the Abu Gweʻid tribe, but elsewhere (245/225) it is stated that 18 of these adults were male. The corresponding figures for Abu Gameʻ are almost equally implausible: 26 adults of whom 18 were male. (The Abu ʻArar contained 98 adults [77/69], of whom only 39 were males [202/168], which makes the alleged preponderance of males among those affiliated with Abu Gweʻid even more unlikely.)

are categorized as External Bedouin. It is at any rate evident that the people in this category are few in number and of little importance.

VI.2

Marx does not offer many details about the ties between the sub-tribe and its various attached groups. The relationships of the Fallahin and the Blacks with their respective sub-tribes evidently existed in some not very different form even before 1948, although they may have been altered by the arrangements imposed on the Bedouin by the Military Administration. Marx remarks that the Black group that is attached to the Ma'abdah "is attached only for registration purposes" (70/63). This indicates that its ties with the Ma'abdah are not of a kind based in Bedouin custom; whether it had customary ties with some other group is not stated.

Marx's view of the nature of the relationship between the sub-tribe and its Fallahin dependents, and the influence of that relationship on the structure of the sub-tribe, is illustrated by a remark he makes about the Ma'abdah sub-tribe, which had no Fallahin attached to it, and which, it will be remembered, consists of no less than six distinct co-liable groups. "The Ma'abdah never needed to be as united as the other two core-groups of the [Abu Gwe'id] tribe,[83] as they did not have to keep peasant sharecroppers in subjection" (154/115, and similarly 201, 229/166, 192, where there are references to the possibility of "rebellion" or "insurrection" of the Fallahin against the True Bedouin).[84]

Whether the ties of the Related and External Bedouin were of a kind that existed before 1948 is less clear. At least as regards the first of these groups, we know that "they are not bound to their core-groups by the well-defined obligations and interdependence that prevail between members of co-liable groups" (133/117). This implies that there is no formal, legally binding customary law contract between a Related Bedouin co-liable group and the sub-tribe to which it is attached. Marx writes of one such group that "they have freely attached themselves to the sheikh's *ruba'* [i.e., the Abu Gwe'id-Rahahlah sub-tribe], in order to enjoy his political protection and in order to obtain land,"[85] and goes on to indicate that should they wish to do so, they could easily switch their allegiance to another sub-tribe of the Abu Gwe'id tribe.

---

[83] There seems to be some confusion here, since there were actually three other coregroups in the Abu Gwe'id tribe (see Table 17.1 of this chapter).

[84] Kressel 2003: 33–68 offers considerable information about the traditional relationship between a True Bedouin and his Fallah tenant; see especially 50–51 on the non-economic ties between the parties.

[85] 71/64. The phrase "and in order to obtain land" appears only in the Hebrew version; the Hebrew also makes it clear that the phrase "freely" means "of their own free will."

VI.3

Marx views *all* the Bedouin as belonging to one tribe or another; I have suggested, in contrast, that if one of the tribal units is a territorial group, then we must assume that all the tribal units are made up only of True Bedouin.

A different way of approaching the question of who belongs to a tribe would be to ask the True Bedouin themselves what they think. I suspect that Marx was misled both by the way in which the Military Administration grouped the Bedouin, and by the way in which the Bedouin talked about the attached groups. I have little doubt that they sometimes used the term *'ashirat Abu Gwe'id* in contexts where it was clear that they were referring both to those whom I have called members of the tribe and to its attached groups. One might guess, furthermore, that they said things like "our Blacks" or "the Fallahin of the Abu Gwe'id tribe." Such expressions would not mean that these groups were members of the tribe, merely that they were attached to them in some way. I believe that a close investigation would show that the True Bedouin themselves make this distinction.

Marx writes that "membership of the *ruba'* [i.e., the sub-tribe], the tribe and the tribal group is obtained by permanently living on Zullam land."[86] This is misleading. One obtains the right permanently to live on Zullam land either by being born a member of one of the Zullam tribes; or by a contract with a member of a Zullam tribe (e.g., by marrying a man of the tribe, by buying land from a member of the tribe, or by entering into a share-cropping agreement with a member of the tribe); or by being born of a father who had such a contract; or by marrying a man who has such a contract or has inherited the benefits of such a contract. One becomes a member of a Zullam tribe either by being born of a man of the tribe, or, much less commonly, by a special contract (e.g., joining a co-liable group of the tribe). Not all members of a tribe live on its land (the children of a man of a Zullam tribe would, in Bedouin eyes, be members of the tribe even if they had never set foot in the tribal territory), and many of those who live permanently on its land are not members.

Marx's account does not take into account a fundamental feature of the Bedouin legal system. As in most systems of archaic law, including those of sedentary peoples, mere presence, or even residence, in the territory controlled by a given people does not in itself endow a person with much, if anything, in the way of legal rights. In order to obtain legal rights a person must enter into some kind of formal relationship with the people.

---

[86] 185/153, where the word "tribal group" in the English is translated in the Hebrew, evidently erroneously, as "confederation."

VII. Conclusion

In this chapter I have been concerned with two different kinds of difficulties in Marx's book. One is terminological. Marx has, I think, been too economical in this respect, sometimes consciously using a single term to refer to several different entities. The other is conceptual. I believe that some of the concepts that Marx uses do not reflect those used by the Bedouin themselves. The most striking instance is the co-liable group. For the Bedouin this is a group that includes only men, whereas Marx has also included in it women and children. In my own presentation of the material I have tried to reflect the categories that the Bedouin themselves use. This is not to say, of course, that they have anything in their own language that corresponds to the elaborate terminology proposed here. They get by very well with a much more limited range of terms, since the context in which a term is used will normally make it sufficiently clear what the speaker is referring to.

Marx based his book on a total of about twenty-one months with the Bedouin (xi/9), and in that period he accumulated a remarkable amount of information. But twenty-one months is not enough time for an outsider to master the intricacies of Bedouin customary law, especially given the fact that the Bedouin prefer to keep to themselves the details of many of their disputes. And one cannot give an accurate account of the traditional social structure of the Asiatic Bedouin without dealing with their legal system in some detail. What happens on the ground is, of course, not always the same as what the law demands; but to understand fully what happens on the ground one needs to understand the law that lies behind it.

It is not uncommon for an ethnography to be re-analyzed, but in many cases such a re-analysis can be little more than speculative. Those who have the privilege of working on Marx's data are in a very different position. The richness and accuracy of the information that he provides is such that his work is, and will remain, part of the short list of indispensable anthropological studies on the Middle East.

References

Abu-Rabia, Aref. 1994. *The Negev Bedouin and Livestock Rearing.* Oxford: Berg. al-ʿArif, ʿArif. 1933. *al-Qadaʾ bayna al-Badw.* Jerusalem: n.p.

———. 1934. *Taʾrikh Biʾr al-Sabʿ wa-qabaʾiluha.* Jerusalem: n.p.

Ben-David, Yosef/Joseph. 1989. The Negev Bedouin: from Nomadism to Agriculture, in *The Land that Became Israel: Studies in Historical Geography*, edited by Ruth Kark. New Haven: Yale University Press, 180–95.

———. 2004. *Ha-Beduim be-Yisrael.* Jerusalem: Mekhon Yerushalayim le-Heqer Yisrael.

Canaan, T. 1928. Die 'Azazime-Beduinen und ihr Gebiet. *Zeitschrift des Deutschen Palästina-Vereins*, 51, 89–118.

Gal-Pe'er, Ilan. 1979. Be'er Sheva' ve-ha-Beduim, in *Sefer Be'er Sheva'*, edited by Y. Gradus and E. Stern. Jerusalem: Keter, 269–98.

Gilbert, Margaret. 2006. *A Theory of Political Obligation.* Oxford: Clarendon Press.

Henkin, Roni. 2010. *Negev Arabic.* Wiesbaden: Harrassowitz.

Jaussen, Antonin. 1948. *Coutumes des arabes au pays de Moab.* Paris: AdrienMaisonneuve.

Kressel, Gideon. 2003. *Let Shepherding Endure.* Albany: State University Press of New York.

Marx, Emanuel. 1967. *Bedouin of the Negev.* Manchester: Manchester University Press.

———. 1973. Circumcision Feasts among the Negev Bedouins. *International Journal of Middle East Studies*, 4, 411–27.

———. 1974. *Ha-hevra ha-Beduit ba-Negev.* Tel-Aviv: Reshafim.

———. 1977. The Tribe as a Unit of Subsistence: Nomadic Pastoralism in the Middle East. *American Anthropologist*, 79, 343–63.

Meir, Avinoam. 1996. Territoriality among the Negev Bedouin in Transition from Nomadism to Sedentarism, in *The Anthropology of Tribal and Peasant Pastoral Societies*, edited by Ugo Fabietti and Philip Carl Salzman, Pavia: Collegio Ghislieri, 187–207.

Musil, Alois. 1907–08. *Arabia Petraea.* 3 vols. in 4. Vienna: Kaiserliche Akademie der Wissenschaften.

Oppenheim, Max von. 1939–68. *Die Beduinen.* 4 vols. Leipzig and Wiesbaden: Otto Harrassowitz.

Randolph, Richard R. 1963. *The Social Structure of the Qdiiraat Bedouin.* Unpublished Ph.D. dissertation, University of California, Berkeley.

Shmueli, Avshalom. 1970. *Hitnahalut ha-Beduim shel Midbar Yehuda.* Tel-Aviv: Gome.

Stewart, Frank H. 1986. *Bedouin Boundaries in Central Sinai and the Negev.* Wiesbaden: Otto Harrassowitz.

———. 1988–90. *Texts in Sinai Bedouin Law.* 2 vols. Wiesbaden: Otto Harrassowitz

———. 1991a. Le-mivneh ha-hevra ha-Beduit ba-Negev. *Ha-Mizrah he-Hadash*, 33, 132–44.

———. 1991b. The Woman, her Guardian and her Husband in the Law of the Sinai Bedouin. *Arabica*, 38, 102–29.

———. 2003. The Contract with Surety in Bedouin Customary Law. *UCLA Journal of Islamic and Near Eastern Law*, 2, 163–280.

———. 2006. Customary Law among the Bedouin of the Middle East and North Africa, in *Nomadic Societies in the Middle East and North Africa*, edited by Dawn Chatty. Leiden: Brill, 239–78.

———. 2010. The Word *xamsah* in Bedouin Language and Law. *Jerusalem Studies in Arabic and Islam*, 37.

