Myron J. ARONOFF

FORTY YEARS

AS A POLITICAL ETHNOGRAPHER

I, on my side, require of every writer, first or last, a simple and sincere account of his own life, and not merely what he has heard of other men’s lives; some such account as he would send to his kindred from a distant land; for if he has lived sincerely, it must have been in a distant land to me. (Henry David Thoreau, *Walden*, 1854)

I choose the autobiographical approach in this discussion of political ethnography for several reasons. First, I know my own work best and do not presume other’s familiarity with my publications beyond specialists in

* An earlier draft was presented as the keynote address on October 26, 2006 at a workshop on “Political Ethnography: What Insider Perspectives Contribute to the Study of Power” held at the University of Toronto. All further references will be cited as *op. cit.*, workshop on Political Ethnography. I thank Edward Schatz for inviting me to give the address and for his helpful comments on it. I am grateful to my fellow participants for a most stimulating exchange of experiences and ideas. I am indebted to Marina Mogilner and Alexander Semynov for soliciting this essay for publication and for their probing comments and questions.

my fields. This approach, therefore, affords an opportunity to broaden awareness of the fruits of four decades of my own ethnographic research while discussing a number of important general problems and issues. Second, I hope that young scholars at the outset of their careers may benefit from my experiences so they do not constantly attempt to reinvent the same wheel. Finally, my self-referential approach introduces the self-reflexivity that presently dominates in anthropology to scholars in other disciplines. I shall illustrate, for example, how the unintended consequences of choices I made influenced my career, my work, and my life.

I have been fascinated by politics for as long as I can remember. I was the only kid in Middletown, Ohio in 1952 proudly sporting an Adlai Stevenson campaign button. My liberal Democratic family was likely considered by most of our neighbors in the bible belt of southwestern Ohio to be communist. My fascination with other cultures began while working a summer in Israel and traveling through Europe during the Fall of 1960. I discovered ethnography in graduate school at UCLA (1962-1965). As a political science major with an area concentration in African studies, I was obliged to choose an additional major “outside” of political science. Anthropology was a natural choice for understanding the postcolonial politics of nation building and identity formation in Africa. These developments were part of a general redefinition of the field of political science that began after WWII and received greater impetus in the 1960s with the independence of the new African states.

Among the outstanding scholars with whom I studied the political theorist (philosopher) David C. Rapaport and the anthropologist Michael G. Smith had the greatest intellectual influences on me. By studying classical and more contemporary political theory with Rapoport I learned to ask important questions – particularly about the nature of political legitimacy, which has remained the central conceptual focus throughout my academic career. Smith introduced me to ethnography in his course on traditional political systems. I delved more deeply into the nature of legitimacy in his seminar on Max Weber. I decided that I must do ethnographic field work for my doctoral dissertation because I felt that the only way I could understand the meaning of politics was to observe the people involved in the processes I wanted to study and learn how they understood what was going on. Nation building was the hot topic at the time for Africa. However, for reasons beyond my control I was unable to do the fieldwork I had planned in Africa. As an ABD (all but dissertation) I turned down an attractive, well paid tenure-track job offer at a respected university in the United States in order to accept a very poorly paid position on a research team from Manchester University (UK) directed by Professor Max Gluckman to conduct fieldwork in Israel. In other words, I chose the opportunity to conduct ethnographic fieldwork over my fascination with Africa and over a decent salary and the promise of potential job security. I was bitten by the ethnographic bug and have remained infected ever since. As I shall elaborate below, once you have the opportunity to observe and interact with people who are engaged in the activities that fascinate you and that you are attempting to understand, you realize that there is simply no better way to understand what is going on, and no other way to understand what these events mean to the participants themselves, than through participant observation.

Strangely enough there were no courses offered, nor was there any formal training in ethnographic methods in the department of social anthropology at Manchester University in 1965. We picked up informal tips from gossip about famous anthropologists in the field and personal anecdotes in the common room and in the pubs to which we retired after our seminars. For example, we learned about extended-case analysis by reading the classic formulations by Max Gluckman and by J. Clyde Mitchell. The (in)famous Manchester seminars when classes were called off for intensive critiques by professors and graduate students of the work of those just returning from the field was a baptism under fire through which we became initiated in the Manchester “school” approach. Max’s only direct methodological advice to me as I set out for Israel was to “keep your eyes and ears open to the daily life of the people you are studying.”

