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'Almost One of Us': Fieldwork in Turkey 1969-1971
"Neredeyse Bizden Biri": Türkiye'de Saha Çalışması Yürütmek 1969-1971

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I. The Beginning

I first came to Turkey in September 1964. I was twenty-one. I spent two years teaching English, first in a town in Central Anatolia and then in one in the East. By the end of my stay I was speaking quite fluent Turkish. My perspective on Turkey was, and still is, heavily influenced by my intense personal, and inter-personal, experience in Anatolia during those years. I gained a perspective on the country that might even be called anthropological, though I had not yet studied anthropology. I was taken aback by the incredible palimpsest of past civilizations everywhere I visited (and I travelled throughout the country whenever I had the chance), by the dynamics of contemporary Turkish society, and by the warmth of social relationships. I was personally welcomed wherever I went and began to feel a strong affinity for the country

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and its people. I only came to participate in the more sophisticated, cosmopolitan life of "the Other Turkey" in Istanbul after my marriage in 1967 to Ipek, whom I met as a fellow graduate student at the University of Chicago. I began graduate studies at Chicago in September 1966, and was awarded a PhD in social anthropology in 1973. My dissertation was, quite naturally, on Turkish society. The account of these first and formative years in Turkey remains to be told. In this paper I take a critical look at my anthropological fieldwork experience between 1969 and 1971 from the distanced vantage point of the now fifty years that have passed since those days. In reading the account below, it is important to remember that when I started fieldwork I already spoke the language very well, had married into an Istanbul family, and already had had significant experience in the country. In other words, I knew my way around.

II. The Research

> By the mid-1960s . . . the relations of ethnographers to the peoples they studied had become much more problematic – pragmatically, politically, ethically, and epistemologically. (Stocking 1992, p. 366)
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> . . . the relationship between an anthropologist and an informant rests on a set of partial fictions half seen-through." (Geertz, p. 34)

Fieldwork

In September 1969 Ipek, and I boarded a cargo ship in New York for Tangier with our VW in the hold, the first stop on our trans-Mediterranean trek to Istanbul. A PhD candidate in anthropology at the University of Chicago, I was heading to "The Field," the appellation anthropologists give to the often faraway places where they undertake research, the research process itself given the exoticized denomination "Fieldwork" (sociologists just do plain Research), and most iconically embodied in the work of Bronislaw Malinowski, the "mythic culture hero of anthropological method" (Stocking, 1983a, p. 71). Malinowski's close-up participant-observation of the natives of the Trobriand Islands in Melanesia from 1914 to 1918 constituted the *ur*-moment in the discipline's claim to a distinctive method. As it turns out, the posthumous publication of Malinowski's field diaries (1967) debunked the myth of the master fieldworker and his relationship with the natives. Subsequently, starting in the 1980s with the post-modern re-examination of the bases of knowledge production in the social sciences, there has been a proliferation of critical self-reflexive examinations of fieldwork as a research method. This turning point in anthropological thinking was given strong voice in Clifford and Marcus's seminal *Writing Culture*, published in 1986.

In the pre-World War II heyday of anthropology, "the field "was usually somewhere far from home, an exotic tribal spot in Africa, a Pacific island, the remaining habitats of the indigenous peoples of North America, or the jungles of South America (Goody). In most cases, as was true of Malinowski, little was written about the larger, very often colonial, setting within which the research was undertaken, a setting frequently previously having gone through the violent pacification of the natives by the colonial power or nation-state in charge. The anthropological analysis was often set in what later disparagingly came to be referred to as a kind of timeless "ethnographic present," which excluded the "irrelevant" colonial setting in the "background." According to George Marcus, "The world of [these] larger systems and events has thus been seen as externally impinging on and bounding the little worlds [of anthropological focus], but not integral to them." (Marcus, p. 166) Though the earlier accounts often only told a part of the larger story, the fact of the lone ethnographer's "being there" came to constitute his (or less likely her), and the ethnographic text's, foundational cognitive authority. With that said, the question of what constitutes the "there" in "being there" remains – the case in point being my doctoral "fieldwork."

From Tangier Ipek and I drove to Marrakesh at the edge of the Sahara and back, and then headed across the Strait of Gibraltar by ferry following the northern coast of the Mediterranean, finally arriving in Istanbul ("the Field" for me) in early October. In the 1960s fieldwork was a mandatory rite of passage for novices such as myself wishing to gain membership in the tribe of Real Ethnographers. Ipek, then a PhD candidate in Political Science at Chicago, accompanied me, but she was heading "Home" to undertake research (not known as fieldwork) on religion and politics in Turkey, the country in which she grew up and lived in until she finished high school. She was the daughter of a well-known doctor and head of a major hospital in Istanbul and a high school biology teacher. She grew up in a home and a larger social environment in which the modern, western-oriented secular republic of Atatürk was the unquestioned foundation of their ideals and national aspirations, and an essential theme of their daily discourse.

Even though we were both planning to return to the States in two years, after having spent long years as a student in the United States, Ipek was, at least in her mind, in the midst of the first stage of a Rite of Passage of Return to the homeland. To put the seal of The State on that consequential move she applied to the Turkish Consulate in Chicago for a *naklihane*, a permit allowing her to import personal possessions duty free offered to permanent returnees from abroad. Consequently, trailing us about one month later in the hold of a Turkish freighter was our cargo of household goods and personal possessions which were to remain in Istanbul for our Final Return sometime in the indefinite future. We did indeed go back to the States in 1971 as originally planned, and then in 1976 once again applied for a *naklihane,* as we had decided to move to Istanbul, this time to stay —we hoped. "You're returning for good again?" (*Yine kesin dönüş mü yapıyorsunuz?*), quipped a bemused older Turkish friend long settled in New York at our comings and, especially goings. I began teaching at Boğaziçi University in Istanbul in the spring of 1977.