Chapter 18: Bones of Contention

Alex Weingrod

Public burials and reburials typically are political events, organized and produced by the state to serve its own particular purposes. Consider, as one example, the following series of events.

On May 11, 1982, the Israeli Prime Minister Menachem Begin presided over an elaborate ceremony in which the skeletal remains of nineteen persons were reburied on a cliff overlooking Nachal Chever, a remote spot in the Judean Desert. The bones were proclaimed to be remains of the followers of Bar Kochba, the heroic-yet-failed leader of the second century Jewish revolt against Roman rule, and they had been discovered in nearby caves twenty years earlier by Yigal Yadin, the celebrated Israeli archeologist. Analysis of the bones showed that these were the remains of men, women and children, but the media immediately labeled them "the Bar Kochba warriors," and Begin, who had a flair for the melodramatic, decided that these ancient warriors should be reburied with full military honors in an official ceremony sponsored by the State of Israel.

Prime Minister Begin was the impresario and a moving force behind the happening, and he enlisted the Israeli Army to provide the proper setting and organization. The day's events were both surreal and, somehow, oddly impressive. Activities began in the morning when army helicopters ferried the hundred and fifty invited guests from Jerusalem and Tel Aviv to nearby specially constructed landing pads. The list of those attending was extraordinary—it included the President of the State, the Speaker of the Knesset and numerous Knesset members, the Chief Rabbis, past and present government ministers, the army chief of staff and selected generals, the heads of the Supreme Court and Bank of Israel, plus various other dignitaries. This was, in other words, a state ceremony in which the audience was made up of the highest level political elites. The helicopters also brought three identical wooden caskets with the warriors' remains, and the ceremony began when soldiers lifted the caskets from the planes and carried them to their desert burial place.

The program followed the Israeli Army's standard Jewish burial practices. Mourning prayers were recited, soldiers placed wreaths with the inscriptions of various state agencies on the graves, and an honor guard fired three volleys. The final eulogy was delivered by the Prime Minister. Standing stiff under the hot desert sun, Begin spoke at length about the historic ties between Bar Kochba's brave warriors and the Israeli soldiers of today; he repeatedly mentioned the unification of Jerusalem under Israeli rule and the "liberation" of the West Bank, and at the close of his remarks he loudly

proclaimed: "Our glorious fathers, we have a message for you: we have returned to the place from whence we came. The people of Israel lives, and will live in its homeland in Eretz Yisrael for generations upon generations. Glorious fathers, we are back and we will not budge from here" (Aronoff 1986:19–20). The ceremony then ended, the distinguished guests quickly re-boarded their helicopters and roared away, and only the three fresh graves remained.

It had been an extraordinary performance. The event itself was entirely contrived—there was no way to *really* know whose bones had been reburied. Why bring the state's elite leadership to a lonely desert site to view chief rabbis conducting funeral rites over make-believe bodies? Had the Israeli Army nothing better to do than to fly politicians into the desert to attend mock funerals? And, indeed, this state-managed event was loudly criticized from many directions. Opposition Knesset members called it "a farce," "the worship of false gods," and a perversion of "the meaning … of the reburial of bones in Jewish tradition." Yadin, the archeologist who had originally uncovered the skeletal remains, and who also was a prominent member of Begin's government, was conspicuous by his absence: he complained that the event had "a somewhat overly nationalistic flavor," and, besides, "the landscape is too beautiful and it's a shame to ruin it." Finally, a band of youthful protestors parodied and burlesqued the ceremony while it was taking place. They had sneaked into the area before dawn, and then, dressed in Roman-styled togas and tin helmets, they suddenly appeared waving signs and loudly chanting "You're making a laughing stock out of history." Policemen and soldiers chased after them, and for an hour or so they raced up and down the nearby hills (Aronoff 1986).

Contrived, burlesque, a farce—and yet, this desert reburial was a supremely serious event. Put in general terms, it aptly illustrates ways in which modern states lay claim to and make use of the "bones of ancestors" in order to advance their particular political goals and ambitions. To put it differently, the bones of ancestors are something of a treasure, and the state decides how to expend these assets. Not unusual in Zionist discourse, Begin drew a direct line between past and present: the Israeli troops were represented as the descendants of Bar Kochba and his warriors, and overlooking the fact that their ancient revolt ended in national disaster he made parallel non-negotiable claims in the present. Moreover, while reburials may be presented as having religious motifs or as "celebrations of the past," they are primarily meant to advance the goals of political actors and their agendas. Andras Zempleni's caustic remark (referring to contemporary Hungarian reburials) sums it up appropriately—these are instances of "politics at the edge of the grave" (Zempleni 2003).

Keeping this event in mind, let me explain the objectives of this chapter. My focus will be on political burials and reburials, as well as some recent monuments of memorialization, among both Israeli Jews and Palestinians, including Israeli Palestinians. I want to examine three loosely related issues: First, how can we best explain the widespread practice of state-sponsored reburials? What sense do they make? Second, why are reburial and memorialization projects unfinished, what is the dynamic

that spurs their restless energies? And third, what can these practices reveal about the political as well as cultural structures of Israeli (and perhaps other) contemporary societies?

The Pantheon's Message

And so, we are off to the cemeteries, first to Mount Herzl, the Jerusalem hilltop that has been transformed into the Israeli version of the Pantheon, the place where the nation's founders and heroes are buried. The overall design of this national cemetery, and the growing number of its monuments, constitutes a statement and consequently an interpretation of Israeli history. Mount Herzl continues to be shaped into an historic document that is, inevitably, unfinished.

This founding state memorial originated when, in the summer of 1949, Theodore Herzl's bones were transferred from Vienna to Jerusalem. It was among the new Israeli government's first decisions, and it seemed perfectly appropriate. Herzl was the prophet of Zionism, he had inspired and led the political movement that culminated in the creation of Israel, and in his will (Herzl died in 1904) he requested, with a seer's confidence, that he and members of his family be reburied in the Jewish State. What could be more fitting?

The reburial was set for August 18, 1949. It was a hot summer day, and the crowd stood while speeches were delivered, prayers offered, and then, covered with the Israeli flag, the casket with Herzl's bodily remains was lowered into the ground. In the ceremony's concluding act, representatives of settlements from across the country poured sacks of earth from their home area into the open grave. Herzl, the nation's prophet, was thereby symbolically united with Eretz Yisrael, the Land of Israel (Azaryahu 1996).

Herzl was the first to be buried or reburied on the hill that bears his name, but since then much of the hilltop has been covered with trees, flower-beds, paths to stroll along, and, of course, graves and gravestones. It has become Har HaZikharon, Israel's "Mountain of Memory." The entire area—about the equivalent of twenty acres— is thematically grouped into different sections. The first includes Herzl's grave and the nearby graves of the historic leaders of the Zionist movement. Second, an area is set aside for "G'dolei ha'umah," the "nation's greats," where many of Israel's political leaders are buried. All of these "greats" are former political leaders—there are no poets or Nobel Prize winning mathematicians buried in the Israeli Pantheon. Third, Mount Herzl also includes a large military cemetery where soldiers who died in Israel's repeated wars lie buried. These are the hubs of the Pantheon, and, more recently, around its outer edges a number of new memorials have been added, and more are planned.

Herzl's grave, covered with a large black marble gravestone and engraved with a single word, "Herzl," is the central site at the hill's summit. Black in color and simple

in form, the gravestone's plain simplicity makes a strong statement of restraint and power. In his will Herzl requested that his family be buried near to him, and over the years the bodily remains of his parents, sister and three children have been re-buried nearby. This series of graves is off to a side and peripheral: Herzl is the star of the hill that bears his name, and with a large Israeli flag positioned overhead, his grave is at center stage.

The second major section is an avenue of graves reserved for "G'Dolei Ha'Uma," the "The Greats of the Nation." This relatively small area is placed at the top of the hill, separate from Herzl's tomb and the graves of the Zionist leaders who represent the period before the state's formation. These are the burial places of the state's political leaders—Presidents, Prime Ministers, Speakers of the Knesset, and occasional others. Who is a "Great," and therefore qualifies to be buried on Mount Herzl, is a matter decided by a government committee. Not all of Israel's past political leaders wished to be buried here—for example, the graves of David Ben Gurion, Moshe Sharret, Menachem Begin, and Chaim and Ezer Weizmann, uncle and nephew, are conspicuously absent. The graves and gravestones in this section are modest and straightforward; wives are buried alongside of their husbands, and hence "the greats" are aligned as small families.

Mount Herzl is also Jerusalem's military cemetery, and more than 4,000 soldiers who died in Israel's many wars, including those who were Jerusalem residents, are buried there. Shaded by the cypress and evergreen trees, the military cemetery ranges along the lower reaches of the hill, then tilts upward in the direction of the graves of the political elite. These graves are spartan in presentation— minimal, polished, lined up in rows as if at some military roll-call. The fallen soldiers belong to the nation— the state's existence depends upon sacrifice in war, and the long lines of identical tombstones merge personal tragedy with national salvation.

In an elegant and powerful essay titled "The Presence of Absence: The Memorialism of National Death," Don and Leah Handelman have written about the meanings of Har Herzl, and particularly the military cemetery and the nearby Holocaust memorial called "Yad Vashem." Their description of the military cemetery is worth quoting:

> The grave consists of a rectangle of stone raised above the surface of the ground, and covered by earth planted with the evergreen shrub, rosemary. The effect is that of the body within rock and earth vital with life. The dead not only rest, but nourish the living earth of the national landscape with their last remains ... This careful cultivation of the cemetery accords with the Zionist vision of the reclamation of the wasteland ... Each self-sacrificed body put into the soil practices the nationalist vision of the state and its territory. (2004:151)

The Israel Department of Defense has overall responsibility for maintaining this section of Har Herzl, and families and close friends also tend the graves of their loved ones. They gather in small groups on the day of the soldiers' death, or on some other

personal date, and on Yom Ha'Zikaron, Remembrance Day, the day that precedes Israel Independence Day, the cemetery is overflowing with families who place flowers and wreaths on the graves. This portion of Har Herzl is unfinished—each military operation, skirmish, or war, brings additional casualties and deaths, and consequently new burial sites are prepared where comrades-inarms are buried together in the same cluster.

Herzl's grave, the "Nation's Greats," and the military cemetery, are the central spaces and major spokes of the Israeli Pantheon. Together they are meant to tell the story of Israel's past and present, the state's creation and the sacrifice of war and death. However, this narrative can be amplified, changed, improved upon, and consequently other memorials have been constructed and more apparently are on the way.