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I was asked on my oral comprehensive Ph.D. exam at UCLA. “Is political science a science or an art” to which I immediately replied, “If we are to succeed in understanding people and politics, it must combine both.”


open and your mouth shut tight.” The former was easier than the latter for
me. The only stricture he placed on us was that we were required to study a
community small enough to employ participant observation as our primary
research method.

Although I received an excellent education at Manchester, training in
ethnographic methodology was not the only gap. Most of my professors
had worked with Gluckman at the Rhodes-Livingstone Institute of Social
Studies in central Africa and were African specialists. Emrys Peters taught
the only seminar dealing with Middle Eastern cultures. (Peter Worsely taught
a more general “third world” seminar.) I have never taken a course at the
undergraduate or graduate level that dealt with Israel even in passing. Also,
like my British trained professors of anthropology at UCLA, M. G. Smith
and Hilda Kuper, my professors at Manchester were all British social an-
thropologists. We studied social structure and networks, not culture. In some
ways this was closer to the political science I studied than is the work of
Clifford Geertz and other American cultural anthropologists who I read
outside my formal education. Whereas the methodological innovations of
extended case analysis, particularly of protracted political strife, developed
by the Manchester school are highly relevant for political scientists, I shall
suggest below the cultural focus on the semiotic and hermeneutic analysis of
the interpretation of meaning is the most important contribution of
American cultural anthropology to understanding politics.

I chose to study one of the two newest of Israel’s thirty development
towns that had been recently established in the Negev desert. Two sociolo-
gy students had conducted surveys for their masters’ theses in town so the
residents were familiar with what sociologists do. I explained that I was a
political anthropologist doing an ethnographic study. It later became ap-
parent that not everyone understood what ethnography involved. Many
thought I was just a lazy sociologist and asked when I was going to conduct
my interviews. Others bluntly suggested I get a job. One local recent immi-
grant who was serving in the border police manned a check point on the
border between the West Bank and the pre-1967 war border. When I ar-
rived at his check point he excitedly called his colleagues over to introduce
me as an “American astronaut” living in town.

The leader of the opposition who was elected mayor during my study
was shocked when he read a copy of my dissertation saying he had no idea
it would be so personal. He pointed out a dissertation on local government
in Israel on his desk written by a political scientist which he thought was
the kind of work I was writing. He objected that my study was so personal
that publishing it would be like publishing an x-ray of his ample stomach. He
was the son-in-law of the prime minister at the time and had higher
political ambitions. In fact, he eventually became finance minister.

I lived with my wife and infant daughter in town, participating in the
life of the community from October 1966 through the summer of 1968
(including the war of June 1967). Toward the end of my stay I conducted a
survey to test a hypothesis developed from my observations and to prove
not only that I was not a lazy sociologist, but that I was a competent political
scientist. After months of getting data that made no sense based on my
intimate knowledge of the population, I discovered that the magnetic tape
had broken and a piece of someone else’s data had been accidentally spliced
into mine. Had I not known the population as well as I did, under the pres-
ture to complete my dissertation, I might have been forced to attempt to
make an interpretation of spurious data. On the other hand, the multivariate
regressions I ran once the problem had been corrected corroborated the
central hypothesis of my analysis derived from the ethnography: the con-
struction of a strong collective identity and sense of communal pride within
a remarkably short time was due primarily to the mobilization of the resi-
dents through competing local socio-political factions. Whereas I certainly
agree with Ed Schatz that one need not utilize multiple-methods in all re-
search, there are definitely contexts when they are not only useful, but per-
haps even essential.

My analysis of Frontiertown was framed in the context of Victor Turn-
er’s “political phase development” in which social situations were presented
as phases in an ongoing process of political strife over an extended period
of time. Each phase was analyzed using the method developed by the

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6 Gluckman attempted to replicate the spirit of the Rhodes-Livingstone Institute in the
team he assembled to study Israel that was funded by the Bernstein family (owners of
Granada television in the United Kingdom).