This Ethnographer's World

> Work is not divided among independent and already differentiated individuals who by uniting and associating bring together their different aptitudes. For it would be a miracle if differences thus born through chance circumstances could unite so perfectly as to form a coherent whole. Far from preceding collective life, they derive from it. They can be produced only in the midst of a society, and under the pressures of social sentiments and social needs. That is what makes them essentially harmonious. (Durkheim 1933, p. 277)

> It now appears to me that I examined the rationality of factory organization primarily from a managerial point of view . . . . I did not examine the very real issue of the role of th[e] cultural system in fostering and masking the exploitation of the workers in the factories. More generally, I did not focus on areas of conflict or potential conflict, but rather emphasized the harmonious nature of the situation, as the quotation from Durkheim [above] . . . indicates. A more accurate analysis would have taken both factors [harmony and conflict] into account. (Dubetsky [Duben] 1976, p. 451)

My research was to focus on the impact of urbanization and the culture of small-scale industrial production in Istanbul in light of the massive movement of people from countryside to city that was beginning to transform the country. This paper relates the heretofore-untold account of the larger world in which I conducted my fieldwork during those transformative years. It is an account of the research boundaries anthropologists set for their inquiries, boundaries which in the past frequently intentionally or unintentionally excluded the larger transnational and national political and socio-economic events taking place in the anthropologist's "out there" from their "little world," the domain conceptually demarcated for research. In another sense, this is a reckoning with the theoretical and methodological-epistemological quandaries within which I found myself during and particularly after fieldwork as I attempted to explain both what I had experienced then and come to terms with what I felt I missed.

In 1950, on the eve of the major exodus from rural Anatolia that would change the face of Turkey over the coming decades, roughly only twenty percent of the population of the country resided in cities. Today, in 2020, that urban-rural ratio has completely flipped, with approximately eighty percent of the population living in cities and only twenty percent in rural areas. The urban population of Turkey had increased by five million alone during the 1960s, the decade before I arrived to begin fieldwork, and by 1970 constituted nearly forty percent of the total population of the county. That, as we now know, was just the beginning. Anthropologist and historian Ali Sipahi (2020) describes a cultural re-evaluation (no doubt from 'on high') of the livability of countryside versus city in Turkey, most vividly portrayed in films of the period. Prior to the 1950s it was the cities that were considered the most livable places in the country. The rural countryside was portrayed as poor, primitive, and even "malignant." "Subsequently," he writes, "country and city switched places in the popular mind as Istanbul's haphazard urbanization gave birth to [a negative] image of the city. . ." As the fifties gave

way to the sixties the focal point of much sociological and anthropological research in and on Turkey quite naturally began to shift from village to city, with a special focus on the lives of the mass of new urbanites of Anatolian origin – those caught in-between two rapidly changing worlds.

I left Chicago for Turkey armed with a largely functionalist, very sociological, perspective on the issues of concern, grounded in the thinking of Parsons (1965) and the central Weberian (and focal anthropological) concern with understanding the meaning of social action, subsumed under the anthropological rubric of culture. I began my fieldwork with a theoretical focus on the implications of the differentiation of home and work, one of the key concepts guiding the work of both Marx, Durkheim and Weber, and one of the keystones of modernization theory.

Differentiation was described (and subsequently questioned) as a process which characterized actual events on the ground in the West, where industrialization (and hence "modernization") first took place, and then served as a theoretical construct for examining modernization in other, largely non-western societies. Lloyd A. Fallers, my thesis advisor and a committed Weberian anthropologist, succinctly summarized the reigning non-Marxian theoretical perspective on modernization and inequality in particular in the 1960s as follows: ". . . the systems of stratification that emerge from the process of political and economic modernization are the products of the interaction between the forces of generic modernity . . . and traditional societies and cultures upon which, and within which, modernity works" (Fallers 1973, p. 91). What Fallers meant by generic modernity was the "widespread separation of occupational roles from domestic life, and their location instead in specialized [impersonal] structures." (Fallers 1973, p. 111). He continued by pointedly asserting that such differentiation was not just a spatial one but "even more important, [these separate worlds] are in large measure normatively segregated and subject to different social rules" (p. 112). What Fallers meant by traditional society, in this context, was the highly personalized social and cultural worlds of work embedded in historically-based sets of obligations which structured relationships and provided a larger framework of meaning for workplace relationships. Following Weber's relativizing of the Marxian historical trajectory, and his (Weber's) removing it from "the illusion of historical inevitability," Fallers (1973, p. 14) argued that the diverse mixes of modern and traditional and the local meanings attributed to the relationship of owners and workers in industrializing non-western societies directly affected the various local trajectories of differentiation and capitalist modernization in those places. In this light there were, it seemed, multiple paths to modernization.

I had set off to examine this theoretical problem on the ground in newly industrializing Istanbul. Empirical studies emerging in the 1960s from societies like India and Japan (Singer; Abegglen; Nakane) suggested that that a "modern" social order could indeed be constructed on the foundations of a mix of "traditional" social relationships and the varying exigencies of modern workplace organization. Such an approach would naturally require a rethinking of

the meaning of tradition and of modernity. That differentiation might be the dynamic force behind exploitation, class conflict, and a future socialist society was secondary to my way of thinking at that time. To my way of thinking the meanings of "exploitation" were various and contextual and in the end rested in the eyes of the beholder, and the emergence of a class-for-itself highly contingent.