There are, of course, other persons and different interpretations, and in the case of Mount Herzl, this is best illustrated by the reburial of Zev Jabotinsky, the founder and historic leader of the Zionist Revisionist Movement. A critic of the pre-state Labor party leadership, in 1923 he resigned from the World Zionist organization and organized his own political movement, and in Palestine his followers also organized their own Rightist political groups. Jabotinsky died in 1940 while in the United States, and he was buried in a cemetery in New York. In his will he requested that he be reburied in the Jewish State—but only when the government formally requested his reburial there.

Re-burying Jabotinsky consequently became a lively political issue. In the 1950's his Israeli followers requested that he be reburied in Israel, and equally important, that his new grave site be located on Mount Herzl. Ben Gurion, who was then Prime Minister, refused the request. However, his successor adopted a policy of rapprochement between the Israeli Right and Left, and in 1964 the Labor-led government agreed to transport the remains of Jabotinsky and his wife and rebury them on Mount Herzl. But then the question arose where to locate the grave: Jabotinsky had organized his own Zionist organization, and he also had not been one of the "Greats." Where should he be buried?

The solution was to rebury Jabotinsky in a separate spot situated at a distance from both Herzl's tomb and the graves of the "Nation's Greats." Reburying Jabotinsky on the nation's sacred reserve not only initiated a new state ceremony, it also began the process of legitimizing his political followers. The reburial was a well-understood symbolic act, and it had practical results: in 1966 the Herut party joined the government coalition, and in the decades that followed Jabotinsky's ideological heirs became a dominant Israeli political party. What is more, the story told on Mount Herzl also changed. The past became more complex—there were several competing Zionist political movements, and the "Nation's Greats" included the historic leaders of different Israeli political parties and ideologies.

State memorial spaces seem always to be open to modification and elaboration—as cultural-political sensibilities change, and as new groups rise to power, new monuments are created alongside the old. Along the northern ridge of the hill, in an area bordering between the military cemetery and nearby Holocaust Memorial, Yad Vashem, a series

297

of quite different sites have recently been added. Although these memorials also bear the names of the dead, they are not dedicated to a particular person, but rather to an ideal. Rather than celebrating the memory of a famed hero or fallen martyr, they seek to add different dimensions and interpretations to the Pantheon.

The Return of Jews to Israel, aliya in Hebrew, is one of Zionism's most fundamental and deeply valued acts. In the 1990s two new memorials were built, each joined to the other, dedicated to those who perished in the attempt to Return. The first tells the story of a tiny ship, the Salvador, that had set out from Rumania in December, 1940, with some 350 Jewish passengers who were seeking refuge in Palestine. The boat succeeded in reaching Turkey, but later sank in a storm off the Turkish coast. Most of the passengers drowned, some were saved (those sent back to Europe perished in Nazi death-camps) and a small number of bodies were recovered and buried in Turkey. The Salvador's tragic story is told on a plaque attached to the memorial, and, in addition, the remains of persons originally buried in Turkey were reburied here.

The second story of heroic immigrants has many similarities. Again, a small boat (it was originally named the Pisces, but following the disaster it received a Hebrew name, Egoz) packed full with Moroccan Jews illegally set out for Israel, and it capsized in a storm and sank in the seas between Gibraltar and Morocco. All of the forty-three immigrants aboard the ship drowned (the captain and crew escaped unharmed). Some of the bodies later washed ashore, and the Moroccan authorities buried them in a mass grave. This tragic event took place in 1962, and for the next three decades sporadic negotiations were conducted between the Israeli and Moroccan authorities regarding transferring the bodies for reburial in Israel. Finally, in 1992, the Moroccan officials agreed, and the bodily remains flown to Israel for reburial on Mount Herzl.

But there is more to this particular memorial then a tribute to immigrants and their Return to Zion. The social divide between European and Middle-Eastern origin Jews in Israeli society is well known: without going into all of the details, the European, or Ashkenazi segments, have held dominant positions in the society since its early twentieth-century beginnings, while the Middle Eastern origin groups, including Moroccan Jews, were thrust into lower status positions. This has been a continuing source of ethnic tension and conflict, and the decision to rebury the twenty-three Moroccan olim on Mount Herzl and to dedicate the memorial to them, was meant to celebrate, or to "naturalize," their place in the society. They were being defined as "maapilim," courageous illegal immigrants who were willing to court extreme danger in order to reach the shores of Zion. This term and its associated prestige had until then been reserved for European Jews who braved the 1940s British blockade of Palestine. Their reburial on Mount Herzl had a clear political message, signaling the fact that Moroccan Jews had played a heroic role in the state's creation, and that they too made the ultimate sacrifice.

Finally, two additional new monuments are situated nearby, and they too lend new meanings to this "Mountain of Memory." The first is a memorial to Israeli civilians who were killed in Arab-Jewish communal violence and terror attacks. Called "The

Monument to Victims of Terror," the design of this memorial takes the form of a large open plaza, polished white in color, and faced by a wall on which are engraved the names of more than 600 persons who were killed in violent Arab-Jewish encounters. The names are listed by decade, beginning with 1860 and continuing across the wall from left to right until they reach the 2000s. There are no actual burials or reburials, but the long lists of names are meant as a testimony and remembrance to those killed in Jewish-Arab, and later Israel-Palestinian, communal violence.

Each of Mount Herzl's monuments tells its story, and the "Monument to Victims of Terror" is no exception. The monument was built after lengthy negotiations and represents something of a compromise. During the 1990s, and especially at the time of the second Palestinian intifada, attacks against Israeli civilians became almost a fact of everyday life, and many were killed and injured. Some of the families joined together in order to assist one another, and also to advance the interests of those who had suffered loss. A principal request was that civilian deaths be equated with the death of soldiers killed during war—for example, that those killed in terror attacks be buried in military cemeteries, and that their families receive funds and services from the Israeli Army and Department of Defense. Following lengthy discussions, the army rejected this request—they argued that death in war was different from civilian casualties, and that civilians who were "victims" were not the same as soldiers killed in military combat. Consequently, the terror victims came under the jurisdiction of the Israeli Social Security Agency (Bituach Leumi), and the monument on Mount Herzl was financed by this group.

The "Monument to Victims of Terror" is not just in memory of those who were killed: it is meant to tell the story of the conflict over Eretz Yisrael, the Land of Israel, and of the Jews who were killed by Palestinians during the last century and a half. The message is somewhat complex, but also clear. Today's violence is nothing new, but rather the continuation of previous decades of ceaseless conflict. The message is pessimistic, or perhaps realistic—present-day aggression is the continuation of an ancient struggle, and there can be little hope that it will be soon resolved. No less important is the monument's phrasing of the dead as "victims"— the six hundred persons whose names are etched on the wall are categorized as "victims of Arab aggression." (Not all of them are Jews—terror attacks also claimed Israeli Palestinian victims, and their names are also inscribed on the wall). How the names were selected, or what distinguishes a "victim" from Jews who in earlier periods fought and were killed in armed battles with Arabs, is unclear. Moreover, the "victim" phraseology is part of a relatively new Israeli discourse that depicts Jews as eternal victims, and thereby presents a particular interpretation of historic events and their meanings. Not surprisingly, this monument is positioned on the Mount Herzl ledge closest to the nearby Holocaust memorial, and it is meant to bind these two neighboring memorials into a single narrative.

The second recently constructed monument both reinforces these claims and also adds new dimensions to it. Completed in 2002, this monument is called "Netzer Acharon," meaning "the last offshoot," and it commemorates the memory of Holocaust

survivors who reached Israel on the eve of the 1948 War of Independence, and, thrust immediately into battle, were killed during the war. They were the only surviving members of their family—the others perished in the destruction of European Jewry—and thus they were "the last offshoots" whose memory, otherwise lost, is being preserved and celebrated. The monument has the names of 275 dead soldiers inscribed on it, and it is also located on the elevation that looks down towards Yad Vashem. In addition—and this is the monument's most radical feature—a new asphalted "linking path" has been built that leads between this monument on Mount Herzl and the adjoining Yad Vashem memorial. A yearly march now takes place along this path—in this new state-sponsored ritual youth movement youngsters begin their march in the Holocaust Memorial, and then walk en masse to several sites on Mount Herzl, finally returning to the "Last Offshoot" monument. In this fashion Israel's history, its leaders and fallen warriors are symbolically joined together with the dark destruction of the Holocaust.

Even though they were designed to be adjoining state memorials, Yad Vashem and Mount Herzl were originally conceived as entirely different places. Without going into all of the details, Yad Vashem expressed the Holocaust horror in which diaspora Jews were marched off to their destruction, whereas Mount Herzl celebrated Israel's national rebirth and the heroic sacrifice of young warriors who died defending their old-new land. Yad Vashem was a kind of "foreign land within Israel," whereas Mount Herzl was etched into the living tale of Zionism and the modern Jewish State. As Jackie Feldman has convincingly argued, the new monuments and the "linking path" between them change their relationships in a radical way:

> The Israeli narrative of battles for the nation-state is recast as part of a specifically Jewish narrative of eternal suffering and victimization. The discontinuity between the soldier who dies in combat, and the victim of the Holocaust has narrowed. In the new landscape, they leak into one another ... The child killed by terrorists on a city bus, the immigrant suffering and dying on his way to the Land, and the survivor-soldier, become the bridge between them. (Feldman, 2008: 1158)

In this new version the presumed Jewish fate becomes accentuated, and continuity is established between the bitter Diaspora experience and the everendangered, embattled Israeli state. The stories are merged, previously distinct messages are blurred, and old-new themes become accentuated.

Mount Herzl and its Critics

Mount Herzl appears as a sacred place, a national monument where society-wide consensus reigns. The state and its bureaucracies control the site, and who would find fault with Herzl's tomb or the ways in which fallen soldiers are buried? In fact, however,

acts of opposition periodically do take place, and the normally placid atmosphere is sometimes disturbed by violent acts as well as highly emotional debates. The opposition takes various forms, and it both reflects and expresses some of Israeli society's major divisions.

One field of conflict involves the state, who controls and is symbolized by the Pantheon, and members of Israel's ultra-orthodox Jewish communities, or haredim in Hebrew, who do not recognize the state's authority. Some of these groups consider their own rabbinic authority to be pre-eminent, and they reject the secular state's political prerogative as well as its symbolic expressions, such as the flag and the national anthem.