7 I negotiated with him and agreed to delete a few of the most “personal” matters which
did not detract from my analysis. He finally consented to the publication of my disserta-
tion. The town and its inhabitants were all given pseudonyms in the tradition of anthro-
pology.

8 Edward Schatz. The Problem with the Toolbox Metaphor: Ethnography and the Limits
to Multiple-Methods Research. A paper presented at the annual meeting of the Ameri-
can Political Science Association, August 31-September 3, 2006. A similar argument is
Political Ethnography.

Manchester & Jerusalem, 1974; Victor Turner. Social Dramas and Ritual Metaphors //
Manchester school known as the extended case method and situational analysis. One case constituted what Turner termed the deployment of “ad-
justive” or “redressive” mechanisms. I analyzed the ritual interaction be-
tween representatives of local merchants and housewives employing Erving Goffman’s *Encounters*, which analyzed the ritual nature of face-to-face in-
teractions. A confrontation over economic issues on the eve of a hotly contested local election in which violence had been threatened was defused by the skillful employment of framing through what Goffman metaphorically termed an “interaction membrane” that excluded direct reference to politics and disguised references to ethnicity.

The encounter, which began with considerable tension, ended in good humored laughter prompted by a joking exchange between the unofficial leader of the housewives and the head of the merchants’ association. Coincidentally, they were the only two people present who were of Middle Eastern background. The housewife, who was from Yemen, joked about the incongruity between her dark complexion and her European (married) name. She also called the leader of the merchants, who was from Morocco originally, *habibi* using the Arabic pronunciation rather than the common pronunciation used by Israelis of European background. I suggested that the use of the Arabic term, rather than the Hebrew equivalent, in this context was a subtle reference to their common ethnicity after the two had confronted each other over economic issues. It successfully brought the encounter to a conclusion because of the relative absence of ethnic prejudice and tensions among the participants.

When I gave my presentation back at Manchester, Professor Emrys Peters, who had worked among the Bedouin in Libya and in a Lebanese vil-
age, insisted that there was a sexual innuendo in ... of my fellow graduate students was engaged in a conversation with a stranger in the booth next to ours. The stranger asked

Dramas, Fields, and Metaphors. Ithaca, NY., 1974. Pp. 23-59). The model was formulated earlier in the introduction to: Marc J. Swartz, Victor W. Turner, and Arthur Tuden (Eds.). Political Anthropology. Chicago, 1966. His co-editors credit Turner for the major contribution in formulating the approach. Turner was one of Gluckman’s most prominent students. He moved to the United States where he had a significant impact on American anthropology as well as British anthropology.


issues. I was able to add conceptual clarity to this discussion and empirical evidence through my analysis of the suppression of extremely important and controversial issues from the national convention of the party. Scholars dependent upon archival evidence and interviews were completely unaware of this phenomenon which never appeared in previous studies of this party or any other. However, I feel that my greatest theoretical contribution in this study is to the analysis of ritual, the refinement of Gluckman’s notion of rituals of rebellion, and the conceptual challenge to the predominant reified, mutually exclusive, dichotomous distinction between traditional and modern societies.

