With Ithaca on My Mind:
An Anthropologist's Journey

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My theme, anthropology as journey, owes much to a reading of the poem *Ithaca*[1] by Constantine Kavafy, a magnificent Greek poet who wrote during the first part of this century. This deceptively simple poem deals metaphorically with the process of discovery, with journeys of both mind and body, and it draws on Odysseus' near mythic Ithaca as symbol and inspiration. For me, an anthropologist with cross-cultural experience, the intellectual impact of this Kavafy poem is powerful and its philosophical message profound. For an American of Greek descent, like myself, whose mother and father were born and raised in Ithaca, it also evokes memories of the past, bittersweet dissonances, emotional detritus of a family formed in cultural transition. Perhaps just because of the obvious asymmetry in these strong impressions, I make use of this poem today to frame my remarks. A spinner of images of beauty and sensitivity, Kavafy captures for me the illusive core of an anthropology I value and attempt to practice—and—my father would have been so pleased even with these elliptical references to his beloved island and its Homeric past.

The first stanzas of the poem *Ithaca* stand almost as an exhortation to those entering anthropology, a charge to the science of man. In my translation, the opening lines are as follows:

When you set forth for Ithaca,
pray that the journey be long,
full of adventures, full of knowledge.
The Laestrygonians and the Cyclopes,
angry Poseidon, fear them not,
such you'll never find along your way
if your thoughts are lofty, if select
emotions touch your spirit and your flesh.
The Laestrygonians and the Cyclopes,
fierce Poseidon, you'll not encounter
if you don't hold them in your soul,
if you don't conjure them up before you.

Pray that the journey be long.
That many be the summer morns,
with what pleasure, with what joy,
you enter harbors first-time seen
and stop at ports Phoenician,
and acquire wares of beauty,
mother of pearl and coral,
amber and ebony,
and sensuous scents of all sorts,
as many as you can,
excesses of sensuous scents.
To many Egyptian cities go,
to learn, and to learn from the learned.

What is it that anthropology seeks to learn, in those many Egyptian cities and Phoenician ports? Here in the United States anthropology is, quintessentially, the science of man, an overarching discipline with ties to the biological sciences, to the humanities, and to its sibling social sciences. A science which in theory explores all facets of human life through both time

and space, it comprises physical anthropology, archeology, linguistics, and sociocultural anthropology. Like any science, it is ultimately concerned with the establishment of general laws and principles; as a unique and distinct science, however, its objective is the determination of the principles that govern mankind. Emile Durkheim, that seminal social theorist, once stated that scientific theory has as its goal the expression of reality—anthropological theory is no exception to this dictum.

There are many theoretical currents in contemporary American sociocultural anthropology, the most popular among them being structural-functionalism and Weberian theory in their modern guises, structuralism, symbolic theory, and Marxist social theory in its several current guises. Significantly, no one approach dominates American anthropology, nor can any one be viewed as presenting the perspective of most American anthropologists. This situation lends itself to a potentially fruitful combination of frustrations and flexibilities. The plethora of approaches can be frustrating, especially for students new to the field, in that the subject often seems to lack a clear set of epistemological understandings; it is not always obvious *what* it is that the anthropologist *wants to know*. Conversely, the absence of rigid boundaries and definitions offers an arena for creative combinations and innovative solutions to theoretical and methodological questions. The history of anthropology continues to demonstrate the value of multiple perspectives and the flexibility gained from a lack of a dominant approach and rigid definitions. In this, the discipline has had great good fortune.

Anthropology, or perhaps better, anthropologies, are part and parcel of the historical context in which they develop, in which they are used—to answer some questions and to pose others—and in which, as often as not, they are eulogized. Anthropological theory has not sprung *de novo* from the heads of individual scholars. It has not developed in a vacuum. It has reflected the concerns of the day, the issues and questions of moment. The tone and direction of scholarly work, more frequently perhaps than its ivory-tower exponents know—or admit—respond to the changing demands of policymakers and to altered financial conditions and constraints. This reality is one that anthropologists need to understand better.

Social and cultural anthropologists attempt to express reality by

observing, recording, analyzing, and then abstracting the essentials of specific realities. We normally carry out these tasks in the field, *in vivo,* among living groups of people in their quotidian rounds of life. For the occasional romantic and the misinformed, the field is something far removed from the beaten track, a *New Yorker* cartoon, an isolated locale peopled by bizarre folk peered at by pith-helmeted anthropologists so preoccupied with the savage and exotic as to exclude the complex and the civilized. Such a view, of course, never held true. The anthropologist can find and define the field, the locus of study, as easily in the boardrooms of Wall Street, among AIDS victims in New York and Kampala, or among the schoolchildren of Barbados and Grenada as among the surviving Ona of Tierra del Fuego or the hunting and gathering Siriono of the Bolivian lowlands. Anthropology, of all social sciences and humanities, has pursued a vast variety of problems within the widest range of geographical locales and sociocultural and technological contexts.