In 1973, back in the United States, and soon after the paper on work organization quoted above was accepted for publication, I began to question my analysis, which then led to my writing an addendum to the article finally published in 1976. Looking back I was taken with the question of why, it came to seem, that in one sense the clearly economically exploited members of the working class in Çağlayan were acting against their so-called "objective" interests by accepting such conditions. Irrationality? Or, perhaps, was it a broader-based rationality set in a combination of their realistic perceptions of the job market, traditional social relationships, loyalties, and obligations in the form of cross-economic class service and patronage, and longer-term economic interests, hopes and aspirations? I outlined some of the dimensions of this tacit bargain in "Class and Community in Urban Turkey." In theoretical terms I argued that,

> I do not view the development and formation of social classes as a self-evident phenomenon. [. . .] Like consciousness of one's social class, consciousness of one's position in a sect or caste or kinship group cannot be dismissed as an epiphenomenon [false consciousness] where it is a category of significance to the members of that particular society and serves as a guide for their social action." (Dubetsky [Duben] 1977, p. 365)

But then I hesitated, concerned about the loss of perspective often arising from the anthropologist's penchant for studying small places and bypassing the impact of larger "external" factors. By the end of two years, as my awareness of events in Istanbul on the ground grew under the influence of my experiences with politically engaged friends and family, and in light of the events in which I had participated or witnessed in Istanbul, I began to question the limits of my research domain. I concluded in the 1976 factory organization paper that ". . . to ignore the . . . impact of social forces about the significance of which the locals have no idea, such as the growing strength of class in the society as a whole, is to fall victim to the myopia from which some anthropologists will occasionally suffer" (Dubetsky [Duben] 1976, p. 365). Here the thinking of Georg Lukacs (1971), whom I had been reading, trying to better understand the bases of class consciousness, from a Marxist perspective came into play as did that of the Weberian Şerif Mardin (1967) on social class in Turkey. With all of these seemingly contradictory perspectives regarding the social, cultural, and economic bases of class in mind, I found myself in the midst of a serious methodological and epistemological quandary from which I could not easily extricate myself. I took up this issue once again in a paper on the rationality of the informal Turkish economy in light of the thinking of French Marxist anthro-

pologist Maurice Godelier (Duben 1991; Godelier)

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As a budding ethnographer, a person steeped in the ethos of Malinowskian fieldwork, I felt I should set up house right in the *gecekondu* (urban migrant) community I was planning to study. I should, I felt, be living, eating, and continuously fraternizing on an everyday basis with the people I had come to study -- with no differentiation of work and home, that is. That was indeed the calling of the rarified myth of Malinowski's fieldwork which even the master himself was not able to live up to. I knew, of course, that there were always limitations to participation, in which the anthropologist, to use the title of a classic book on the subject by Hortense Powdermaker which I read as a student, is both *Stranger and Friend* (1966) to the people he or she studies. Total immersion, we were taught, even with its limitations, was the only way to get a holistic hold on the life of the natives, or at least as big a bite as was possible. In any case, the holism sought in the classical approach was that of the proverbial anthropological village or tribe. Not a megalopolis like Istanbul. Urban anthropology was newly emerging in those days and there was a lot of methodological confusion. The anthropologist was, according to the old fieldwork ethos -- which many now disparage – supposed not only to be observer and questioner, but also eager participant in the everyday life of the people being studied. Participant as what; in what sort of role? Participant as anthropologist? That was a question few anthropological neophytes such as myself could answer very well.

There is a famous cartoon describing the members of the over-studied Hopi Indian household of the American Southwest that includes three generations of native Americans and a live-in anthropologist. It is easy to be cynical -- call it realistic -- about a young middle class American "participating" for a year or two in the lives of poor people in some underdeveloped non-Western country, regularly taking notes about all his or her encounters, dignified with the role of "one who knows" (from the "advanced" West) as opposed to "those who are known" (from the under-developed East or South) (Geertz 2000b). And then when the job is done, the anthropologist gets on a plane and flies back home to write a scholarly dissertation for a scholarly readership in the protected luxury of American academia as the final step toward a university career. Did that venture implicate the anthropologist as complicit in a diffuse sort of cultural imperialism, or was it merely the source of some moral uncomfortableness? For the anthropologist the stay was undertaken in the name of Serious Scholarship and, one must not forget, a personal career. These issues of cultural dominance and the bases of the authority of the anthropologist have been taken up at great length since the postmodern turn of the 1980s. Though politically aware, I doubt that my informants had the perspective to see themselves in the patronizing role of "those who are known." How could they? When I was doing my fieldwork I too did not see my role versus theirs in that way. Given the built-in asymmetry of the relationship between ethnographer and those studied I have chosen to refer to the subjects

of my research with the now disparaged term informant rather than the preferred referant "interlocutor," a term which assumes a balance in a relationship which by its very nature is not there. Living with this structural imbalance is disturbing for most anthropologists, who like to see themselves as respectful, fair and sympathetic voices of those they have been studying face-to-face over long periods of time.