Mount Herzl is a monument to Zionism and the State of Israel, and consequently it has been the target of occasional attacks by ultra-orthodox protestors. For example, in November, 1995, two ultra-orthodox young men were arrested and accused of desecrating the grave of Yitzchak Rabin, the Israeli Prime Minister who had been assassinated the previous year. When brought to court, one of the haredi men explained that they were expressing opposition to placing wreaths on graves, a "custom that is non-Jewish" (Haaretz, February 1, 1996). To cite another instance, in 2001 the police reported that the graves of Herzl, Levi Eshkol, Golda Meir, and Jabotinsky, had been vandalized; black paint was sprayed on the graves, and on Jabotinsky's memorial the attackers left a message warning archeologists who "were digging in Jewish graves."

What lies behind attacks on the Israeli Pantheon? These aggressive acts reflect the deep split between the secular majority and the increasingly militant ultraorthodox minority. The major issue that divides these groups is their recognition and allegiance to the State: the secular majority, as well as substantial numbers of religious Jews, are committed to and thoroughly engaged with the state, while, in contrast the haredi sectarian groups do not recognize the state's authority since it is not based upon orthodox religious prescription and was established in political struggle rather than having been created by God's will. The ultra-orthodox community has in recent years grown both in size and self-confidence—Jerusalem, the nation's capital, has a large activist haredi minority (a recent mayor was a haredi), and orthodox and ultra-orthodox Jews have a heightened sense of their power. Mount Herzl is an outstanding symbol and expression of Zionism and the secular state, and consequently it is an obvious place for protest and opposition, and even for occasional vandalism.

This is the principal reason, but there also are a number of additional features. First, although internally divided, the ultra-orthodox community considers itself the heir and principal exponent of Judaism. Their leaders, scholars and religious courts are the definers and arbiters of who Jews are and how they should behave— and consequently, as the youth accused of defacing Rabin's grave remarked, he was angered by placing wreaths on the grave since wreaths are a Christian rather than a Jewish form of commemoration. They are the Custodians of the Faith, and consequently they also decide and impose how the faith should be lived. Second, in one of the incidents the vandals scrawled a message attacking Israeli archeologists who frequently dig-up bones

in the course of their excavations. This has been a continuing source of controversy: "Keshet," a word scrawled on the graves, identifies a secretive ultra-orthodox group that has aggressively battled against the archeologists' practice of digging in cemeteries. This was a subtle, but also an unambiguous, message: if you, the archeologists who are supported by the state, continue to defile the bones of our ancestors, then we will also attack the remains of *your* ancestors on Mount Herzl. After all, Herzl and Rabin are Zionist ancestors, and hence the threat is clearly stated.

This is one field of conflict, and the military cemetery is the site of another. The tensions there are of a different kind, but they are nonetheless present.

One source of conflict has to do with the gravestones and other forms of remembrance. The Israeli Department of Defense regulations state that the gravestones must be identical: the dead belong to the nation, and consequently the regulations insist that each grave be the same as the others. On the other hand, each of the fallen soldiers belongs to a family, and some families wish to add a personal touch to their gravestone, or to inscribe some message of remembrance.

There is a long history of conflict and negotiation between the families and the army authorities. These are tense, complicated confrontations, in which the families are able to mobilize the power of the dead heroes, while the army personnel appear as bureaucrats insensitive to family members who bear the brunt of tragedy. These disputes have frequently ended up in the High Court of Justice, where so many of Israel's contested issues of principle finally arrive. For example, a recent suit asked that the name of a fallen soldier's sister be inscribed on the grave, next to the names of his parents. The High Court rejected the claim, although in its decision it added that new Army regulations permitted the family to place personal inscriptions "twenty centimeters from the front of the grave but not on the headstone." In effect, some families continue to press their wishes against the state's regulations, and negotiations between them sometimes produce new rules and modifications.

Another issue has been where to bury non-Jewish soldiers killed in war. Mount Herzl's military cemetery is exclusively for the Jewish dead. The Israeli Army also includes Druze soldiers, as well as smaller numbers who are Muslim and Christian, and those who died in war are buried in separate military cemeteries in their own towns and villages. However, in recent years, and with new countries of immigration, the question of whether a fallen soldier is Jewish, and consequently where he should be buried, has become a divisive issue. To be more specific: rabbinic authorities have questioned whether some Russian and Ethiopian immigrants were "Jews"—in Russia marriages between Jews and non-Jews were relatively common, and the religious status of Ethiopia's Jews has also been placed in doubt. A truly absurd situation therefore emerges—following immigration to Israel, Russian and Ethiopian-born youngsters are required to serve in the Israeli Army, but if they were killed in military action the Army's rabbinical staff decides whether they were "proper Jews" and therefore may be buried in a Jewish military cemetery! The decision is made by the Army's rabbinical authorities, and like others, they too have become "more orthodox" and therefore less

willing to find "creative ways" to bury comrades in the same military cemetery. Once again, this is a continuing issue of dissent, controversy and negotiation.

Palestinian Memorialization

Israel/Palestine, this tension-rocked Holy Land, is brim-filled with monuments. The best known and most prominent are the famed ancient religious memorials— the Cave of the Machpela in Hebron, The Church of the Holy Sepulcher and the Great Mosques in Jerusalem, the Churches in Nazereth, and many others. In contrast, the sites that have been considered thus far are national, and they too are spread across the country-side. Not surprisingly, a parallel process of recovering or designing national memorials can be seen among Israel Palestinians, as well as in the Palestinian West Bank and Gaza. There too new sites of memory have been constructed, and the Palestinian dead have been enlisted to serve the interests of the nation.

Following the 1948 War the Israeli Palestinian population found itself to be a dis-couraged, defeated minority, drastically diminished in population, its lands confiscated by the victors, and under the strict control of an Israeli military government. Since then, this growing minority (approximately 18 percent of the Israeli population) has sought to develop its own political institutions as well as fashion a Palestinian identity and political-cultural consciousness. One feature of this evolving process is the interest in identifying the ruins and remains of Palestinian villages that were destroyed during and after the 1948 War. Many of the villages have disappeared, practically obliterated, and in some cases Jewish communities were built on or near to the ruins of the previous homes. In recent years research projects have begun to identify these "lost" places, and memorials commemorating the place and persons who lived there have also been built. Similarly, some 1948 battle sites have been located, and plaques placed there to commemorate the Palestinian dead. In this way the Palestinian past is being recovered and given new meanings (Ben- Ze'ev and Aburaiya: 2004).

Two memorials are especially significant. The first is located in the Muslim grave-yard in Nesher, a small municipality that adjoins the larger urban center of Haifa. This is the burial place of Izz ad-Din-al-Qassam, a Palestinian who led the 1930's armed opposition to British mandatory control and to the Jewish settlers who were then ar-riving. Al- Qassam was killed in a skirmish with British troops in 1935, and buried in the village cemetery. His grave stood relatively unmarked and unnoticed, until the period of the first Palestinian intifada in 1986–1989. His name and memory were then resurrected by Palestinian militants, particularly those allied to Hamas, who named their West Bank and Gaza military group after him. With this sudden fame (or notori-ety), the grave in Nesher became something of a place of Palestinian pilgrimage (Ben- Ze'ev and Aburaiya 2004: 643). However, the site has also become a place of conflict, since groups of right-wing Israeli Jews have on several occasions defaced the grave. A

metal fence was built around it, but this too has been broken, and so the site itself has a forlorn appearance.

A second Palestinian memorial is located in Kfar Kassem, an Israeli Palestinian town in central Israel situated close to the West Bank. Kfar Kassem is infamous in Israel as the place where, in 1956, forty nine Palestinians were killed by Israeli soldiers in a terrible massacre. The time was October, 1956, during the Suez War, and local Arab villagers had been ordered by the army to remain within their village borders after five in the afternoon; however, many never heard the order, and when they returned to their homes later in the evening they were fired upon by a squad of Israeli Border Police. The dead included old men and women, and the officers responsible were put on trial, found guilty, and sent to prison for relatively brief periods of time.

In the years following the villagers observed the tragedy by staging a march and public memorial service—but the Military Government and police succeeded in banning any large demonstration. Later, in 1978, a modest monument was built near the town center that included the names of those who were killed, and this became the focus for a yearly gathering. This memorialization has recently been redesigned and enlarged: situated in a central square, a green marble monument rises like an obelisk, and the names of the dead are inscribed on the four sides. In addition, a museum devoted to the massacre and the victims is installed in the nearby community center, and the exhibits include pictures of the dead, official documents from the trial, as well as paintings done by local artists that express grief and anger. The event itself is commemorated yearly by a march through the town to the monument, where speeches and prayers are offered by local notables and officials.

The best known of the Palestinian memorials is the grave of the great historic Palestinian leader, Yassir Arafat, in Ramallah. Arafat was the dominant figure in Palestinian politics during the latter part of the twentieth century, and he traveled ceaselessly in world capitals in the effort to promote the Palestinian cause. Following the Oslo Accords in 1992, Arafat moved his headquarters from Tunisia to the West Bank town of Ramallah, where the Palestinian government center was built. This site, known as the Muqata, became his headquarters, and when the slim Israeli-Palestinian understanding began to fall apart, Israeli troops restricted his movements and made him a prisoner within his own office. Battles flared around the building, and much of the structure was destroyed. Later, in 2004, Arafat became ill, and was flown to France for medical treatment. Arafat died there on November 10, 2004, and arrangements were then made to return his body for burial in Palestine.

Arafat's funeral took place in two stages. His coffin was first flown to the Cairo airport, where a brief funeral service was held attended by Arab heads of state and diplomatic representatives from numerous countries across the world. Later the same day, Egyptian helicopters flew the coffin to Ramallah, where he was to be buried in the square that leads into the Muqata, the ruined government center. Thousands of Palestinian mourners had already poured into the square, and when the helicopters landed the police lost control of the crowd that engulfed the aircraft and the burial

site. Arafat's grave had earlier been prepared—earth had previously been removed, a border of Jerusalem stone and black-and-white marble was set in place, and five trees stood overhead. Earth taken from the Haram-al-sharif in Jerusalem was placed over the casket, and the burial and funeral service were quickly completed. Since then the Muqata has been rebuilt and much enlarged, and Arafat's tomb has also been redesigned. The grave is set as the center piece of a new cubical structure, during the day an honor guard stands at attention, and the mausoleum-like enclosure itself is made of Jerusalem stone.

Arafat's wish, it was said, was to be buried in Jerusalem's great mosques on the Haram-al-sharif. However appropriate, the burial at the muqata was considered to be a temporary resting place, so that, like Herzl, Arafat too may one day find his final burial place in the Palestinian State.

Conclusions

What do these tales tell us about national commemorations and monuments, politics and culture and the connections between them?