As anthropologists know, if only intuitively, the field is not mystical or metaphysical but rather methodological in concept. It is an amalgam of the scientific problem to be pursued, the human aggregations thought necessary for resolving that problem, and the essential locales that link both. To enter the field, then, is to embark on a journey of discovery that, seriatim, confirms or adjusts the parameters for study, identifies the specific knowledge necessary for understanding these parameters, and indicates the appropriate techniques for generating that knowledge. The field, in short, is a process that unveils that precise context in which the specific research problem is embedded. When such a context is revealed and comprehended, the problem *qua* problem is resolved, the journey ends.

If anthropology has a soul, that soul is the field. It is the corrective of dogmatic extravagances in theory and zealous chauvinisms in ideology. The field, the cumulative experience of the field, cleanses the anthropological psyche of its own Laestrygonians and Cyclopes and of its other assorted windmills.

There is a personal dimension in this process as well. More than just *rite de passage,* the field is both crucible and molder of anthropologists—a placid depository of that which must be known, teacher to those willing to

learn, stern judge of those who come to it with preconceptions. Perhaps most importantly, it is the source of that constant experiential pressure which helps shape and maintain anthropology's distinctive world view. For most who have experienced it, the field is intense and demanding, in some ways comparable to psychoanalysis or psychotherapy. Differing greatly from the psychiatrist's couch, however, the field provides a litmus test to all who enter, a test of that intricate dance between researcher's personality and intelligence and the unfamiliar social and cultural forces that initially engulf him. The novice in the field, proverbial bull in the china shop, learns the steps of this dance, gradually adjusts his strengths and weaknesses to a strange milieu, masters the subtleties of his trade, collects far too many answers for far too many questions, and sojourn suddenly completed, he emerges from the field, rarely satisfied, but substantially more knowledgeable than when he entered—sprung not from the psychiatrist's couch but from some bull ring of conflicting realities. This cycle tends to be repeated, perhaps more muted in psychic cost and more productive in objective result each time the researcher re-enters the field to undertake new study. For me and, I would hazard, for most of my colleagues, it is the sum of these many fields, the sum of these journeys of mind and body, that constitute the discipline of anthropology. So, would it not be fair to say that repeated and sustained effort in the field, Kavafy's "many summer morns," is what allows his "harbors first-time seen" to take scientific shape, and his "wares of beauty," and "sensuous scents," and learnings "from the learned" to be acquired.

Illustrations drawn from my own field experience might add dimension to these comments. Each quite different from the other, they show my interest in the nature of society and in basic research on real-life issues. The first deals with social innovation in Jamaican society; the second with community and change in Bolivian society; and the third with individuals and deviancy in Greek society.

In 1957, I was commissioned to carry out a study of social innovation among Jamaicans who were ostensibly fishermen—at least, they were so considered by those branches or agencies of the Jamaican government most concerned with the fishing industry. The objective was to isolate those factors contributing to the success or failure of a government-sponsored program organizing fishing cooperatives among coastal folk. I include this study

here to indicate that youth and energy may well be antidotes to limited resources.

Having just completed work on a somewhat similar study of fishing folk in Barbados, I arrived in Jamaica aware of the many pitfalls that await the brash ethnographer. Consequently, before deciding on an overall research strategy and with the Barbados experience vividly in mind, I devoted the first weeks to familiarizing myself with fishing, fishermen, and associated phenomena throughout Jamaica. During this initial reconnaissance, I visited over a third of the 140 fishing beaches of the island. I catalogued the range of fishing methods, equipment and techniques, documented the more obvious relationships of regional or local ecology on fishing activity, and examined, at first hand, as many fishing cooperatives as possible. The reconnaisance yielded some unexpected results: for example, although Jamaican fishermen could come from very different socioeconomic settings, in order to eke out an existence, they all, as a rule, had to combine fishing with wage work of various kinds and, very often, with own-account cultivation. What also came to light was that the particular kind of fishing practiced by an individual depended on the type and extent of the work he did on land. Consequently, it seemed that fishing itself was a dependent rather than a dominant variable in the so-called fisherman's economic equation. These tantalizing leads were used as connecting threads for the research that followed.