The people I studied in Istanbul patiently tolerated my constant questioning and poking around in their lives (I often wondered why). Some I think even got to like me, but not one of them shared my understanding of what I was doing. How could they? At times even I had my doubts about the worthwhileness of sitting around all day, drinking tea and asking a bunch of locals who are trying to get work done a slew of questions about something that is either totally obvious to them or of no relevance in their lives. No doubt, they participated because they too got some benefits from the association with the American anthropologist, though those benefits must have been offset for some by the dangers of associating with a person whose intentions were often suspect, especially during a period of increasing anti-Americanism in Turkey and elsewhere in what used to be referred to as the Third World.

Suspect in Divriği

In the early spring of 1970 I decided to spend some time in the Divriği (pronounced Deevree) district of the province of Sivas in east-central Anatolia. I was hoping to visit some villages like the ones from which many of my Istanbul Alevi informants from Çağlayan (pronounced Chaalayan) had migrated and learn more about the reasons why so many people from those parts had been leaving for the city. I had official permission to conduct research in a number of Anatolian provinces, including Sivas, obtained from the Turkish consulate in Chicago before I left the U. S. in 1969. On my way to Divriği from Istanbul I paid my respects to the provincial governor, the *Vali*, in the provincial capital. I remember being a bit intimidated by the palatial building in which he was located and by the huge office he occupied at the far end of which he received guests and supplicants. The *vali* was unexpectedly hospitable, though seemingly a bit bemused by the especially young-looking American wishing to do research on migration in the province for which he was responsible. From the provincial capital, I drove along an unpaved road through barren countryside, continually rising in altitude as I approached the remote town of Divriği. The road came to an end at the town located along one side of a deep gorge at the base of which flows the Çaltı (pronounced Chalta) River, which from there winds its way south and empties into the Euphrates.

My first order of business was to visit the local district officer, the *Kaymakam*. It was a visible step down in status visiting the *Kaymakam* after my encounter with the *Vali*, clearly marked by the more modest make-up of his office. After greetings and some small talk, followed by a discussion of the object of my planned research, he summoned the local agricultural engineer who I learned had a jeep at his disposal and frequently visited villages in the district. He

would take me along with him on his visits I was told. At one point another man walked into the room. I was introduced to him, the chairman of the local Republican People's Party (CHP), the party of the Kemalist establishment. He greeted me rather coolly, and after a moment of uncomfortable silence brusquely asked what I was planning on doing in the area. I explained my project, which understandably did not seem plausible to him given the great effort and probable cost of flying an American all the way to Turkey, then transport him to Divrigi just to find out why poor peasants were leaving for the city. I tried to explain. The CHP chair responded by asking me if I was also going to investigate the growth of anti-Americanism in Turkey. The *Kaymakam* then joined in the conversation and started lecturing me about the "social crisis in America." As we parted, he made sure to announce to me (and, by inference, to the CHP party head) that his motivation for helping me was that I was undertaking some sort of scientific research, not that he or other administrators there likely believed that. But I did have official permission. In any case, the seeds of suspicion were sown. A week later, after visiting a number of Alevi villages, I was called into police headquarters and told that my permission to research in rural villages had been revoked. I would have to confine myself to working within the city limits.

Distraught, I set off for Ankara to remedy the situation. At the Ministry of the Interior I hit a stone wall. No one was sympathetic to an American wanting permission to nose around in the remote Turkish countryside inhabited by Alevis, a heterodox Islamic sect with strong Shiite and pre-Islamic components, and whose members typically voted for parties on the Left in those days. Though I had permission to continue my research, the restriction to the town did not serve my purpose, and after remaining there a bit longer, I returned to Istanbul with little information about the trials and tribulations of village life to take with me.

III. The World around Me

> . . . the 1960s had an enormous impact on Turkey, and the changes those years brought forth had such a staying quality, they left their mark on the following forty years. . . . Consumption patterns, brand names, basic rights and freedoms, autonomous bodies, the Kurdish problem, socialism, art and science for the people, a planned economy, an import substitution economic perspective, Anatolian rock, arabesque, varieties of popular music, the *gecekondu* . . . (M. Alkan, p. 933, translation mine)

Family and Friends during a Decade of Rapid Social Change and Political Crisis

I arrived in Istanbul to begin fieldwork on small-scale industrial organization and local culture at the very end of a decade of escalating political crises that would conclude on 12 March 1971with a military ultimatum and intervention followed by a declaration of martial law.

Martial law was, in turn, followed by a massive crackdown on Leftist political organizations, the arrests, imprisonment and torture of large numbers of young political activists, and the fleeing of many others, some of whom I knew personally.

I continued with my fieldwork even after the military intervention, though with special care only to work with informants I already knew. I left for the U.S. in September 1971, none the worse for all the political turmoil and the painful absence of many Turkish friends. I was, after all, "just" doing fieldwork, and had to return home to write my findings up. In addition, I had lots of engaging fieldwork stories about my radical Turkish friends and myself and the political situation in the country to share with my American family, friends, classmates, and professors, stories which were set apart in a cognitive realm of their own and were not to be a part of the Serious Ethnographic Account of my fieldwork I was to write. Paul Rabinow (253) referred to this common sort of banter among anthropologists about their fieldwork experiences as "corridor talk."

It was not surprising that I was suspect by some during those times. What *was* surprising was that no harm came to me, that, in fact, most people, even radical Leftists I knew, were very hospitable. Much of the time I was not really aware of the extent of the animus toward America and the suspicion Americans working in the country were under, perhaps because in many ways I also saw myself, to some degree, as a part of Turkish society, having married into a Turkish family. With my almost perfect Turkish and command of everyday cultural mores and body language, potentially antagonistic Others seem to have accepted me on quite inclusive terms.