I had not planned to study ritual in my research design. But after exhausting the explanations for much of my data there remained a significant range of activity, particularly in one closed top party forum, which defied explanation by the aforementioned concepts. The more I examined the symbolic dimension of behavior in this assemblage of the secondary echelon of national party leaders, the more I was reminded of Gluckman’s classic essay “Rituals of Rebellion in South-East Africa” (1952). It met Gluckman’s key criteria that the outcome was known in advance and that the social unit must end united as a consequence of the ritual. The rebellious criticism by the secondary leaders of their patrons in the top party elite was strikingly similar to that of the Lozi priests of Barotseland analyzed by Gluckman and to the chiefs designated by the king of Baganda reported by Lucy Mair. Yet, Gluckman argued quite explicitly that with the development of proto-classes you cannot have rituals of rebellion because when actors can opt for alternative social roles you get genuine revolts rather than ritualized rebellions. By explicitly delineating the conditions that prevented the actors in my study from opting for alternative roles (e.g. overthrowing the top leaders or switching parties), I eventually convinced Gluckman that what I observed was, indeed, a ritual of rebellion. By showing the limited scope and efficacy of such ritualized “solutions” and the suppression of issues that were highly salient to the public I was able to document Labor’s loss of ideological dominance and legitimacy and to anticipate its loss of political dominance in the forthcoming election. I note that no other political scientist and only one (little known at the time) pollster predicted the defeat of Labor in 1977. If I had not managed to observe the events analyzed I would not have been able to make either this theoretical contribution or the successful prognosis. In 1993 I published a substantially expanded and updated edition of this book dealing with Labor’s years in opposition and eventual return to power.

My third major research project (which resulted in Israeli Visions and Divisions) was even more unconventional since it was an ethnography of Israeli society, culture, and politics in the period from 1977 to 1990, which was a period of major cultural and political transformation and polarization. Based largely on fieldwork in Israel during 1982-1983 and 1987-1988, I utilized a wide range of methods. I engaged in participant observation of selected meetings of the Ministerial Committee on Symbols and Ceremonies, the Knesset plenary, parliamentary committees, and the delegates dining room, the activities of several peace movements (particularly Peace Now), the major settlers movement (Gush Emunim or Bloc of the Faithful), academic conferences, theater performances, movies, television programs, e.g., a documentary series on the 1981 election campaign, and the first Palestinian uprising (intifada). I interviewed more than a hundred political, religious, cultural, and educational leaders. I also examined an archive of more than twenty years of meetings of the Ministerial Committee on Symbols and Ceremonies (housed in the Prime Minister’s office), from which I selected for analysis two major decisions that focused on the manipulation of political culture.

The leader of the nationalist Likud party, Menachem Begin, became prime minister in 1977 and set out to overcome the pariah image with which Labor had stigmatized him and his movement. He attempted to eradicate the last vestiges of Labor’s ideological legitimacy and to establish the Likud’s political dominance and ideological hegemony. Begin utilized state agencies to reinterpret Israeli history; to elevate his movement’s ideological leader, Vladimir Jabotinsky, to the national political pantheon; to enshrine as heroes the martyrs of the dissident underground movements – particularly the one he commanded; and to establish the authority of their myths. The Begin government made extensive use of ceremonies commemorating historical figures whose actions were used to attempt to lend legitimacy to Begin, his movement, and his government’s policies. The most elaborate of these ceremonies was an official state funeral held on May 11, 1982, in the Judean desert for the purported remains of the fighters and followers of

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Shimon Bar Koziba, popularly known as Bar Kochba, who led the second Jewish revolt against Rome in 132-135 CE.

I contrast the elaborate official state ceremony attended by state officials and representatives of foreign countries who were brought by helicopter to the remote desert site with an unofficial parody of the event. The central event of the official ritual was the prime minister’s eulogy. Premier Begin, frequently referring to the liberation and unification of Jerusalem, emphasized the historic link between the Bar Kochba revolt and the rise and expansion of the new “Third Jewish Commonwealth.” He reminded the audience that it had been the Roman emperor Publius Aelius Hadrianus who had given Judea the name Palestine, ‘a name that still haunts us.’ He declared, ‘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 of Eretz Israel for generations upon generations. Glorious fathers, we are back and we will not budge from here.’

The full ceremonies were covered by Israel’s only television channel (at the time) as well as by radio broadcasts, thereby reaching a wide section of the deeply divided population.

A group of twenty-four young protestors wearing Roman-style togas and carrying spears parodied the official ceremony chanting “You are making a laughing stock out of history.” When Chief Ashkenazi Rabbi Shlomo Goren emerged from his helicopter they broke out in a song about chasing darkness from the land which is traditionally sung on Hanukkah. Although the police and soldiers eventually succeeded in destroying their signs and forcibly removing them from the ceremonies, ... rabbi and Labor member of the Knesset claimed the ceremony perverted Jewish tradition. Opponents of the government’s expansive settlement policy in the territories Israel occupied during the war of June 1967 were particularly critical of the obvious political implications of the ceremony. Even a very senior member of the government avoided the ceremony which he told me he considered to be a “farce.”