To ensure that all major ecological zones be included, five coastal settlements were selected for intensive study. Given the constraints on my time, this meant I could spend no more than two months in each study site. This restriction had major, although not necessarily negative, implications on the style of field research that I could conduct and the methods and techniques that could be employed. Consequently, the study took the form of what I would call a survey in depth with a balanced, two-pronged approach, quantitative and qualitative. Utilizing the quantitative approach, I managed to collect data on kinship, marital status, internal migration, schooling, literacy, house types, land tenure, cultivation, livestock, occupations, type of fishing, and the like. Employing the qualitative approach, I was able to gather material on fishing, agricultural and handicraft technologies, to conduct in-depth interviews with key informants, and to study the cooperatives in detail. Both approaches yielded rich research dividends. I still find it remarkable

how efficient data collection can be when problem, field, and method neatly mesh. For that matter, I still marvel as to how easily such ideal conditions can propel a usually composed ethnographer into manic fits and frenetic motion.

I was able to demonstrate that the people studied did not warrant blanket categorization as fishermen; that even though they cultivated land, they did not neatly fall into the sociological categories then in vogue, that is, peasants, farmers, or plantation workers. This enabled me not only to meet the objectives of the study, but also to enter into the then quite vitriolic scientific debates about the nature of Caribbean society. With regard to the fishing study, however, the social innovators who had attempted to promulgate fishing cooperatives in Jamaica unwittingly had borrowed a model and concept of cooperation drawn from the experiences of full-time fishermen in industrial nations and it had little or no relation to conditions in Jamaica. The fishing cooperative was a benign innovation which assumed an occupationally homogeneous membership. Given the heterogeneous adaptations necessary for survival in the harsh socioeconomic reality of Jamaica, it was also doomed to failure.[2]

The Bolivian case that I turn to now is a rare example of an anthropological team study of an entire nation-state. It is an illustration of some of the theoretical flexibility and methodological range possible when adequate financial and personnel resources are available. In 1964, twelve years after a major social revolution in Bolivia, Vera Rubin and I were asked by the Peace Corps to identify the major health problems of rural Bolivian communities in different ecological settings; to analyze the dynamics of rural community organization; and to assess the implications of rural social organization for community action. To accomplish these tasks, we undertook a series of linked epidemiological and anthropological studies which were intended to facilitate the work of the Peace Corps and other development agencies in that country. To put these studies in context, some background is necessary.

In 1952, a combination of social and economic events forcibly pitched Bolivia into the twentieth century. After a series of coups and counter-coups, the Nationalist Revolutionary Movement took power. The MNR, a party of urban intellectuals and politicians with widely differing

ideologies, led, guided, and occasionally diverted the several elements in Bolivia clamoring for change and recognition. With a sweeping platform of social reform, in total opposition to the ideas and wishes of the traditional elite, the MNR had to assure itself of the support of the Indian population, that great majority which until that time had never been allowed participation in Bolivian national life. To ensure this support, a number of basic actions were taken which transformed the power shift of 1952 into a frontal attack on the traditional order of Bolivian society. First, universal suffrage was granted to all adults, with no requirements for literacy in or understanding of Spanish. Secondly, pressured by the tin miners, the most highly politicized workers in the country, the MNR nationalized the vast holdings of the three most important tin barons. Finally, and most importantly, propelled more quickly by the extra-legal seizures of latifundia lands by organized Indians, now labelled *campesinos,* the government legislated a national agrarian reform, returning to the Indians land that once belonged to their forefathers. Through this legislation and its somewhat circumscribed implementation, the government, supported by *campesino* strength, weakened the power of the traditional elites. The partial redistribution of the country's national resources and the newly mobilized but politically potent force of the *campesinos* formed the backdrop for social change.

Aside from these three fundamental slashes at the fabric of an almost classic example of a plural society, in the immediate years after the Revolution other less obvious efforts were made by the MNR to integrate a sharply stratified, culturally heterogeneous, regionally oriented, and clearly disunited polity. The most important of these secondary efforts in the essential task of nation-building came in the manipulation of social legislation for the benefit of workers, in the building of institutional supports for the rural population, and in the dramatic expansion of rural education.

In the twelve years of MNR control, Bolivian society had been unquestionably altered. However, given the lack of adequate field and archival research, often misleading national statistics, and the pervasive fog of official propaganda, the extent and form of the restructuring remained unclear. In addition, satisfactory answers had yet to be given as to how far the revolutionary leadership intended to carry its reform, to what extent it was willing to institutionalize and legitimatize change, and to what degree it was ready

or even able to incorporate the Aymara or Quechua Indian into the mainstream of Bolivian national life and to permit his free competition for position in society. Answers to these and many other questions were to be sought by the research team.