First, the political burials and reburials that I have described are by no means isolated events; comparable happenings have taken place in many other places. To cite examples: since the end of the Communist epoch, Hungary has witnessed literally hundreds of political reburials, the best known being the huge funeral and reburial of Imre Nagy, the hero of the failed 1956 Hungarian revolution, whose remains were "properly reburied" in the famous 1989 mass funeral ceremony; the repatriation and state-sponsored reburial ceremonies of "el-Negro" and the "Hottentot Venus," two native South Africans who, in separate circumstances, were finally repatriated from European countries where since the nineteenth century they had been degradingly displayed as non-humans; and the frenetic burials and reburials of the Perons, Juan and Evita, whose bones were shipped back and forth between Argentina and Europe. Other instances could be cited. Why this repeated interest in burying or reburying dead heroes?

Burials and reburials are particularly well-suited demonstrations of state power and national revival. Ceremoniously placing great men into the ground seems above rebuke—this is the kind of state-controlled action that can mobilize crowds and presumably generate common sentiments. The buried dead are portrayed as conduits and connections with the past— setting them into the ground and extolling their deeds links the present with the (supposedly) glorious past, binding present-day actors and policies with previous moments of glory.

Moreover, as Katherine Verdery wryly remarks in her book *The Political Lives of Dead Bodies*, "dead bodies do not talk," and consequently burials and state- initiated funerals are wonderful vehicles for political figures and others to depict and celebrate the dead as it suits their purposes (Verdery 1999). Who will argue or take issue with

sweet eulogies and wondrous tales told about those whose remains are about to be placed into the earth? What could be more fitting then reburying Herzl, the prophet of Zionism, on the highest hill in Jerusalem, or burying Yassir Arafat, the Father of Palestine, amidst the ruins of his muqata.

Nationalism is the powerful logic that underpins these events. Given its premises, reburying old heroes become perfectly sensible "natural" events. Peculiar or not, once established and in gear, nationalist thinking produces the logic that leads to digging up old bones and then reburying them in special ceremonies. Andras Zempleni sums it up accurately: "the main distinctive trait of the national cult of the dead is the turning of the latter into ritualistic means for the nation's self-celebration" (2004, p. 11). Moreover, over time nationalist thinking also takes previously unexpected twists, develops different versions, and reburials and commemorative structures are also statements of different approaches and new interpretations.

Why this continuous tinkering with national sites of memory? The impulse behind this is, in part at least, political—reburying Jabotinsky, or the new memorial to Moroccan immigrants, are political acts that add to or modify sacred space in order to fulfill on-going political interests and demands. This is a process also unfolding in other sites of national memory—for example, Washington's Mall and adjoining areas have recently seen new commemorative sites that tell the tales of other American groups, notably minorities, and the national myth consequently takes on changing dimensions. What is more, new monuments may be added as political views and sensibilities take on new and different dimensions. At one point in time the ancient Bar Kochba "warriors" are linked to present-day Israeli soldiers as exemplars of non-negotiable toughness, while, at a later moment, the image shifts radically to "Jews as victims": the basic ideology is not much changed, but the nationalist tropes and the images they employ are certainly very different. To be sure, such re-designs are not without heated debate and contention—the ideology of "Jews as victims" has its critics as well as supporters— and yet national memory and memorials may be continuously reshaped and presented anew.

Finally, what are we to conclude regarding the conflicts that periodically erupt at Israeli memorials and "sanctified places"? After all, national monuments, heroes reburied and memorials to supreme national endeavors, are supposed to be sacred sites, awesome places of silence and reverence: and yet, the Bar Kochba warriors reburial is burlesqued, the graves of Zionist founders are desecrated, and despite the state's objections the Kfar Kassem massacre is effectively memorialized. At some fundamental levels, Israeli society is deeply divided, and these divisions may be so fractious that opposing groups can directly challenge or reject the state's authority. In addition, on many levels power is divided between contending groups that struggle, negotiate and finally bargain with one another. The state and its bureaucracies may appear all-powerful, nationalist appeals and sentiments may be coercive, and yet resistance and opposition are not only possible, they often are effective. The nation's sacred scripts

are far from having been made final, and consequently the balances of power shift as new challenges and opportunities emerge.

References

Allen, Lori. 2004. Martyr Funerals in the Palestinian Intifada. Paper Presented at the University of Chicago Middle East History and Theory Conference.

Aronoff, Myron. 1986. Establishing Authority: The Memorialization of Jabotinsky and the Burial of the Bar Kochba Bones under the Likud, in *The Frailty of Authority*, edited by M. Aronoff. New Brunswick: Transaction Press.

Azaryahu, Maoz. 1996. Mount Herzl: The Creation of Israel's National Cemetery. *Israel Studies*, Vol. I, No. 2.

Ben-Ze'ev, Efrat and Aburaiya, Issam. 2004. "Middle-Ground" Politics and the Re-Palestinization of Places in Israel. *International Journal of Middle East Studies*, Vol 36, 639–55.

Benvenisti, Meron. 2003. The Politicization of Commemoration. *Haaretz*, May 22.

Feldman, Jackie. 2007. Between Yad Vashem and Mt. Herzl: Changing Inscriptions of Sacrifice on Jerusalem's "Mountain of Memory." *Anthropological Quarterly*, 80(3): 1145–72.

Hallote, Rachel and Joffe, Alexander H. 2002. The Politics of Israeli Archeology: Between "Nationalism" and "Science" in the Age of the Second Republic. *Israel Studies*, Vol. 8, No. 2.

Kimmerling, Baruch. 2001. *The Invention and Decline of Israeliness*. Berkeley: University of California Press.

Segev, Tom. 1998. The Battle of the Memorial. *Haaretz*, April 24.

Shafir, Gershon and Peled, Yoav. 2002. *Being Israeli: The Dynamics of Multiple Citizenship*. Cambridge, Cambridge University Press.

Shavit, Yaacov. 1988. *Jabotinsky and the Zionist Movement, 1925–1948*. London: Cass.

Verdery, Katherine. 1999. *The Political Lives of Dead Bodies*. New York: Columbia University Press.

Zempleni, Andras. 2003. Sepulchral Land and Territory of the Nation: Reburial Rituals in Contemporary Hungary. Lecture Presented to the European Studies Section, Annual Meeting of the American Anthropological Association.

Zerubavel, Yael. 1995. *Recovered Roots: Collective Memory and the Making of Israeli National Tradition*. Chicago, IL: University of Chicago Press.

Chapter 19: Through the Kaleidoscope: Looking Back at the Prison

Ofra Greenberg

Introduction

In 1972–73 I carried out research at the Neve Tirza women's prison in Israel under the supervision of Prof. Emanuel Marx, as part of my studies towards a Ph.D. degree in social anthropology. My formal fieldwork was completed in mid-1973, and in 1976 I finished writing the thesis. Upon publication of the thesis in book form I became a *persona non grata* and was prohibited from visiting the prison, which I had continued to do from time to time. I considered this period as closed, both on the personal level and intellectually. I turned my attention to new areas of study and did not imagine that both the anthropological issues and the people I had encountered in the prison would continue to occupy me and trouble my peace of mind to this day.

Information that reached me either haphazardly or by design undermined some of my assessments of what had occurred in the prison during my research. Questions were raised, fresh insights came to the fore, and truths that had seemed so clear to me all those years ago became fuzzy and elusive. Is there really one definitive version of this story, and if so, am I able to expose it?

1986

We are on the road from Natanya to the Galilee. It's a long way, the children are on the back seat, and we're searching for something of interest on the car radio. We find the Voice of Israel's second channel, which we don't usually listen to, but this time our ears prick up. It's about Nadia, about Neve Tirza. I turn up the volume and put my ear closer to the speaker.

> This is the strange tale of a woman terrorist who arrived carrying explosives on a mission for the PLO. The story of a terrorist sentenced to 12 years in prison; a terrorist whose interrogators almost fell in love with; and at whose

trial the military prosecutor demanded a harsh sentence and then came to visit her in prison to see how she was. A terrorist prisoner for whom the governess cared like a mother, and who exerted a strong influence on the prison atmosphere and on the other inmates. And who, when she fell ill, was visited by the Commissioner of Prisons who brought her flowers ... a terrorist who wrote letters of encouragement to our soldiers in the midst of war and knitted them balaclavas. This is a story of Israelis who detested terrorists but loved her.

(Thus began the program, an introduction repeated in the following parts, which I was therefore able to record.) It was Edna Pe'er speaking, a well-known radio broadcaster whom I had seen on her visits to Neve Tirza. We hadn't actually met, as I had preferred to remain in the background. But Nadia had told me a lot about her (everything? I'm not sure).

Edna was talking to various people who had known Nadia, asking about their opinion of her, their reminiscences and also inviting them to express their feelings towards her. The prison governess was fulsome in her praise: Nadia was wise, beautiful, had a developed sense of aesthetics, and had been fascinating to talk to. Yes, Nadia had become pro-Israel. Once she had got to know Jews and had met good people here, she had begun to understand Israel's side of the story and had almost become a Zionist. She had no doubts about the transformation that Nadia had undergone.

The director of a show put on at the prison in which Nadia had starred, now a university professor, spoke with emotion of the lovely, intelligent, educated and sensitive Nadia. Edna enquired whether perhaps he had fallen in love with her. Yes, he admitted as much, but it had been an impossible bond. Nadia was honest, refined, truthful, and there had been something deep and spiritual in their relationship.

And then Edna spoke of her ties with Nadia. She had heard of her from the wife of a member of parliament and had come to the prison to meet her. Her curiosity was aroused and she continued to visit Nadia, meeting her in the governess's office. And she gradually became enchanted by her. They discussed everything. Edna introduced Nadia to her husband, who also befriended her and corresponded with her. They 'adopted' her. When Nadia was admitted to *Hadassah* hospital Edna took her for a visit to her home (without informing the guards – this I knew – and the Jerusalem police were called in to find the 'escapee'.)

Nobody was happier than Edna when Nadia was released in 1974 (an early discharge on health grounds). She naturally wanted to keep in touch, but Nadia severed the contact shortly after taking up residence in France. Some years later Edna set out to trace her. The results of this search would be divulged in next week's broadcast.

The following week I was better prepared. At 10:00 I was sitting next to the radio, ready to record the program. This was the interesting and surprising chapter of the saga, both in its content and the way in which it was presented (all told by Edna in the first person). In May 1986 Edna went to France to locate Nadia and learnt that

she was in Morocco. With her journalist's accreditation Edna succeeded in entering the country at a time when Israelis were still not able to visit there.