The 1960s in Turkey were defined and powered by the engine of population increase, rural-urban migration, urbanization, rapid industrialization, consumerism, an explosion of books and popular magazines, a dramatic increase in the numbers of university students, and by frequent demonstrations and often violent street conflict. The population of the country increased by 2.46 percent per year between 1960 and 1965 and by 2.52 percent during the second half of the decade. (Özcan, p . 188). The total population of the country was 27.7 million in 1960; by 1970 it had reached 35.6 million. Despite that decade of rapid industrial growth, averaging about 9.5 percent per year (Özcan, p. 190; Keyder in Tonak, p. 48), industry was not able to fully absorb the increasing plentiful supply of laborers entering the market. Though approximately 300,000 Turkish workers left the country to work in Germany and other parts of Europe between 1965 and 1970 alone (Özcan, p. 189), estimated formal unemployment figures rose from about seven to ten percent by the end of the 1960s (Özcan, p. 189), and this without taking the volatile informal sector into account

The 1960s were also defined by escalating political conflict and violence. The relatively liberal constitution of 1961 opened the door to an explosion of civil society activity in a country where many basic civil rights had long been suppressed in the name of the State. The result was a much more open society than had ever existed in the country, a major fluorescence of publications, many in translation, feeding young people hungry for knowledge and for a radi-

cally changed society. There was a barrage of once forbidden books on the market. There was also an amazing growth of civil society organizations ranging from *gecekondu* improvement associations to those dedicated to radical change, some to revolution. By the end of the decade there was an absolutely baffling proliferation of radical groups known by ever-agglutinating acronyms competing over hair-splitting ideological and tactical differences.

The late sixties also saw the emergence of a myriad of Left and Rightwing and Islamist activists and groups ranging across a broad ideological spectrum. The rapid urbanization and social change that characterized the 1960s released a hitherto unrealized yearning for fundamental social change among young people in particular. This impatient desire became increasingly radicalized throughout the decade and was also spurred by a radicalized world climate. The spirit of '68 was contagious. Turkish youth were in rebellion, for the first time openly challenging the rather staid, complacent hegemony of the old republican elite.

Legislation passed in 1963 under the new constitution permitted industrial union organization and strikes for the first time. It took some years for the unionization and industrial action to come into full force. There was a major strike at coalmines in Zonguldak on the Black Sea in February 1968. In July workers occupied the Derby Rubber Plant in another major strike, which was followed by many others as the decade came to a close.

In March of that year the ultra-nationalist Turkish National Student Association organized a huge demonstration in Istanbul. In May the religious Right organized a mass prayer *cum* protest at the Dolmabahçe Mosque along the Bosphorus close to downtown Istanbul. In June Istanbul University was occupied for the first time. The same month the U.S. Sixth Fleet paid a visit to Istanbul anchoring near the Dolmabahçe Mosque. Leftist students protesting the US presence in the city attacked the sailors, throwing a number of them into the Bosphorus. Soon after, riot police raided student dorms in Istanbul, beating and arresting numbers of Leftist students. The right of center party in power engaged in a decisive battle with the Left, leaving those on the Right untouched. In February 1968 the neo-fascist National Action Party was founded and in the middle of that year set up commando training camps near Izmir for its youth-wing. In the eyes of most, the ultimate blame for the unrest lay with American imperialism and its local henchmen. Radicals from Right to Left were united in their determined anti-Americanism. Those on the Left believed that the true revolution could not take pace until the Americans were kicked out of Turkey (Cemal, p. 15). Coca-Cola had just hit the rather closed Turkish market in 1964 and soon came to be the symbol of American imperialism. The events of 1968 in France, the Soviet invasion of Czechoslovakia, and the Tet offensive had an enormous impact on the rapidly growing student population in Turkey (Samim, p. 157). The year 1968 was the high-water mark of what was referred to as the "democratic" Leftist youth political activity in the country; from then on the movement became increasingly authoritarian and rigidified (Alpay, p. 173). For a comprehensive overview of the relationship of the intellectual world of the Left and political activism during the 1960s see the article by well-known journalist Şahin Alpay (1988), the ideologist of the Turkish Maoist movement at the time, as

well as a very personal retrospective review of events during the decade and after by a well-placed insider, the journalist Hasan Cemal (2012).

There was a large American presence in Turkey in the 1960s, most notably in the form of military bases all over the country and the ubiquitous signs posted everywhere of USAID work, symbolized by a big Turkish-American handshake bedecked with the two national flags. From 1962 to 1969 the Turkey Peace Corps program brought hundreds of young Americans to Turkey, most of them teaching English in local secondary schools in small Anatolian towns and cities. There were hundreds of young American Peace Corps volunteers living throughout the country. I was one of them – between 1964 and 1966. We were everywhere, living evidence of a widespread American presence in the country. Many volunteers were liked as individuals and established warm relationships with locals. Others were the focal point of questions in people's minds regarding their "actual" mission in Turkey. Most were probably both. Why would the US government go to the expense of flying over and paying the salaries of hundreds of young university graduates just to teach English in little towns and cities all over the country if not for some deeper purpose? In 1969 the Turkey program was shut down as a precaution, fearing the political impact of possible violence against the volunteers in the increasingly anti-American atmosphere in the country.