The theoretical underpinning of the study is revealed by the criteria used to select six rural communities for primary study. These six could not be selected to be statistically representative of the hundreds of Bolivian rural communities because there were no reliable national statistics or other indicators that would have permitted representative sampling. Moreover, they were not selected to represent different regions in Bolivia, or distinctive histories, or divergent economies, even though they were located in different regions, had been formed by distinctive histories, and were widely divergent in economy. Since a primary intent of the study was to assess community action and the mobilization of that action, the theoretically and methodologically appropriate solution was to select communities that represented major organizational types, that is, communities with very different organizational properties.

For understanding post-revolutionary Bolivian communities, the two most illuminating of these properties were undoubtedly social stratification and politicalization. Social stratification, the differential ranking of the categories of people that compose a given social system, was always an integral aspect of Andean history. Even before the Inca conquest of what is now highland Bolivia, highly stratified social systems existed at the local level. The Spanish Conquest compounded the social situation and a variety of complex orders of stratification emerged. By 1964, there would have been no way to understand the dynamics of change without first studying social stratification at the community level. The second property, politicalization, or the intensification of political involvement, is obviously a critical but permutable element in much of the contemporary Third World. It was certainly important in post-revolutionary Bolivia. Before 1952, the vast majority of the Bolivian people, the Aymara and the Quechua, was illiterate, isolated from the centers of power, and totally excluded from participation in national political life, a largely inert but vaguely threatening political mass. The revolution of 1952 began a process of radical alteration, a process of politicalization that ultimately affected, to some degree, all Bolivian communities.

The six communities selected for study, four rural towns and two Indian *pueblos,* represented variations along these two organizational properties, a continuum of community types from minimally stratified and non-politicized to complexly stratified and politically revolutionized. Anthropological teams spent a year in each, worked from a common community study guide, and generated voluminous ethnographic and sociological data for comparative purposes. Each community was also studied by an epidemiological team that, among other results, produced the first national epidemiological study of Bolivia. A census of the study communities was completed as was an extraordinarily extensive social survey of 1130 heads of household. In addition, a variety of sectoral or topical studies were launched and completed. By anthropological standards, the Bolivia study was a mammoth undertaking which, in its three year life span, deployed and coordinated the activities of over eighty social and medical scientists. In objective terms, our study of a country long considered scientific *terra incognita* was a spectacular success. It reached and exceeded goals of scientific and practical value. Of lasting significance, the study also demonstrated the possibilities of productive anthropological team research and produced the most extensive and significant social science data bank on Bolivia to date.[5]

The third and last example of anthropological field illustrates what is sometimes possible when the scientific problem is complex, the available resources minimal, and the research conditions tenuous.

In Greece, hashish users are commonly thought of as deviants, sometimes sociopaths, individuals who are arrested and imprisoned and who transgress the norms of the society more often than non-using Greeks. Against this attitudinal backdrop, I directed a research project during the 1970s, a time when the Colonels' Junta was still in control. In tandem with a medical and psychiatric study of the effects of hashish, this anthropological project sought to identify the social, cultural, and subcultural factors that enhanced or inhibited hashish use in Greece; to provide some idea of use and non-use of hashish in the general Greek population; and, to explore the effects and consequences of Greek drug laws on the lives of users. Preliminary investigation had confirmed initial impressions that hashish use was not extensive at the time of the study; that the few pockets of relatively high densities of users were to be found only in the large cities; that the number

of Greek users was relatively stable; and, that confirmed users were most often from the working class. The principal problems of delineating the field of study revolved around identifying and studying a population which, deviant by social definition, small in number, and scattered in location, was not overly amenable or accessible to scientific probe. Compounding these problems, no trained anthropologists or other Greek social scientists were available to help carry out the research. This meant that the geographical scope of the study had to be constricted without distorting our objectives. I decided to focus on the Athens-Piraeus area, where a high density of hashish users lived, and which would permit the most effective mustering and training of a research staff.

Paucity of historical data on Greek hashish use, insufficiency of objective information on current users and practices, and lack of anthropologists, also forced early decisions on the selection of techniques and methods that would both help to train a staff as well as to identify the sociocultural context of hashish use. The collection of life histories, one of several techniques and methods introduced, proved to be the most successful on both scores. The Greek staff became adept at interviewing and was instrumental in generating over a hundred complete life histories of users and controls. From these voluminous materials, a composite history of urban hashish use was drawn relying only on actually spoken words to evoke the context and to explore the meaning of hashish-related behavior in Greece. Collecting the materials for this composite life history, or put more accurately, this social history of modern Greece from the perspectives of the lower class, proved to be a surprisingly efficient method of beginning to train anthropological neophytes as well as of generating those historical and contemporary contexts from which multi-dimensional understandings of the behavior of hashish users could be gleaned.