Her contacts led her to Nadia, who was living with her partner (she had recently divorced her husband, a senior operative in the Hawatmeh Palestinian faction) in Casablanca. Nadia willingly played host to Edna for several days and was pleasant towards her, out of gratitude, as she explained, for Edna's friendship in the past. But Nadia's opinions were very different from those that Edna had known. She had chosen to join Fatah of her own accord, and maintained that Israel had stolen the Arabs' land and had harmed many innocent Lebanese civilians. All those 'Zionist' pronouncements she had made in Israel had been a fake. She hadn't meant a word of all the expressions of love towards the Jewish state.

The astonished Edna asked her: 'Am I to understand that all this time you were lying to us? This was all a charade?' To which Nadia replied: 'When you yearn for freedom you are prepared to do a lot for it, even to lie, to put on a show and to pretend. I respect you as a person and my hostility towards the state of Israel did not prevent me from offering you my hospitality'.

Edna returned to Israel where she met Nadia's admirers and, once they had innocently talked of her (for Edna already knew the end of the story), she divulged the epilogue of her visit to Morocco. Their reactions ranged from surprise to disappointment. I, who hadn't really believed Nadia's Zionist pronouncements, tried to confront the rationale for her pretence. Her words, 'when you yearn for freedom you are prepared to do a lot for it', made a deep impression upon me.

1972–73

Fatma

Fatma was born in East Jerusalem into a family of African origin. She was a trained and practicing nurse. At the time of the Six Day War she was working in a hospital in Qalqilya and moved to Jerusalem during the hostilities. On her return to Qalqilya she found it destroyed and the hospital a pile of rubble. Back in Jerusalem she was consumed by anger and hatred towards the Israelis who had destroyed the town after the fighting had ceased.

Fatma's younger sister, a member of the Fatah movement, mobilized her to plant a bomb in the Zion cinema in central Jerusalem in August 1967. Her sister escaped to Jordan, but Fatma was apprehended. As this was the first act of resistance of its kind she received a harsh sentence of life imprisonment.

Fatma had had no contact with Israelis prior to her incarceration. As she told me later, she thought they were all dark-haired and evil. She was amazed to meet Israelis of a different complexion. Her long stay in detention also led her to differentiate between them, and she admitted that there were some decent people among them.

When I met Fatma she had already spent five years in Neve Tirza and had wearied of prison life. My first encounter with her was emotionally charged for me. I was studying in Jerusalem when she planted the bomb there, and the event had at the time aroused my fear and anger. It was only by chance that a calamity had been avoided; I could well have been there myself. I felt as though I had some personal threatening tie with Fatma.

In 1972 Fatma was one of the longest serving inmates in the prison, perhaps the longest. She had established a special status for herself. Her diligence, decency and honesty (someone defined her as 'oil that floats on the surface of the prison's water') were remarkable and contributed to the esteem in which she was held, mainly by the warders, although some among them did not forgive her for being a terrorist. The governess respected and appreciated her, and she was on particularly good terms with one of the more senior staff members, who supported her in internal struggles. During my time in the prison Fatma was in charge of the kitchen, a position that carried considerable responsibility as well as power. She had gained this appointment owing to the trust put in her. In a place in which food, particularly good food, is scarce, to be in control of the entire food store presents a great temptation to engage in illicit trading or differential distribution dictated by ulterior motives. This did not happen with Fatma. Work in the kitchen is physically more demanding than in other places, particularly on holidays and special occasions. The compensation is one's chance to reap the benefits mentioned above, but Fatma would not do so. After some years at the head of the kitchen Fatma asked to be relieved of her duty (to the governess's great regret), owing to her weariness.

Socially, her position was problematic. She lived in the security wing (inmates lived in two separate wings, one containing Arab women convicted of securityrelated offences, the other housing regular Jewish prisoners as well as non-Jewish 'security' prisoners), but she did not fit into their social environment. Most of these 'security' inmates were well-educated women, some from prosperous and esteemed families. Fatma came from the margins of Palestinian society, and this was evident within the prison. Her family was poor (her father was a sanitary worker in the municipality), of African origin (this was immediately evident from her appearance). She had not been a member of Fatah, and after her prolonged imprisonment did not fully identify with the aims of the organization.

The many years spent in a Jewish environment had made her aware of the complexity of the Arab–Israeli conflict. She had never been cut out for terrorist activity, but as she explained to me, what she had witnessed in Qalqilya had blinded her with rage. The Jewish inmates regarded her as a terrorist. Neither did she seek their company, as most were young women who had led a life of prostitution, theft and violence.

During the initial period of my research I used to work with the inmates in their various jobs, and the place I liked best was the kitchen, with its commotion and bustle, and because I could do something useful there, such as peeling vegetables, mainly potatoes. And there I was at close quarters with Fatma, the head of the kitchen.

Breaking down the distance between us was a slow and gradual process. She did not try to win me over (as did Nadia at one stage), but showed some curiosity towards me. There was a calmness and sincerity about her that appealed to me. Sometimes she would offer me coffee when she made some for the other kitchen workers.

After some time she invited me to her room. As a long-serving inmate she was given a room of her own. It was spotlessly clean, with knitted covers spread around. Family photos stood on the shelf. She told me about her past life and present troubles, and of her dreams for the future.

Her foremost dream, as she told me, was to be released from prison at an age at which she would still be able to marry and have children. Now, aged 34, she feared that she would not make it in time. She was particularly fond of children. She had two foster children, babies who had been abandoned in the hospital at which she worked. Her family was now looking after them, but she knew that they were struggling to make ends meet, and she was tormented by the separation from them, unable to have a proper meeting with them. She submitted an application for early release citing the need to help her family, but this was rejected.

One of the things that troubled her the most was loneliness. She was well aware of being different and had learnt to live with it, but the thorn in her side was her relationship with Nadia. Like many others, she was captivated by Nadia's charms, and thought that they had something in common – the type of offence, and standing out from the general run of inmates. At first Nadia had responded to Fatma's overtures and they had been considered good friends. But Nadia had toyed with her, approaching and then distancing herself from Fatma again and again. Fatma thus at times found herself excluded from Nadia's inner circle. She also doubted whether Nadia was being frank with her and this hurt her considerably. We spoke a good deal about Nadia, with Fatma seeking my assistance in trying to understand her inconsistent behaviour. At one stage Nadia began working in the kitchen, where she exhibited independence and initiative and created new working procedures, sometimes to Fatma's consternation.

I developed strong feelings towards Fatma and used to sit and converse with her not only for research purposes, but because I enjoyed her company and felt that my company was important to her.

After a year and three months I completed my fieldwork, but continued to visit the prison so as to meet Fatma. She took pleasure in observing my expanding pregnant belly. When Noam was born she asked me for a photo. I gave her a recent one of him aged one year. That was our last meeting. Some two weeks later I heard that Fatma had been released and deported to Jordan.

My heart ached at the news. She had requested to remain in the country and feared being transferred to Jordan, where she would be unable to escape the clutches of the Fatah people. And over the years I wondered about two things: had she managed to give birth? Did she keep Noam's photo with her? I have the answer to the first question – she married too late. And about the photo – I can only conjecture that it was lost

or discarded at some point during her arduous and nomadic life, as she moved from Lebanon to Tunisia and then on to Gaza.

Nadia

What was it that attracted people to Nadia like bees to a flower? Just about everyone she approached and smiled at became a groupie of hers. Was it her looks? Her wisdom? Her pleasant disposition? Her rolling laughter? Her ability to show great interest in you and in your life? The capacity to gently compliment you, so that even if there was an element of flattery you accepted it as a genuine compliment? The good taste evident in all that she wore, even the prison uniform (from which she succeeded in ridding herself)? Her ability to speak to everyone at their own level, while expressing empathy with their distress? All of these and more.

In my thesis I analysed Nadia's power relations with warders and inmates and the friendly relationships that she developed with some of them. This was but a very partial picture.

Nadia was born in Morocco into a wealthy and well-established family. Her father died when she was 19, throwing the family into financial turmoil. Nadia moved to Paris with her younger sister (for whom she assumed responsibility), and studied philosophy at the Sorbonne. It is unclear whether these were regular studies, and she did not complete a degree. As an intelligent and beautiful young woman she became a sought-after society lady. Her last romantic attachment before the mission to Israel was with the French Jewish singer George Moustaki, with whom she lived. Without his knowledge she was also in liaison with active Fatah members in Paris, and it was they who in 1970 sent her and her sister equipped with explosive devices to carry out an attack in Israel. They were both apprehended at the airport and sentenced to long prison terms (Nadia to twelve years and Marlene to eight years). Moustaki learnt of Nadia's escapades from the papers, and in his amazement and anger severed all contact with her for a long time. But love still burned in him and he eventually came to visit her, but more on this below.

When I arrived at the prison in early 1972 Nadia, her sister and the other two French women sent with them were living in the wing housing ordinary Jewish inmates, despite their 'security' classification (Arab inmates convicted of securityrelated offences were housed in a separate wing). Nadia had quite soon succeeded in convincing the authorities that she repented her actions, that she had not exactly understood the political issues, that she had acted out of ignorance and wished to have nothing more to do with those involved in terrorism.

When I first met her Nadia already spoke fluent Hebrew, and while she was well regarded by some of the staff and inmates, she was defined as a terrorist by others. A year and a half later she was an undisputed leader. Most of the prisoners worshipped her. A small group of devotees followed her unreservedly. She had unlimited access to the governess and enjoyed privileges that other inmates could but dream of. One

can clearly point to the events that enabled Nadia to express her talents, exploit the situations that arose to enhance her standing, and chiefly to bring forward the day of her release.

A performance put on by the inmates, to which a number of outside guests were invited, constituted the turning point. Nadia played the central role, acting with considerable talent. But what really brought her acclaim was her surprising finale, rendering a song of her own composition – both music and lyrics – titled 'the governess' this was a song of praise. The governess was ecstatic, along with several of the guests.

The next important event was the visit of Moustaki, who came specially to see Nadia. He was allowed to visit Nadia every day during his week's stay. The visits took place in the governess's office. A performance of his in the prison yard for invited guests brought his visit to a close. A prominent number added to the repertoire was Nadia's 'Song of Peace'. I must return here for a moment to Edna Pe'er's program, in which she quotes an earlier recording made in the prison for public broadcast, during which she asks Nadia to sing the 'Song of Peace' which she had written. Nadia refuses, explaining that were someone who had been injured during the War of Independence or perhaps in 1967 now to hear the song of peace sung by a terrorist, he would surely relive his pain. 'I can't do it ... I'm not speaking here to someone who knows me ... what would people think when they hear me singing about peace? They would surely think that now it's easy for me to say whatever I like, that I feel sorry ....' And is there a listener who would question the sincerity of her regret, I wondered.