On a visit to the American-funded Middle East Technical University campus on the outskirts of Ankara in January of 1969, known to be a Leftist hothouse, the automobile of the American Ambassador, Robert Komer, was set on fire. This led to a brutal police intervention on campus and the beating and arrests of numerous students. In February the Sixth Fleet returned to Istanbul and again there were incidents. A major demonstration in central Istanbul in February turned deadly with clashes between Leftists and the religious Right. It came to be memorialized as "Bloody Sunday." Further strikes at major factories exacerbated the increasingly unstable political environment. Oya Baydar, a member of one of the Leftist groups describes the escalating clashes beginning in the fall of 1970, especially in Istanbul and Ankara, as the start of a major violent struggle [*tam bir çatışma ortamına girilmişti*]" (Baydar and Ulagay). Enter Alan Duben, the fledgling anthropologist. Many of the *gecekondu* neighborhoods were the focal points of political discontent, though most residents were not radicalized. The community in which I worked for two years was quite calm throughout that time. There was calm in the neighborhood during my stay, but it was a calm in the eye of an ominously growing political storm in the larger world surrounding it. Many, perhaps most of the locals, had non-union jobs, largely in the informal sector. They were typically underpaid; there were many child workers given meager wages and no benefits other than the personal help that their bosses chose to dispense following traditional codes of patronage. The neighborhood was not far from a number of major factories ın Kağıthane where unionization was more active and working class consciousness more developed. So why were things so calm in the neighborhood? To what extent were those in Çağlayan aware of the implications for them of what was happening down below in Kağıthane and in İstanbul as a whole? Following Fall-

ers et al., was it this probable "culturally-based" myopia due to the interplay of "traditional" Turkish relationships of loyalty and obligations in a new and "modern" work setting that kept them down, what one might call a Turkish version of modernity? Was it a larger rationality, an accounting made by the workers, consciously or not, regarding their short term and longer terms interests? Was it the relatively small size of the enterprises in which they worked? Or was it their looking back to where they had not very long ago had come from, and recognizing their relatively better off living conditions and their rising expectations in the city as opposed to the countryside? It is difficult to know. All of these and other factors may have been important to varying degrees. Perhaps some answers might have been found through in-depth interviews with workers and their families. But by the time I had become fully aware of these dynamics in the neighborhood and their possible theoretical implications the 12th of March 1971 was upon us, the military had intervened and made it impossible for me to pursue such questions openly. Back in Chicago for a semester in the fall of 1971, I pondered and fretted about this unresolved issue.

Privileged Marginality

On 3 May 1995 *Hürriyet,* then the highest circulation daily newspaper in the country, published an interview with me for a series titled "From Four Continents to Istanbul." I was the representative from the Americas. The title of the full-page spread about me was 'Neredeyse Bizden Biri,' which translates as 'Almost One of Us' in English. A Turkish colleague at Boğaziçi University objected: "Why the 'Almost'?"

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Where to live "in the field?" Renting a place in one of the *gecekondu* neighborhoods was out of the question. I was in Turkey not just in the role of foreign anthropologist. Having married into an upper-middle class Turkish family I was also part of *their* world. I came to fear that I would lose what historian of anthropology George Stocking (1983, p. 64) referred to as the "privileged marginality" of the ethnographer if the people I were to study knew about my membership in a Turkish family. I was referred to as *damat*, meaning son-in-law, by members of the family, and when one of them described me I was *bizim Amerikalı damat* (our American son-in-law) for the senior generation and *enişte* (uncle or brother-in-law) for those of my generation – all of these appellations meaning that I had married in to the *whole* family, perhaps the whole nation. A friend of my sister-in-law (who quite naturally called me *enişte*) began jokingly addressing me as *milli enişte* (brother-in-law of the nation). For more on *enişte*s, foreign and local, see Bora and Çifçi, eds. *Enişte Risalesi* (2019).

I now had a *kayınpeder* (a father-in-law), the doctor, and a *kayınvalide*, (a mother-in-

law), the biology teacher, a *baldız* (a sister-in-law) who was writing a PhD dissertation on Chaucer, and a *bacanak* (a brother-in-law) who was a civil engineer and a graduate of the prestigious Istanbul Technical University. There were also cousins, and a niece, with all of whom I had various degrees of tacit mutual obligations. If *gecekondu* locals with whom I had developed amicable relationships, instrumental "friends" from my perspective, were to learn of my Turkish family relations, that would have become a point of discussion among us and I would immediately have been subject to the standard bundle of requests and obligations from my informants, as we used to call our with regard to my family. A well-known doctor and head of an important hospital for a father-in-law would have been a major asset for low-income "friends." How could I refuse to secure his services for a medical need, serious (who can say?) or not? None of my *gecekondu* informant/friends ever knew about my *damat*, *enişte* or *bacanak* statuses. It was clear that my wife and I could not even think of living in the neighborhood I would be studying. Firstly, we would not have "fit in." We would have been the focal point of unusual attention and, very likely, suspicion. Our private life would have been gone. Turks or others of our background and education would never think of living in such a place.

When I was with my Turkish family during my fieldwork days I imagined I was off-duty. I liked to believe I wasn't doing fieldwork at those times, though I later came to see that my perspective on events during those years was very much shaped by the abundant and often intimate, open, and certainly privileged conversations I had had with family and friends about Turkish society and politics. What Turkish friends and others thought I was doing – research, spying, wasting time, all of those things — was never entirely clear to me. Was I a tool of American imperialism in their eyes? For some no doubt. Some of my wife's old friends, even a first cousin, were suspicious and shared their dark thoughts about me and my ilk with her. Her status and loyalties, being the local wife of such a person, were even called into question by some. In spite of all of that unease in the air, many of my friends sensed my sympathy to their cause and trusted me enough to ask favors of me, favors that could possibly have compromised both parties.