Over time, as the life histories demonstrate, hashish smokers, never very large in number and largely ignored by the authorities during the early decades of this century, were subjected to increasingly stringent sanctions. Analysis of these life histories indicates that changes in Greek drug laws had less to do with any imagined escalation of a hashish or "drug" problem than the ideological repercussions that followed the drastic political and socioeconomic turbulences long buffeting Greek society. Even a casual reading of

these life histories makes this direct relationship abundantly clear. As a consequence, it became obvious that hashish use, hashish users, and the consequences of hashish use in Greece could not be comprehended entirely if not viewed and examined in the context of shifting political, social, and ideological pressures, that is, in the context of contexts.

This goal, the essential part of this journey, was reached through a controlled and systematic presentation of the views of the actors themselves crafted to give a balanced, contextualized, and chronological picture of hashish-related phenomena in urban Greece. By use of this format, questions related to hashish are debated by those most affected, a debate carried through the overlapping contexts of connected issues—how hashish was diffused into the country, the influence of the Asia Minor refugees, the trauma of refugee resettlement, the urbanization of Greater Athens and attendant problems, the ambience of the work place, the jails and their effect, and the paradoxes of the law. These and other issues are traced through time beginning with the great Catastrophe of 1922, the rise of fascism in the 1930s, the Albanian War, World War II, the German occupation of Greece, the bloody civil war of the post-war period, and, finally, the Colonels' Junta of 1967.

The subjects selected for anthropological study lived in several contiguous neighborhoods of Old Athens, neighborhoods that lie almost in the shadow of the Acropolis. Included were a number of older men, ethnic Greeks from Turkish Asia Minor, who came to those once desolate Athenian locales over a half century ago. Victims of Turkey's victory over Greece in 1922 and refugees because of the resultant forced exchange of populations between the two countries, many of these old respondents were born and reared in a particular region of Asia Minor where ethnic Greeks had been in the minority. Significantly, the use of hashish among the pre-1922 inhabitants of that region, ethnic Greeks included, seems to have been greater than in other Asia Minor regions where Greeks and other Europeans lived in denser concentrations as, for example, in cosmopolitan Smyrna *circa* 1910. A surprisingly large number of other hashish users, almost all born in Athens, were found to be offspring or otherwise related or connected to refugees from Asia Minor. In addition to this refugee link, many respondents held similar jobs as porters, carters, food vendors, and butchers in the central food markets and slaughterhouses of Athens and Piraeus. Through-

out this century, but particularly during lean years of war and occupation, these bustling centers of aggregation offered ample opportunity for forbidden activities such as hashish smoking and dealing to those so motivated. One eloquent hashish user,[4] ruminating about the days when he migrated to the outskirts of Athens, evoked the following sensitive and sociologically precise images.

> 1917. It was Piraeus. A port. A vagabondish people. The young men. . . some played cards, others ran to women, others smoked hashish. I started to smoke at Foukas'. The people then, without hindrance from anyone—only three days lockup for those they caught smoking—smoked hashish easily without knowing that it embarrassed them. Hashish came from Turkey illegally. It existed in Greece also but of inferior quality. All those who worked in the ports acquired the habit and transmitted it to the porters, to the manual laborers, and to whomever yearned to forget.
>
> . . . There we were in the stinking port of Piraeus. The sound of thievery echoed there, there were card sharks, pickpockets, lottery sellers, hashish users and others, *mànges* and criminals and *koutsavakia.*[5]
>
> When I had met enough of those kinds of friends who were both good and bad, I began to keep constant company with the best of them. The group also brought out the particular disorders of that time. At nineteen, I became a pimp in the bordello of an Irene from Soumi. It was in the second district of Vourla, my first erotic contact. An older woman, twenty-seven, twenty-eight years old, used to give me money and clothes. I fell in love with another, a girl from Mani called Zikoala, and abandoned the first. Even after my marriage, when I was newly wed, I used to go with the common women that hung out in Vourla. That's where I played the *koutsavaki.* I was a pimp there. Whatever I saw others doing, I'd do also. Slowly step by step, I went downhill. I too began to go to the *tekéthes.*[6] I've been to all of them, I've tried them

> all, all the *tekéthes* of Piraeus and Athens and the surrounding districts. Wherever there was a *teké* I was there to roll in the mud just as everybody did. I was a proper *mangas* and a real hashish user and I had no equal.