All this time Nadia continued to study Hebrew and topics concerning Israeli history and geography. She was preparing to participate in a national quiz for prisoners on the subject.

The wife of a member of parliament, who heard about Nadia, came to make her acquaintance and subsequently 'adopted' her, making frequent visits held in the governess's office. It was during this period that Edna Pe'er met her and developed a close relationship with her. And as she freely admits in her broadcast in 1986, she did much to facilitate Nadia's early release from prison.

Nadia was now by no means a regular prisoner. She wore her own clothes and her room was filled with her private possessions: books, cosmetics, French fashion journals which she passed on to interested warders. She received food packages, which were generally opened in the office and not subjected to standard censorship procedures. The governess's office was open to her and Nadia passed on information to her, as she defined it to me, 'I'm not an informer, but there are things that she needs to know, if they cause harm to an inmate ...'

Her new status naturally gave rise to jealousy and hostility among many. Inmates who were excluded from her inner circle spoke angrily of her. In their opinion she was only pretending to be pro-Israel. They were incensed by the privileges granted to her.

Certain warders complained of her haughty attitude and disregard towards them 'because she has the governess's backing'. A few suspected her motives. As far as the prison management was concerned Nadia and her French accomplices were no

longer regarded as 'security' prisoners. They had repented, and so when exceptional authorization was given for a day trip to Jerusalem for all the regular inmates, Nadia and her fellow French prisoners were included. Not all members of staff viewed this favourably.

Nadia encouraged a closeness with Fatma on the one hand and kept her distance on the other. In the beginning they were good friends. But once Nadia was surrounded by Jewish inmates (in addition to the other French women) and had fostered connections with senior staff members she had less time and less need for Fatma. When she began to work in the kitchen and initiate her own working procedures this led to outright confrontations. Nadia respected Fatma's honesty, but she was not really a partner for conversation. She – Fatma – was too ordinary for her, she didn't belong to the guitar group, and Fatma felt hurt. She still admired Nadia but was more reserved towards her.

For my part, I tried to remain detached. Nadia tested me. We had things in common – we were the same age and the only ones with an academic education. We could discuss literature with each other. She tried to teach me to use make-up, to cover my head with a scarf, in her own appealing manner. Sometimes she would tell me 'secrets', and I suspected that she was trying to win me over. I was careful, but was once almost taken in by her. She spoke sorrowfully, about the future and her longing for freedom. I felt that I was being drawn by her magnetic personality. I understood her, identified with her. Suddenly I shook loose: perhaps this was all a sophisticated charade, aimed at adding me to her coterie of admirers? When she tried to persuade me to pass on a letter to the director of the show (so as to conceal intimate material from the censor) and I refused, she became distant, as if she had decided it was not worth her while to invest in me. I was tacitly critical of her, judgmental of her flattery, pretences and manipulative behaviour.

When, after completing my fieldwork, I heard that Nadia was ill, I attributed this to the array of techniques designed to hasten her release. But her illness was indeed serious and she was hospitalized in Hadassah in Jerusalem. It is interesting to quote the doctor who treated her there, as conveyed by Edna Pe'er in her broadcast.

> she was suffering from a serious disruption of the blood flow to her hands. This manifested itself in the greyish blue colour of her hands, which also perspired. We found no illness in the background that could explain this. The illness was caused by exposure to the cold in the prison, and certainly had a psychosomatic element. During the month that she was hospitalized I learnt of her background, mainly from your (Edna's) stories and hers. For me, as a doctor representing the hospital, she was a terrorist ... I was compassionate towards her, I was particularly surprised by the change in her attitude towards Israel and the Zionist enterprise. When she was admitted in the winter of 1974, a few months after the Yom Kippur war, she told me how excited she had been during the war, and had written a

song about the war. She gave the impression that she was just the opposite of what I had imagined her to be, as a terrorist. At the time I forgot that she was a terrorist. Nadia was one of us ...

As mentioned, she was released in 1974 on grounds of ill health and was flown to France. The other French women also had their sentences commuted after a while, and disappeared.

1995

For some time now I have toyed with the idea of contacting Fatma, after reading about her in the paper. For many years after her release and deportation to Jordan I had heard nothing of her. And then one day there was a report in the paper on some international gathering in Switzerland which Fatma had attended as a representative of Fatah. I learnt from this that what she had dreaded had indeed occurred: she had not succeeded in evading the embrace of the organization. The same report mentioned that she was living in Tunisia.

More years passed by. In 1994, following the Oslo agreements between Israel and the PLO, the Fatah command moved from Tunis to Gaza. The newspaper report listed various functionaries among the arrivals, including Fatma Barnawi, commander of the women's police force. My memories of her were rekindled and I felt happy; perhaps we would be able to meet?

Time went by. Then one day I took the initiative and found a phone number in Gaza. Avner, my husband, called, explained, was met with a very polite response and given the number of Fatma's office. I called with a thumping heart, and presented myself as Dr Ofra Greenberg, hoping that this would help to be taken seriously. The next day I received a call from Fatma. She sounded like a complete stranger. 'Hi Fatma, it's Ofra, the one who was in Neve Tirza with you'. She's sorry, but she doesn't recognize me. She called without knowing who I was. (Later, when she called again, she explained that she had taken me for a medical doctor enquiring about the Israeli prisoners whom she had cared for in Lebanon. That's why she had responded, since she generally didn't want anything to do with Israelis.)

I try to describe myself to her, to remind her of the relationship between us, but to no avail. 'I have deleted the time in prison from my memory. It was a bad period and later things were even worse in Lebanon. I remember nothing of the prison'. In desperation I keep trying, mentioning names of those who were with us at the time, to arouse her memory. 'Fatma, I was there at the time of Nadia'. And she: 'Don't you know? Nadia is dead'. My heart sank. I was shocked. Agitated, I ask how she had heard the news, when and where did it happen? Fatma explains that the illness she had contracted while in prison had never been fully cured, and she had died very recently. I offer to send Fatma a photo of the two of us in the prison, to remind her, but she

doesn't sound very interested. The conversation ended with me feeling frustrated and confused.

A few minutes later the phone rang again. 'I remember you'. Now she too sounded excited. She had felt the sorrow in my voice upon hearing of Nadia's death. She understood that I really did care. And then the curtain had lifted and she remembered events from the prison. She remembered me, and as proof she recounted the content of a conversation we'd had while sitting near the gate, about the significance of the flight of the birds beyond the prison walls.

We talk at length, about our lives, mainly about Fatma's. She speaks about her time in Lebanon during the war, the consequences of which had fuelled her animosity towards Israel. She sounds almost 'brainwashed' regarding her negative view of Israel's motives and actions.

I raise the idea of a meeting, wanting to see her so badly. She explains that she can't and doesn't want to enter Israel. She proposes meeting in Gaza and passes me on to her husband to discuss the practicalities. He is a native of Acre who has spent time in Israeli prisons on account of security-related activity and had also been deported. He is exceedingly friendly on the phone, shows far more interest than had Fatma in what Israelis were thinking and asks about my opinion of the chances of peace. He would be happy to invite us (my husband and I). If we let him know when we intend to come he will take care of everything once we pass the border crossing. I promised to take up the offer, and didn't keep the promise. I was afraid to go to Gaza. Avner was in favour, as was the 21 year-old Noam, but I was simply afraid. I didn't call again and still regret it.

Epilogue – 2007

On occasion, when I feel particularly attached to a class and the subject matter is relevant, I tell the students briefly the story of Nadia and Fatma, presenting the 'anthropological' conclusions: you can never tell when the research will actually come to an end; you can't accurately trace the boundaries of the research field. As far as I was concerned my research had ended with my last conversation with Fatma. As if things had been clarified. As if we had reached the end of the story, twenty-two years after leaving the research field, the prison. That is what I thought until recently.

I propose writing an article on the subject, and to my surprise the topic is accepted. I sit down to write (still using pen and paper), and my hand can't keep up with the flow of words. As though a dam had burst, the memories wash over me, as though they had long-awaited this moment. I pick through the low-grade recordings I had made of the Neve Tirza vocalists, led by Nadia. Among them I find the partial recording of Edna Pe'er's program. I listen to it again from a distance of so many years, and things appear differently.

Until now I had had a clear picture of Nadia – motivated by self-interest, manipulative, succeeding in duping everyone else in her quest for freedom, without changing her political opinions in the slightest. And now, at this distant point in time, listening attentively to the recording I discover a more complex, even engaging, figure.

After her release Nadia had returned to Paris, to Moustaki, where she awaited the release of her sister Marlene. The relationship with Moustaki had soured and she had to move out. She found refuge with a pair of friends, expressing her gratitude by doing the housework. During that time she kept up her correspondence with the governess and with Edna, but stopped writing once Marlene had been released. Marlene didn't get on with their hosts and they moved into a hotel. They had no money as Nadia couldn't work owing to her illness. They couldn't return to Morocco, as she was told that she would be put on trial for her terrorist involvement. Her Moroccan passport was invalidated. The only ones who were willing and able to assist were her old friends from Fatah. They sent her for treatment and an operation to the USSR. Following a brief stay in Italy she moved with her sister to Lebanon, where she married a senior functionary in the radical Hawatmeh faction. The Lebanese war caught her in Beirut. She tells Edna at length of the devastation she had witnessed in Lebanon in 1982, the result of attacks by the Israeli air force. She explains her animosity towards Israel for its indiscriminate attacks on civilians. But she knows there are also good Israelis. She admires the 'Peace Now' movement and 'Yesh Gvul'. The similarity of her and Fatma's fate suddenly dawns on me; both are victims of family and political circumstances, both were held hostage by a terrorist/liberation organization.

It transpires that for both Nadia and Fatma the Lebanese war constituted a formative experience. Fatma refuses to meet Israelis following those events. The confrontational ideology sprung from there, and blurred the conclusions drawn from their time in prison. In my current 'reunion' with them the prison constitutes but the beginning of the road. I also had some experience of that awful war (now called the first Lebanese war). At the time we were living in Qiryat Shmona and suffered from the Katyusha rockets. The war drove me and my small children away when our house was hit. Even then I had rubbed shoulders with Fatma and Nadia without knowing it.