Israel became embroiled in a polarized national debate over the meaning of Bar Kochba’s revolt and its implications for the contemporary quandary caused by Israel’s occupation of land on which two million Palestinians reside. The national debate arguing contradictory implications of the

My most recent book, The Spy Novels of John le Carre: Balancing Ethics and Politics, employs an ethnographic approach to the analysis of works of fiction. Although not based on participant observation as were my previous studies, it is based on what Jan Kubik calls “ethnographic problematization and framing.” I reverse the trend of many post-modernist scholars who interpret the words and actions of real people as literary texts. By contrast, I interpret the plight of fictional characters in literary texts as representative of real life situations and moral dilemmas. This approach is consistent with the author’s intent. As he told Melvyn Bragg, “at the moment, when we have no ideology, and our politics are in a complete shambles, I find it [the espionage novel] a convenient microcosm to shuffle around in a secret world and make that expressive of the overt world.” I suggest that le Carre is the ethnographer, having experienced the secret world personally and imaginatively recreated it in fiction. I then supplied an interpretation of the cen-


tral tension in his work between ethics and politics. I treat the novels as extended cases which I interpret very much as I did the data I gathered in my previously discussed political ethnographies.

Using the notion of ideological temperament, which Wilson Carey McWilliams defined as “dispositions of the soul” as distinct from more codified ideological doctrines, I suggest that high tolerance of ambiguity is one of the key defining features of the liberal temperament.20 George Smiley best represents the liberal temperament and skeptical balance that I argue are the core concepts in Le Carre’s political ethics. Smiley, who appears in eight novels, is le Carre’s most fascinating, enduring, and endearing character. I devote an entire chapter to him as the center of an extended case-analysis of skepticism. Le Carre writes of Smiley in his second novel, A Murder of Quality (1962), “It was a peculiarity of Smiley’s character that throughout the whole of his clandestine work he never managed to reconcile the means to the end.” Smiley constitutes the moral center in those novels in which he appears, as do other Smiley-like characters in those novels in which he does not appear.

The chapter in which I most fully explore the concept of skepticism is titled “Learning to Live with Ambiguity: Balancing Dreams and Realities.” My analysis of The Little Drummer Girl constitutes the central case for the elucidation of this theme. It is the story of the recruitment of an English actress to infiltrate a Palestinian terrorist ring operating in Europe against Jewish and Israeli targets. She is recruited by an agent of the Israeli Mossad as bait to track down the leader of the Palestinian cell in order to assassinate him. The agent, Gadi Becker, a younger and more physically attractive version of George Smiley, is the moral center of the novel. The novel forces the reader to consider the psychological and ethical price paid by the agent and her handler (and by inference by Israel as well) for the successful accomplishment of this goal. It also symbolically addresses the future of Israel/Palestinian relations in the “twice promised land.”

With reference to my analysis, former Senator Bill Bradley (who served on the Senate intelligence committee) wrote: “Aronoff poses challenges, such as the limits to which democracies can go in using nondemocratic means to protect democratic freedoms – for example, in the war against terrorism – without undermining those very freedoms.”21 Democracies, unfortunately, sometimes violate the spirit of liberty and freedom in the name of their defense – especially under perceived threats to national security. The discussion of the implications of this has never been more salient than it is today amidst the current “war on terror.” I suggest that the ethnographic reading of novels helps elucidate this by allowing the reader to enter into the hearts, minds, and souls of individuals engaged in this activity and exploring the personal, institutional, and national costs and implications of these ethical compromises. It thereby makes abstract Jeffersonian principles concrete and more understandable in the present world context. My approach wed two ethnographic spirit of inquiry with what political scientists call a political theoretical (philosophical) analysis of ethical issues.22 The combination of ethnography with political philosophy explores the broader moral public implications of private actions. This is done implicitly without invoking a broader academic discussion of the relevant philosophical literature. I deliberately avoided such an academic discussion precisely because I wanted to address a broader audience than my colleagues in academe who specialize in these issues. Moral dilemmas are discussed without invoking contractual theory, natural rights, and notions of sovereignty. The problems facing us are too important to be limited by obfuscation by self-segregating academic jargon. Although this work may not constitute a conventional ethnography, to me it is ethnographic in spirit and it helps clarify dilemmas which date back to the Hebrew bible and classical Greek philosophers, not to mention other cultural traditions.