This social history of modern Greece, based on the words and lives of Athenian hashish users, plays out over a half century of turbulent change and is suffused by two, intertwined *leit motifs*—social opprobrium and governmental persecution. Welling from almost every passage of this history is the insistent claim of hashish users that the misfortunes that befell them in personal and familial life resulted not from some harmful property of hashish itself but from the scorn and disgrace heaped upon them by their fellows and most directly from the extraordinarily heavy penalties exacted by the authorities for their use of hashish. Petros, a respondent who had served time in Greek jails for hashish, vents his frustrations:

> For years now. . .they've shattered my nerves. I can't take it any longer. I've had enough. Nobody will hire me for a job. Nobody wants to take me. They say I'm a madman, a *hasiklis,*[7] a drug addict . . . But I challenge anybody to step up and say that I—I'm speaking about myself now, I don't know about others, they're not my business, they don't concern me—that I owe him a penny or that I stole from somewhere or that I've done anything . . . The only thing on my record is that I 'drink' hashish. That's it. I've found it to be the best 'drink' there is and I 'drink' it. When I find it, I 'drink' . . . I don't find any, I don't. I never harmed anybody. And now I'm not allowed to work. Why? The state, the authorities . . . all they've learned all those years is how to break up homes. There are policemen, 'correctional officers' they're called, who instead of going about their duties, go and break up somebody's home because they have something personal against him. 'You can have a license, you can sell, you can't' . . . why? To be a 'correctional officer' means that he must treat everybody equally. You have to be just . . . isn't that the way it should be? I don't want any favors. All I want is a license . . . even just to

> sell flowers . . . as long as they give me a license for a spot I like. You see, I never met people with pull nor do I want to . . . I want to know people of my own kind. Others happen to have pull, others know so-and-so . . . All I want is to work, for whatever time I've got left . . . five, six more years, before I die.

Markos, once lionized as composer and performer of ***rembetika,***[8] songs of the *bouzouki*[9] early linked to hashish users and the urban poor, brings deep insight as well as fate to this modern Greek tragedy.

> I was not born bad nor have I ever found satisfaction in the sadness of others. I was not born bad nor born to live life as I have lived it. And because of that, I take courage to declare my sins to the world, to those people whom I first sang to about their joys and sorrow, their riches and their poverty, their orphaning and their dark days abroad. I want these people to be my confessor and I believe that all whom I have written about and continue to write about in hundreds of my songs will forgive me when they know that the aim of this narrative of my life is . . . apology and forgiveness. For this, those of you who read my story, friends and strangers alike, but particularly my acquaintances, come, clasp my hand, and greet me with open heart. Tell me that everything has gone by, that all belongs to the past. Tell me that if you had lived the same life as mine, the same would have befallen you, and that you would have done the same as I.

Despite the fervor and eloquence used by hashish users in presenting these claims of opprobrium and persecution, the self-reports in this composite history do not constitute scientific proof in the ordinary sense. However, other quantifiable measures of Greek values, attitudes, and behavior related to hashish use were collected which essentially substantiated these claims. Strangely enough, while these measures were of analytic use in their own right, their real significance emerged only when they were examined against what had been learned from the life histories, that is, from the

most qualitative of qualitative data. If we had given high analytic priority to predetermined and quantifiable variables before having established the general parameters and the appropriate context for study, this project would have been very substantially flawed.[10]

The field is only one of the two indispensable components of anthropology—the anthropologist, of course, is the other. I would be remiss, at this lectern and on this occasion, if I made no comment on the training of those who will take the anthropological journeys of the future. In this regard, the training of those who will choose careers of applied research in domains somewhat distant from traditional academic pursuits, is of special concern to me.

Let us consider the formal relationship of education to anthropology. Despite their availability, anthropologists actually employed as faculty or researchers in American schools of education or in educational research facilities number only several score. Some of these institutions have no anthropological presence at all; a majority of those that do, have but a single representative; only a very small minority include two or more on their faculties or staffs. Contrast this situation, for example, with the historical preference of these schools and organizations for psychologists and with the overwhelming dominance of psychology in the training of a vast variety of specialists in the educational field. I do not intend to review the history of psychology in education or to disparage, in any way, its undoubted contribution to the enterprise. However, in this particularly trying period of educational crisis in the United States, the gross imbalance of the various disciplines represented in education is not just out of step but absolutely counterproductive.

