



TRANS-HIMALAYAN TRADERS

Economy, Society, & Culture
in Northwest Nepal

JAMES F. FISHER

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This book, based on field research conducted in a village deep in the mountains of northwest Nepal, is a revised, expanded, and generally overhauled version of an earlier formulation that was presented as a Ph.D. dissertation to the University of Chicago in 1972. Although the modern intellectual roots of this study extend from Mauss and Malinowski through Barth, its more immediate muses are the faculty of the Department of Anthropology of the University of Chicago. I have followed theoretical tacks increasingly divergent from those of my mentors over the last ten years or so, but I remain profoundly grateful for the general aura of intellectual ferment, stimulation, and creativity exuded by the Anthropology Department at Chicago during my time as a student there. I am particularly grateful to Professors McKim Marriott, whose unbending rigor has made me deal more honestly with the South Asian context than would otherwise have been the case, and Manning Nash, whose general anthropological approach and theoretical orientation have provided the stimulus and sustaining energy for this effort. In addition, I am thankful for penetrating insights and criticisms from Professor (now Sir) Raymond Firth, whose pioneering work in problems of economic anthropology and social change are an obvious and invaluable influence, and to Shepherd Forman for a careful and critical reading of sections of the work which contributed to its transition from chaos toward coherence.

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knowledge of Nepalese agriculture. At the University of California Press, Martin Orans and two anonymous reviewers provided excellent, detailed, and constructive critiques. Victoria Scott performed meticulous copyediting. Richard and Vivian Waite hand-carried the edited version from California to Kathmandu and back. I am greatly in debt to all of these people.

During our year in Tarangpur, my wife and I were for all practical purposes sealed off by the high passes that enclosed us from the rest of the world, with which we could communicate only sporadically by mail. The runners of the Postal Service of His Majesty's Government of Nepal perform an onerous, essential task efficiently and reliably, but under the best of conditions it took at least a month for letters to reach us from Kathmandu, so we had to rely heavily on other people for more favors than would be necessary under less logistically austere circumstances. I am grateful to Dipak Mathema and Susan Southwick of the U.S. Educational Foundation for efforts to get the mail through which went far beyond the call of duty; to Tom and Marilyn Vernon and Richard and Marleane Mitchell for morale-boosting care packages; to Dr. Larry Wilson for medical advice and supplies; to Peter and Martha Fritts for their welcome contribution of warm clothing; to Sir Edmund Hillary for his emergency donation of a tent after our own failed to arrive until we had already been in Nepal for a year; and to Barry and Lila Bishop for massive infusions of peanut butter from their camp in Jumla when our own supplies were exhausted.

Our expedition would never have gotten off the ground without the permission of His Majesty's Government of Nepal and the friendly and helpful assistance of its officers. I am especially grateful to two pioneering Nepalese social scientists, Mr. Dor Bahadur Bista and Dr. Harka Bahadur Gurung, for their support, advice, and encouragement. Of course the research would have been out of the question without the participation and cooperation of the people of Tarangpur, who put up with my presence, persistence, and infernal questions with restraint. To two of its leading citizens, Mr. Takla Tsering Budha and Mr. Chandra Man Rokaya, whose lives have followed very different paths but who each in his own way personifies the highest qualities of his people, I owe a special debt for aid in helping me understand the Tarangpur

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world. Mr. Rokaya has also devoted considerable time to discussion of various questions by mail, and his cooperation has been invaluable. The late Sirdar Changchu Sherpa, the third permanent member of our expedition, managed all the nuts and bolts details, from arranging for porters and looking out for their loads to buying and cooking food, as well as innumerable other small but vital tasks. No detail escaped his careful attention, and he performed his tasks with enormous enthusiasm, initiative, and unfailing good humor. We would never have survived without him. In Kathmandu, after our return from the field, Mr. (now The Honorable) Keshar Bahadur Bista helped uncover and analyze archival data, and Mr. Govinda Bahadur Shrestha assisted in intensive linguistic work with a village informant. My thanks are due to them both.

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Kathmandu, Nepal
January 1985

James F. Fisher

Note on Orthography

The orthographic complications which normally vex nonlinguistic scholarship involving an exotic language are in this instance compounded by the phenomenon of trilingualism. In the interests of general intelligibility and readability, I have consistently attempted to use English equivalents of local terms wherever possible, sometimes adding the original Nepali, Tibetan, or Kaike in parentheses. Where an indigenous term is used, I have italicized it and added diacritical marks at its first appearance, but place names have not been italicized, and I do not use diacritical marks on familiar place names (e.g., Kathmandu, Jumla).