During her meeting with Edna, Nadia did her utmost to avoid political discussions. She was tactful and courteous. But Edna the interviewer persisted in raising contentious issues. Nadia explains that she was ill and only wanted a peaceful life. She had recently divorced and is now working in journalism, editing a periodical. She has regained her inheritance after prolonged litigation. She is a frequent visitor to the palace.

With genuine interest she asks Edna about her prison friends, not only about the governess but also about her fellow inmates. She thirsts for information about them. She treats Edna with affection and sensitivity.

The complex character that emerges from the description of Nadia's behaviour towards Edna in Morocco illuminates the past in a different light. Perhaps then too not everything was merely a charade, and there were layers of sincerity, concern, affection,

and expression of genuine attitudes? And there is something more. Upon listening to the recording again I take note of what Edna says about Nadia in Morocco, that she steadfastly refused to allow Edna to record her, responding to her provocative and unrelenting questioning with the words 'Don't you understand that we are not in a democratic country here?!' And I get the notion that here too Nadia has to refrain from revealing her true opinions, and has to put on an act. So the 'truth' that Edna brings back with her from Morocco is also just part of the truth.

I suddenly feel that not only had I failed to really get to know Nadia, but that I had missed out on something. Perhaps I could have shown more understanding, back in those distant days, had I been less hesitant, had I allowed myself to let go. Perhaps.

And the regret at squandering the opportunity of meeting Fatma again hits me with full force. My research has not been completed after all. I still need to fill in many blanks. I need to meet Fatma. Will I succeed? Will this story ever have an end? Is it at all possible to bring a research narrative to a natural conclusion?

Index

Page references to Tables and Figures are in italic type. References to footnotes consist of the page number followed by the letter 'n' followed by the number of the footnote, e.g. 4n3 refers to footnote no.3 on page 4.

audiences/readerships 73–4, 94, 105 and colonialism 2, 7, 58, 71, 119 fieldwork issues 1–4 interdisciplinarity 15–16, 104, 175–6, 183, 223 military studies 75 scientific status of 3, 16–17, 110 *see also* academia; appointment anthropology; approaches/theories; auto-anthropology; autoetnography; critical anthropology; cultural criminology; development anthropology; home anthropology; kibbutz studies; medical anthropology; participantobservation; social science Apffel-Marglin, Frederique 58 appointment anthropology 188–9 approaches/theories abductive reasoning 2, 18, 19–20, 28 actualism 26, 28 contrast-oriented approach 76

see also comparative studies/

analysis

deconstructivism 74, 224 developist approach 24n7 dynamist approach 21 evidential approach 18–19, 28 evolutionism 111, 112–13 functionalism 47, 58, 97, 105 humanism 200 interpretive approach 17n2 *see also* Weber, Max

liberalism 200, 202–5, 206 Manchester School 21 Marxism 21, 58, 94, 95 objectivism 17n2, 28, 135–6 orientalism 111, 120

see also Said, Edward

participatory action research 104 postmodernism 2, 3, 17n3, 47, 56, 141 *see also* Baudrillard, Jean;

Bauman, Zygmunt; Bourdieu,

Pierre; simulations socialism 200, 202, 204 structuralism 23, 141

see also Lévi-Strauss, Claude

uniformism 26

see also actualism universalism 200, 203 utopistics 95 *see also* anthropology; discovery; participant-observation; serendipity

ar-Rmal (Shammar Bedouin) 22–8

Arab Human Development Report (AHDR 2003) 222 Arabia Bedouin Ahl Murra tribe 21 ar-Rmal (Shammar) tribe 22–8

Arabs *see* Israeli Palestinians; Negev

Bedouin; Palestinians; Rwala

Bedouin (Syria); Sinai Bedouin Arafat, Yassir 302–3, 304 al-'Arif, 'Arif 260–61, 269n32, 269n33, 269n34, 270n39, 273n54 Aristotle 157, 231

Aron, Raymond 218 Aronoff, Myron 292 art smuggling 182 *see also* criminology

Asad, Talal 24n7, 58

Ashkenazi 117, 296

Association for the Defence of Bedouin

Rights in Israel 132 attached groups *267*, 280, 284–7

see also co-liable groups; core-groups;

Blanc, Haim 119
Blickman, Tom 182
Bloch, Marc 216–17
Boddy, Janice 141, 142
Bogard, William 47, 58
Boilley, Pierre 141n1, 144
Bonnenfant, Paul 21
Borg, Alexander 254
Bourdieu, Pierre 93, 103, 104, 105, 126, 135, 191, 218–19, 224 *see also* postmodernism
Bovenkerk, Frank 183 Bowen, John 58 boycotts, academic 214–15 Braidotti, Rosi 1
Bremen, Jan van 71
Brenneis, Don 221
Brettel, Caroline B. 73
Britain academic boycott of Israeli scholars 214–15 *see also* England Brown, Kenneth 221 *brujos see* Lacén, Tonio; witch-healers
Buber, Martin 93
Bucaille, Laetitia 220
Buck-Morss, Susan 2
Buckley, Anthony D. 254 Burckhardt, Johann Ludwig 35 burials *see* grave vandalism; memorials (Israeli); memorials (Palestinian); military cemetery; Mount Herzl cemetery; reburials
Burkina Faso 144
Burns, James M. 103
Cabrera, Lydia 160n6
Callaway, Helen 186
Canaan, T. 263n19
Carmon, Naomi 48
Carrithers, Michael 47
Carroll, Lewis 8
Casagrande, Joseph B. 119
Casajus, Dominique 146
Catholicism 158–9, 161, 162, 166–7, 169
'cave-dwellers' of Tripolitania (Libya) 110–14, 115, 118 cemeteries *see* military cemetery (Israel);
Mount Herzl cemetery ceremonies *see* reburials; rituals
Certeau, Michel de 134, 135, 157n2
Chambliss, William 176–7
Chamboredon, Hélène 126
Charlick, Robert 144
Châtelet, François 232
Chechnya 214 Chelala, Selina 144 childhood education *see* Japanese day-care centre Childs, Larry 144 cities

Tripolitania (Libya)

Gilbert, Margaret 267n30 Ginzburg, Carlo 18–19, 161, 163 global guesthood 199–211 globalism 205, 206–7, 222–3 Gluckman, Max 176

Goethe, Johann W. von 157

Goldberg, Harvey E. 6, 109–20

Good, Byron 141, 142, 143

Goodman, Roger 124 Gorbachev, Mikhail 181 grave vandalism 299–300, 302, 304

see also memorials (Israeli); memorials

(Palestinian); military cemetery; Mount Herzl cemetery; reburials

Gray, John 206 Greenberg, Ofra 9, 307–18 guesthood *see* global guesthood

Gundaker, Grey 167n9

Gupta, Akhil 74

Ha-Cohen, Mordecai 111, 114–19, 120–21

Ha-Cohen, Yehudah 116, 120

Haaretz (newspaper) 299 Habermas, Jürgen 58

Hail (Saudi Arabia) 22–3

Halbwachs, Maurice 231

Hall, Stuart 206

Hamas 301

Hamm, Mark 176

Hammoudi, Abdellah 220

Handelman, Don 294 Handelman, Leah 294 haredim (ultra-orthodox Jews) 299–301 Harvey, David 2

Hawad, Mahmoudan 145, 152, 153

Hawatmeh Palestinian faction 309, 317

Hayward, Keith 183 Hazan, Haim 1–9, 47–59, 86, 102 healing 143, 247, 252, 253–4

see also herbalists; plant medicine;

spirit possession exorcism; witch-healers

Hebrew University of Jerusalem, The 97 *see also* Machon Ben-Zvi

Hegel 199, 200, 201

Heifetz, Ronald A. 103

Heine, Heinrich 201

Henein, Nessim Henry 37

Henkin, Roni 259 Henninger, Joseph 35 herbalists 143, 148

see also plant medicine

heritage politics 110, 116–19 Hertzog, Esther 1–9, 229–42

Herzfeld, Michael 187

Herzl, Theodore 293–4, 295, 299, 300, 303, 304 *see also* Zionism

What I was in a position to present is in any case an analysis of the processes of social differentiation which the attribution of land produced within the Bedouin community. By combining an analysis of the social structure with that of a pastoral economy increasingly subjected to market dynamics, I attempted to show how the processes of differentiation (and of probable social stratification tending towards a structural stability) were the product of factors such as the attribution of land, the market, political relations which fractions of tribes maintained both among themselves and with the central authorities; but also how factors of individual choice, strategies of risk differentiation and of social prestige contributed to articulate the whole process. These analyses were not included in a detailed manner in my book on the Shammar (Fabietti 1984), but in a series of articles which I published later.

A further sphere of reflection offered to me by my research on the Bedouin was the vexata quaestio of the structurally 'sub-productive' nature of the domestic Bedouin group, and therefore of the policies of intervention in the nomadic sector. These were however reflections that I conducted a few years after having concluded my research. The starting point came to me from the celebrated work of Marshall Sahlins on the sub-productive nature of the domestic group, in which I came across once again, several years after first reading it, quite a while after the end of my research in Arabia. I criticized the 'developist' approaches to the Bedouin reality not certainly through any attachment to an image of the cultures studied by anthropologists as something that should be preserved in its 'authenticity'. This idea, in itself naïve in the social sciences, would be still more bizarre if referred to the nomads of Arabia. Considering their

capacity to adapt to ever new situations to which, for thousands of years, they have had to face (from the birth of the urban phenomenon to writing, from the 'hydraulic' states to the rise of Islamic religion, from the advent of western technology to the oil boom of the 1960s and to the subsequent regional crises), it could be said, by reversing the title of a celebrated book by Bruno Latour, that the nomads of the Middle Eastern regions 'have always been modern'.

My criticism contemplated first of all a redefinition of the domestic Bedouin group released from excessively 'parentological' images elaborated in the past. In this way I was trying to reproblematize the subject in the context of the question both of development and on development, and to show how the central issue was that of the hegemony which the 'stronger' languages (those of the agents of development) exercise upon those of the people affected by the development plans, and above all in the language (and in the minds) of the clients of the projects themselves (in this case the Saudi authorities). On this point I owed much to the well-known essay by Talal Asad (whom I met for the first time in Arabia) on translation in the tradition of British social anthropology, a work that came out several years after the end of my research (Asad 1986); but also to the works on the domestic community by Claude Meillassoux with whom I had mixed assiduously in Paris in the previous years (Meillassoux 1975).

[...] "Becoming a bird" is the passage from life to death.'

www.thetedkarchive.com