No, education has not clutched anthropology to its breast, but neither are anthropologists blameless in this situation. They, too, are out of step. Sharp differences of opinion exist among American anthropologists about their discipline. If one interprets application broadly, for instance, it is clear that the vast majority of active anthropologists have been professionally involved with practical issues, with the resolution of real-life problems, if only as lecturers providing anthropological perspectives to potential practitioners of other disciplines and professions. Many have contributed in very

direct and dramatic ways to the development and implementation of programs and policies, including education. Yet, unlike other social scientists, they have almost always relegated applied work to secondary status. Impassioned reasons are often given, privately, in defense of this stance. Traditional anthropology, in its unease with application, in its fear of crossing over into social engineering, seems to have rejected out of hand the 19th century dictum of one of its founding fathers, Sir Edward Burnett Tylor. The very last sentence of his monumental study *Primitive Culture* reads "Active at once in aiding progress and in removing hindrance, the science of culture is essentially a reformer's science." While very real dangers abound in the indiscriminate application of anthropology to real-life problems, these dangers cannot be circumvented, in these days of crisis and rapid change, by an anthropology in blindfold. "Dragons in dark caves, Justice Oliver Wendell Holmes once reminded us, are far more fearsome than when they are seen in daylight."[11]

Given this state of mind, the discipline remains traditional and suffused with paradox. The ambience of most graduate departments of anthropology in the major universities of the United States remains antipathetic to the training and socialization of applied anthropologists. Over the past few decades, however, several professional schools of education have been providing training opportunities for specialists in educational anthropology and, in at least one case, in applied anthropology. These programs have no parallel in any other sub-field or specialty in social or cultural anthropology: medical anthropologists, for example, are not trained in medical schools; legal and political anthropologists are not trained in law schools, or departments of political science, or in schools of international affairs; and, economic anthropologists are not trained in departments of economics or schools of business. The unique alternative offered students in educational anthropology came about, I believe almost inadvertently, as social foundations departments in a small number of schools of education developed into multidisciplinary teaching and research units.

Each of the very few anthropology programs functioning in schools of education developed, over time, a character and direction of its own, to a point where considerable difference now exists between them in curriculum and in the underlying philosophy of training. One program, for instance,

prefers in-coming graduate students with a strong background in some substantive field of education; it then offers these students what it considers an appropriate orientation to anthropology leading to a doctorate in educational anthropology. Another program prefers students with a substantial background in social science. It then moves them through a sequence of lock-step courses in anthropology together with an appropriate blend of education courses en route to a doctorate emphasizing anthropological theory and method. Although the preference here is for the latter model, pioneered by my colleagues and me at Teachers College, each has specific strengths for training educational and applied anthropologists; each has specific weaknesses. And, conceptually, between these two models stand several others.[12]

These few programs are located in favorable environments for training and socializing anthropologists to apply the principles of the discipline to educational and other "real" social problems. Graduates of such programs are usually well prepared in all aspects of sociocultural anthropology without the usual disdain for and preconceptions about application. These anthropologists are creating the necessary bridges between a reluctantly changing discipline and those domains of real life, those key social institutions such as education, amenable to its services. Many, I am proud to say, are graduates of Teachers College.

These last remarks may well seem tangential to the theme of anthropology as journey. Yet, the training of those who succeed us in anthropology is quite central to it. For me, and I would think for most anthropologists who teach, and while teaching learn, it is the most rewarding journey of them all; a time when new dragons and old Poseidons are confronted and laid to rest, when disparate pieces of "mother of pearl and coral, amber and ebony" unexpectedly make sense, when new voyagers chart their course—a time when anthropology and Kavafy's *Ithaca* are one.

Always have Ithaca on your mind.
Arrival there is your journey's end.
But do not rush the trip one whit.
Better it last for many years
and you reach the isle when already old,

rich with all you've gained along the way,
without expecting Ithaca to give you riches.

Ithaca gave you that splendid voyage.
Without her you couldn't have ventured forth.
But she has nothing left to give you.
And if you find her poor, Ithaca hasn't fooled you.
As wise as you've become, with such experience,
by now you must have understood
what mean these Ithacas.

REFERENCES

[1]This poem, entitled *Ithaki* in Greek, was written in 1911 and very likely first published in the original language in 1916. There are several published English translations.

[2]For a full description of this study, see Lambros Comitas, *Fishermen and Cooperation in Rural Jamaica* (Ann Arbor: University of Michigan Microfilm Series, 1962).

[3]Among the several project-related publications, see particularly Lambros Comitas, "Education and Social Stratification in Rural Bolivia," *Transactions of the New York Academy of Sciences*, Series II, Vol. 29, No. 7, (1967); William J. McEwen *et al., Changing Rural Society: A Study of Communities in Bolivia* (New York: Oxford University Press, 1975); and Abdel Omran *et al., Epidemiological Studies in Bolivia* (New York: Research Institute for the Study of Man, 1976).

[4]The hashish user is Markos Vamvakaris (1905–1969), a legendary figure in the Greek world of *rembetika* music, who worked alongside several of the older respondents in the hashish study. This quotation, also used in the study report, is drawn from his autobiography, published in Greek by Angela Vellou Keil, and translated into English by Lambros Comitas.