In the case of Kaike, an unwritten and undescribed Tibeto-Burman dialect, I have transcribed words phonetically (see Fisher 1973), since a phonemic analysis has not yet been done. Because Nepali written in the Devanagari script is a broad phonetic transcription, I have transliterated most Nepali terms directly into Roman letters, generally following Turner (1931). Tibetan is an altogether different matter, since a direct transliteration from the Tibetan script is incomprehensible to anyone not literate in the language. In Tarangpur only lamas can read Tibetan; few—if any—actually write it (except to copy texts), and what is written is of course classical Tibetan, not the local dialect. In view of these considerations, and because this study is an exercise in social anthropology with little relevance to the constricted concerns of Tibetology, I have transcribed Tibetan terms phonetically also. Although I have generally tried to avoid cluttering the text with distracting diacritical details, where they are used, (ˉ) denotes long vowels, (ŋ) indicates nasalization, and (.) marks retroflex consonants. To distinguish between categories of social organization and occupations, I capitalize the former but not the latter—for example, Lama lineages and devout lamas; Blacksmith caste and skilled blacksmith.

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1

Introduction

It is easy to see that in the long run, not only objects of material culture, but also customs, songs, art motives and general cultural influences travel along the Kula route. It is a vast, inter-tribal net of relationships.

—Bronislaw Malinowski
Argonauts of the Western Pacific

THE RESEARCH PROBLEMS

At its best, anthropology is the studied and stimulating attempt to mine the richness of John Donne's insight that no man is an island—that we are, as Geertz (1973:5) has put it, animals suspended, ineluctably together, in webs of significance that we ourselves have spun. Yet it has taken anthropologists a long time to extend the epigram to the analysis of the societies we live in and observe. Society is not isolable any more than an individual human being is. Webs—cultural or otherwise—are always supported or in some other way connected to the natural world, and cultural webs always merge with other, similar webs at their geographic and conceptual borders. While Radcliffe-Brown (1952) and Marriott (1955) have each raised questions about the definition and analytical viability of traditional anthropological units,¹ it was left for Barth (1969:9) to observe that:

Practically all anthropological reasoning rests on the premise that cultural variation is discontinuous: that there are aggregates of people who essentially share a common culture, and interconnected differences that distinguish each such discrete culture from all others. . . . The differences between cultures, and their historic boundaries and connections, have been given much attention: the constitution of

ethnic groups, and the nature of the boundaries between them, have not been correspondingly investigated. Social anthropologists have largely avoided these problems by using a highly abstracted concept of "society" to represent the encompassing social system within which smaller, concrete groups and units may be analyzed. But this leaves untouched the empirical characteristics and boundaries of ethnic groups, and the important theoretical issues which an investigation of them raises.

The village in the mountains of northwest Nepal which I call by the pseudonym Tarangpur is probably as geographically isolated a community as an anthropologist is likely to encounter today. Yet the ways in which Tarangpur is not economically and culturally self-sufficient are far more interesting than the ways in which it is. This fact led me away from a finely drawn description and definition of Tarangpur society, the delineation of its internal structure, and detailed analysis of its symbolic order, although these are all of course interesting and legitimate topics. My concerns are of larger scale and wider scope. Straddling the larger South and Central Asian Worlds, Tarangpur exemplifies the interstices of the multiethnic society of Nepal, and I therefore attempt to describe and explain Tarangpur's convoluted and changing integration within those worlds.

Tarangpur is a cultural, linguistic, and economic hinge between the Buddhist, Tibetan cultural area to the north, and the Hindu, Nepali regions to the south and west. As such, its people are marginal to both these traditions, and they do not bear any very great affinity even with Magars elsewhere in Nepal—for example, the Magars south of Tichurong described by Hitchcock (1966). While I am concerned with the mechanisms that maintain ethnic boundaries between these mediating mountain peasants and other groups with which they regularly interact, I am not interested here in the similarities and differences between different populations in western Nepal which call themselves Magars.

The word *Magar*—like Gurung, Rai, and Limbu—refers to a "tribe" or "caste," depending on the point of view of the social scientist who is translating the Nepali word *jāt* (Hindi, *jāti*). I do not find either translation satisfactory. The Magars of Tarangpur and its surrounding villages are not tribal in the corporate, territo-

rial, or political sense usually implied by that term; nor, given their concentration in a limited part of Nepal and the lack of traditional relations with Brahmins and other high-caste Hindus, are they a caste in the conventional Indian sense. If a sociological label is required, "peasant," in Redfield's (1953) use of that term, or "ethnic group" (Marriott 1965) is as appropriate as any, but I am skeptical about the utility of several kinds of anthropological labels for a people who are marginal in so many ways. In these mountain communities, Magar is simply a convenient status summation which can be readily and incontestably claimed by anyone (except untouchables) who wants it. I call the inhabitants of Tarangpur Magars as a shorthand device to place them in ethnographic context, not to describe a specific structural or cultural type.