I was loath to share my necessary but unsettling decision about where to live with my American graduate school classmates or professors, whom I naively assumed were having or had had, more pristine fieldwork experiences. I felt I was being unfaithful to a high anthropological code. My research grant was from the U. S. National Institute of Mental Health (NIMH), as was the fieldwork of all my Chicago classmates, but I never thought of myself as allied with U.S. government interests in any way. Though our anthropological forefathers working under colonial regimes benefitted from the Pax Britannica, or the equivalent in the United States, most had little sympathy for colonialism and often sided with the natives (Stocking 1983). By the end of my two years of fieldwork guilt began to creep in. Was I in some way wasting the time of, perhaps even exploiting, my informants for the benefit of a science, *our* science, irrelevant to their lives? At the start of my research period my brother-in-law, the

engineer, who was from a small city in western Anatolia and had vast experience with construction workers and other ordinary working people, seemed baffled by what he presumed to be the fatuousness of what I was planning to do. He suggested that he could give me better answers to all the questions I would be asking than the uneducated people I was going to be querying. Since I knew that I knew much less about social life in Turkey than he did I was a bit taken aback.

In the end my wife and I rented a modest flat in Bebek, an upscale community on the Bosphorus very near Robert College, an American institution of higher education set on the Bosphorus hills, which had been subtly introducing American culture to locals in Turkey since the 1860s. Bebek was a very sophisticated place, populated with urbane very westernized people and numerous Anglophone foreigners, some of whom were teaching or working at Robert College. Bebek was the Mecca of the American community in Turkey. At least Malinowski's tent was just a few steps from where the natives lived. I was to live in another world. Or was I?

Our apartment in Bebek was set at the top of a steep cobblestone road on the European side of the Bosphorus. That was my home, the place where I lived when I was not at work in the *gecekondu*. Our apartment had a large terrace overlooking the verdant hills of the Bosphorus, its slopes punctuated with clusters of elegant cypresses and magnificent pinola trees with their imperial umbrellas shading the green spaces below, stately Ottoman mansions, ornate palaces of wood lining the shores and punctuating the green carpeting of the waterway's rolling hills.

Conflict and Repression

One floor up from us lived Melek Ulagay and her husband Ahmet. Ahmet was an American-educated economist and professor at Robert College, then in his early thirties. Melek was the daughter of a major Turkish industrialist, a scion of an haute bourgeois Turkish family, a Robert College graduate like Ipek, a classmate of her sister – and, at that time, an active Maoist. Ahmet, who refused to join her in the mythologized urban class battle, was a mild-mannered sympathizer, the kind of person whom the Maoist activists referred to as "a petty bourgeois pacifist." (Baydar and Ulagay, p. 128) Given our close relationship with Melek and her's with the Maoist movement, I will try and portray our social and political interactions as an important component of the world in which I lived in Turkey between 1969 and 1971 as I now see it.

The major issue for Turkish revolutionaries was how to appeal to the deeply traditional, conservative peasants of Anatolia and the growing though weak urban working classes — how, that is, to foment revolution in a partially industrialized, fundamentally peasant society. Interminable debates raged on the Left concerning the appropriateness of the so-called Asiatic Mode of Production versus European feudalism as vehicles for understanding the problems of transition to capitalism in Turkey. The subject was not just one of intellectual import, as

the conclusion reached could have a direct impact on revolutionary strategies for the desired transition to socialism.

Over time Ipek and I became closer friends with Melek and Ahmet. Many an evening we shared grilled fish and drank wine or *rakı* together, chatting on into the night on our terrace with a full view of the Bosphorus. Melek's apartment was the meeting place of the Maoist translation committee. That committee was the window to the world for the Maoist organization for which she worked. Young people were constantly coming and going; when we would go in (for some reason, we were never restricted access to this supposedly secret cell) invariably there were several very serious young people perched around a table intently gazing over a pile of papers, and conversation would come to an uncomfortable halt until we walked out. My sympathies were with the Left, and most of our friends in Istanbul were Leftists of various persuasion. It was hard to be an intellectual in Turkey and not be on the Left in those days. Being on the Left meant being part of a very cool and exciting sub-culture. Ahmet Samim, the pen name of Murat Belge, a well-known Leftist intellectual, described the "culture" of the Left in Turkey of which he was a part during those years. "In Turkish intellectual life, the Left enjoyed a scarcely rivaled supremacy," Samim (169) wrote, " . . . which might have been the envy of some even some more advanced societies."

Melek commuted from the comforts of bourgeois Bebek to proto-proletariat *gecekondu*s and participated in demonstrations and strikes. It was from the privileged hills of Bebek that I too forayed into the world of the urban peasants I was studying, and in whose lives I was, so to speak, to be participating. Oya Baydar, the co-author of their fascinating *tete-a-tete* about the period, teases Melek about her peculiar commute: "There you were traipsing between Bebek and the *gecekondu*s; what a contradiction!," she recalls (Baydar and Ulagay, p. 124) Ipek had a much more realistic perspective on Melek's doings. As Melek recalls it, one day some time before the big street clashes of 15-16 June 1970 Melek was again bombarding the two of us with her Maoist talk, and Ipek lost her patience: "Hey, cut it out! Here you are living in Bebek, glass of wine in hand, preaching to us. Then you're off traipsing around in some working class neighborhood. Why don't you just go and live in one of those *gecekondu*s and do your cultural revolution and leave us in peace!" (Baydar and Ulagay, p. 128, translation mine)

Not long after the military intervened on 12 March, when the word was out that gendarmes were searching home to home, Melek stopped by in a bit of a panic and asked us if we could store some "stuff" of theirs, thinking that it would be safe at the home of an American. Not wanting to be in the position of refusing such a request, we naturally said yes. The stuff came in a couple of sealed boxes that we removed to a back storage room and forgot about.