My most recent major project in collaboration with my colleague Jan Kubik, Anthropology and Political Science: Culture, Politics, Identity, and Democratization,23 is near completion. In it we explore the ontological, epistemological, methodological, and conceptual similarities and differences between the two disciplines. A key observation is the paradox that as political scientists have become more interested in ethnography and the concept

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22 My late colleague Carey McWilliams used to tease me about being a “closet” political theorist. After reading the manuscript of this book he said: “Mike, you have finally come out of the closet as a theorist.”

23 It is to be published in a series edited by William Beeman and David Kertzer by Berghahn Books.
of political culture, anthropologists have undergone a soul-searching and scathing critique of the value of both participant observation and the conceptualization of culture.24

Our main argument is that there is considerable “added value” when ethnography is incorporated into political science’s repertoire – for example in evaluating the symbolic dimension of politics such as in ritual, the construction of collective memory (and amnesia), and the constant contestation over collective identity. This is essential in analyzing problems of legitimacy – the transformation of power into authority and the challenging and undermining of legitimate authority.25 Alternatively, anthropology benefits from the experience and conceptual repertoire of political science – for example in taking into consideration the importance of party systems and the nature of regimes. Too frequently anthropologists jump from the local to the global. No one would argue against the importance of understanding the trans-national nature of our contemporary world, but we ignore the continuing importance of the state and its institutions at our peril.

Most scholars tend not to read across their disciplinary (or even sub-field) boundaries. In fact, being interdisciplinary, or bi-disciplinary, can be professionally marginalizing. For example, I have been introduced both as “half a political scientist” and as “half an anthropologist” by very prominent scholars in both disciplines. For some it is apparently difficult to conceptualize a person who earned a Ph.D. in two disciplines as being an equal member of each field. With noteworthy exceptions, like James C. Scott, David Laitin, Susanne Rudolph, and Lloyd Rudolph, few political ethnographers have gained high visibility in political science. James Scott, who was honored with a plenary panel discussion of his contributions at an annual meeting of the American Anthropological Association, may be more widely read and cited by anthropologists than by his fellow political scientists. In this, he is clearly a dramatic exception to the rule. Perhaps not coincidentally, all of the aforementioned scholars with the exception of David Laitin have played leading roles in the perestroika movement in political science.

The perestroika movement is a reflection of, and a catalyst contributing to, the opening up of the discipline of political science to a wider range of approaches than strictly positivist ones. Kristen Monroe called the movement that has challenged the hegemony of positivism “the raucous rebellion in political science” in the subtitle of her edited volume.26 Among the contributors to this volume Rogers M. Smith was one of the movement’s main leaders, Jennifer Hochchild was the first editor of the new journal Perspectives on Politics, Robert Jervis was one of the leaders of the new qualitative research section of the APSA,27 Dvora Yanow and Peregrine Schwartz-Shea, editors of the recently published Interpretation and Method have been active in the organization of panels on ethnography and interpretation at APSA meetings in which many young scholars have participated.28 It is noteworthy that Bob Jervis and Susanne Rudolph are recent past presidents of APSA signifying the success of the perestroika movement and the legitimation of the diversity of approaches it represents. Last, but certainly not least, a group of scholars gathered in Toronto in October 2006 thanks to the efforts of Ed Schatz at a stimulating workshop on “Political Ethnography: What Insider Perspectives Contribute to the Study of Power.” It is particularly gratifying to witness these positive developments and to feel that I may have made a modest contribution to them. I am honored to share my thoughts on this subject with the readers of Ab Imperio.

SUMMARY

27 American Political Science Association.