[5]*Manges* (sing. *mangas*) and *koutsavakia* (sing. *koutsavakis*) were socially significant categories of people who lived at the fringes of urban Greek society. Often thought of by the politer elements as street-arabs, as cunning rascals and rogues. A *mangas*, however, furnishes the following description: "*Koutsavakis, Mangas*, or dervish, all of these are one. The only difference is that the highest among them is the dervish and that people consider the *koutsavaki* to be a bad person . . . even though the *koutsavakithes* were well dressed. Rings, watches, shined shoes, a mustache, eh, a beautiful mustache twirled at the ends. They didn't cut it. Hair parted. A good haircut with the nape of the neck shaved, a fine felt hat, because in those days they made Borsalino felt hats. Quiet when among people, he bothered no one and wanted no one to bother him. *Koutsavaki!* Disturbances occurred which sometimes led to killing. For example, you bother a *koutsavaki*, you don't know him and you tell him, eh, aren't you ashamed, eh, why don't you move over . . . that is, you insult him needlessly. The *koutsavaki* can't tolerate that. He'll jump on you and start beating you. He'll slice you with his knife, another might even kill you. That wasn't unusual."

[6] Plural for *teké*, a hashish den.

[7] A user of hashish (a dry preparation produced from resin contained in the flowering tops of the cannabis plant).

[8] A musical form developed during the late 19th and early 20th centuries in the port cities of Greece. That segment of the proletariat, the *rembetes*, who participated in the socially marginal, almost underworld existence of the *rembetika* musician, generated

a distinctive way of life. While a precise description of *rembetika* is difficult, one definition by a *rembetika* composer quoted by Gail Holst in her *Road to Rembetika* (Athens: Anglo-Hellenic Publishing, 1975) captures its essence: "*Rembetika* songs were written by *rambetes* for *rembetes* . . . the *rembetis* was a man who had a sorrow and threw it out." *Rembetika* music, I would argue, stands in the same sociological relationship to hashish use as contemporary Jamaican reggae and American jazz in its infancy does to marihuana use.

[9] A long-necked stringed instrument of the lute family favored by *rembetika* musicians.

[10] The composite life history of Greek hashish users, tentatively entitled *Opprobrium and Persecution: Hashish Users in Urban Greece,* is in manuscript form.

[11] In Raymond Philip Shafer's foreword to Vera Rubin and Lambros Comitas, *Ganja in Jamaica: A Medical Anthropological Study of Chronic Marihuana Use* (The Hague: Mouton, 1975).

[12] For a fuller exposition of these perspectives on the relationship of anthropology to education, see Lambros Comitas, "Report of the Chairman" in *Report and Working Papers,* ed., Lambros Comitas (The Committee on Anthropology and Education, National Academy of Education, 1978).

Lambros Comitas was appointed Gardner Cowles Professor of Anthropology and Education on January 20, 1988. This chair, established in 1983, is awarded to an individual of outstanding distinction in scholarship and teaching.

Professor Comitas joined the Teachers College faculty in 1964 and was promoted to Professor of Anthropology and Education in 1967. He has been Director of the College's Division of Philosophy, the Social Sciences, and Education for the past ten years, the last three of which he has also directed its Institute of International Studies. From 1977 to 1984, he served as Director of the Institute of Latin American and Iberian Studies at Columbia University. Dr. Comitas is also Director of the Research Institute for the Study of Man, New York City, a leading American center of Caribbean study.

An authority on the scholarly literature of the Caribbean, he has carried out extensive anthropological field research in Barbados, Jamaica, Bolivia, the Dominican Republic, Greece, the Soviet Union, Andorra, and Spain. His areas of primary scholarly interest have focused on social organization, rural life, and the effects of drugs on society. Professor Comitas was elected a member of the National Academy of Education in 1979 and is former president of the Society of Applied Anthropology.

Among his numerous publications are *The Complete Caribbeana 1900–1975: A Bibliographic Guide to the Scholarly Literature* (4 vols.); *Ganja in Jamaica* (with Vera Rubin); *West Indian Perspectives* (4 vols. with David Lowenthal); *Report and Working Papers on Anthropology and Education* (for the National Academy of Education); and *Interdisciplinary Research and Doctoral Training: A Study of the Linköping University (Sweden) Tema Departments* (with T. C. Brock, B. Sigurd and Å. O. F. Sundborg).

The Cowles Chair is named after Gardner Cowles, the eminent American journalist, publisher, and philanthropist He was the founder of *Look* magazine and President of Cowles Communication, Inc. A devoted trustee of Teachers College for over a quarter century until his death in 1985, Gardner Cowles rendered distinguished service to this institution with unfailing wisdom, steadfast commitment, and full-hearted generosity.