In the meantime our elderly landlord was becoming more and more irate at the goings on in Melek and Ahmet's apartment. One day he really lost his temper and started shouting at Melek and Ahmet, and by association, Ipek and myself, from his terrace: "You Bolsheviks, You! Get out of here. Leave!" Melek and Ahmet were evicted. All four of us had a farewell

party writing Leftist slogans in big characters on their empty apartment walls. Not too long after that two gendarmes knocked on our door. When I showed them my American passport, they never even stepped in. My Turkish revolutionary friends were right about the benefit of being an American at times like those.

The boxes came to mind when we learned that Melek had escaped into hiding. We frantically pulled them out, looking over the contents for the first time. They were full of Maoist literature and Kurdish revolutionary writings, the products of the work of Melek's translation committee. There could hardly have been more incriminating evidence than the contents of those boxes, and we knew we had to destroy them or risk arrest. We couldn't take them out of the house for fear of being stopped and searched by the authorities at one of the many check points quickly set up all over the city. Continuing to hide them was too risky since the authorities might come back. One never knew. The only alternative was, we concluded, to burn them. Our bathroom served as incinerator as we nervously tore up, put match to, and flushed down the toilet the ashes of revolutionary tract after revolutionary tract bit by bit. We saw Melek several years later in Amsterdam where she had taken political refugee. She never even asked about the papers. We didn't mention them. So exciting were the stories of her escape via Anatolia, and into Palestinian camps in Lebanon and from there to Switzerland that the once-incriminating papers never even came to mind. I also helped friends remove outlawed books from their homes and stash them elsewhere before the authorities came. Everyone was frantically stashing their books somewhere or other. I was a foreigner. Was I safe? I suppressed my fear of the police searching my car.

IV. Double Dissimulation

Where does the purely personal experience of the anthropologist as just another individual in what is typically a foreign place end, and the professional one "in the field" begin? Or are they inseparable, as Clifford Geertz argues in his highly idealized description of the position of the fieldworker, a description which he himself seems to have already debunked a few pages before the paragraph quoted below:

> The outstanding characteristic of anthropological fieldwork as a form of conduct is that it does not permit any significant separation of the occupational and extra-occupational spheres of one's life. On the contrary, it forces the fusion. One must find one's friends among one's informants, and one's informants among one's friends; one must regard ideas, attitude, and values as so many cultural facts and continue to act in terms of those which define one's own commitments; one must see society as an object and experience it as a subject. (Geertz, p. 39)

The fieldworker in a foreign place is not a stranger once he or she has settled in, but is no true friend either, as Geertz (p .33) himself noted when he referred to the "moral asymmetry of the fieldwork situation." He (Geertz, p. 34) uncomfortably concedes that "the relationship between an anthropologist and an informant rests on a set of partial fictions half seen-through." So, the idealized undifferentiated life of the anthropologist at home and at work was not that different in kind from the reality of the lives of the workers I was interviewing in Çağlayan who were locked into highly personalized relationships with their bosses. The degree of dissimulation characterizing my relationship with my informants was similar in many ways to that of worker and boss in the small factories I was studying: an unspoken agreement to carry on. What I was not aware of at the time was that thinking and writing about my so-called private life with family and friends in Turkey and my problematic political entanglements could also be seen as part of my job as an ethnographer. I had lost an important dimension to my account by not including myself in the story.

Looking ahead to the "big picture" in *Anthropological Futures*, Michael M. J. Fischer proposed that,

> Anthropologies to come require ethnographic vignettes that serve as destabilizing pebbles, ethnographic gemwork shifting back and forth between microscope and setting . . . complications for the simplified official stories and disciplinary truths, other ways of recognizing what is going on. (Fischer, p. 243)

V. The Return – Once Again

Ipek and I returned to Istanbul in 1976. By the end of the decade I had radically changed my interests from urbanization to family history. Then on 12 September 1980 the military intervened once again, and took over the administration of the country, declaring martial law. Once again there were massive arrests, imprisonments and torture and the shutting down of the civil society that was just getting up on its feet after the trauma of 12 March 1971. Universities, typically marked as seedbeds of radicalism, were a special target, the social sciences in particular. Research possibilities dried up out of fear of reprisals. The once decentralized university system was brought under the centralized tutelage of a government-appointed Council of Higher Education. Many lost their jobs; some resigned in protest. The atmosphere was very restrictive. No one I knew could think of doing on-the-ground social research. That included me for sure. It was then that I decided to refocus my family change interests and undertake pure historical research complemented with a limited number of retrospective interviews in the privacy of the homes of elderly friends of my in-laws or parents of friends or colleagues at the university. I joined up with a Turkish colleague and together we spent the 1980s doing archival, census, and library research on the late Ottoman and early republican decades, and a number of interviews. We would not have been able to access late Ottoman population re-

cords then located in district population offices in different parts of the city without the help of the Istanbul provincial governor at the time who, luckily for us, happened to be the father of one of our students. The result of that effort was *Istanbul Households: Marriage, Family and Fertility 1880-1940* (Duben and Behar 1991). The political situation and the universities opened up a bit in the 1990s, but by then Ipek and I were in New York, not to return permanently again (this time for real) until 1999, when I began teaching at Istanbul Bilgi University.

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