Tucked away in an obscure fold of the Himalayas a two-week walk from the nearest motorized transportation, Tarangpurians are acutely aware of the world beyond their valley and have evolved various strategies for dealing with their remoteness. In each case they exchange their way out of isolation. These multifarious transactions can be interpreted through the processes by which they are maintained and adapted to the environment, as a way of organizing interpersonal behavior, and as an index of social and cultural change. In adopting a transactional perspective I lean heavily on the ideas of Fredrik Barth, whose *Models of Social Organization*, according to Kapferer (1976), marks a "paradigm" shift in British social anthropology, although he has been less influential in America. However that may be, I do not believe in or argue for transactional analysis as any kind of specially privileged or uniquely insightful theory. It is simply an intellectual framework that helps to elucidate the problems in which I am interested and to organize the data I present (which is all that what usually passes for "theory" in anthropology ever does).

Fundamental to Barth's concept of transaction is the notion that in any social relationship we are involved in a flow and counterflow of prestations, which is one way of describing the movement of goods and services. The flow of goods and services is determined by our own and our counterpart's ideas of appropriateness and value. These ideas determine not only what we exchange but which statuses may be combined in a set in a given exchange; only those involving commensurate prestations are relevant counter-

parts in a social relationship. Such ideas also affect the course of interaction in a relationship: the flow of prestations is not random over time, since each party's behavior is modified by the presence and behavior of the other in a progressional sequence (Barth 1966).

By viewing interaction in terms of transactions, says Barth, we can interpret behavior by means of a strategic model. That is, a sequence of reciprocal prestations represents successive moves in a game. Each actor keeps a ledger of value gained and lost, and each successive action affects that ledger, changes the strategic situation, and thus "canalizes" subsequent choices. Barth's game theory analogy is unfortunate because it casts his model in zero-sum terms, whereas the fundamental notion of transactions is reciprocity. The ledger is a personal one, and under certain conditions (e.g., between players from ecologically differentiated, symbiotic zones), the "game" can have more than one winner. Nevertheless, Barth's model does have the advantage he claims for it of depicting a succession of events over time—in other words, it is a model of process.

One measure of the analytical importance of such a concept of transaction is that it provides a way to assess the strength of values. It is meaningless, Barth maintains, to say that something has value unless people in real life seek it in preference to something else of less value. This can only be the case when they act strategically with respect to it—that is, make it the object of transactions between themselves and others.

I believe the transactional model has the additional advantage of providing an analytic framework for the study of change, since it allows for specifications of the continuity that links two situations in a sequence of change. Different analyses of change can then be generated depending on the nature of the continuity described. This study makes explicit certain assumptions about the nature of the continuity that exists between Tarangpur today (i.e., at the time of the research, 1968–1970) and Tarangpur forty years before the ethnographic present—specifically, the striking shifts in the transactional patterns, as measured in changing allocations of time and resources.

I attempt to deal with two distinct but interrelated kinds of transactions in which Tarangpurians engage. One is the transac-

tions in trade—the exchange of material goods (counted and measured as best I could for the present, and estimated for the past) between and across distant but contiguous symbiotic ecological zones. I argue that these economic transactions are the bedrock in which the second set of transactions—those consisting of interactions between adjacent ethnic groups of different cultures—takes place.

Data for the latter (unlike the former) frequently had to be gathered indirectly because cultural interactions could not be so frequently or directly observed. To obtain data on the economic set of transactions (and to understand the village cultural setting generally) required more or less continuous residence in the village for the entire fieldwork period. To fully observe the transactions in ethnicity would have required constant travel with the traders. This would have been not only logistically incompatible with the requirements of obtaining the economic data set but also politically impossible, since the necessary permits to visit the sensitive northern border area could not be obtained. Hence both the data and the conclusions on ethnic interaction are frequently inferred from what Tarangpurians said and did while in Tarangpur. Examples of this kind of material are found in the discussion of culture and impression management in chapter 4.

Data on the two kinds of transactions (economic and ethnic) do not represent unconnected domains but two empirical sides of the same conceptual coin. To discuss the latter while ignoring the former, as is sometimes done in ethnicity studies, is an error I have tried to avoid. A central theme of the book is that economic transactions changing over time have profoundly affected the interactional transactions, which have in turn generated changes in the ethnic identity and orientation of Tarangpurians. Baldly stated, economic change has been the cutting edge of cultural change. Assessing the material in terms of transactions and attempting to place the concept of choice on a par with that of structure (Firth 1954) is one tentative, halting, but determined step away from the paradigm fatigue of structuralism, in both its Radcliffe-Brownian and Levi-Straussian varieties.

A final, obvious fact may be too easily lost in the abstractions: namely, that the problems addressed here—even the question of choice—are, like all anthropological problems, primarily the prob-

lems of the anthropologist and not of the people whose collective lives are the basis for the solutions (people who, like us, just muddle along and struggle through their lives day after day as best they can). As one Tarangpurian said to me, replying to questions which were to him, of course, childishy simple: "We go to the fields, plow, and weed; we put heavy baskets on our backs and go trading—that's it! This is all there is to discuss about our life here."

Attempting to come to grips with the symbiotic links between regions and the bonds between village and nation is not an entirely novel endeavor. But it does warn of a shift away from intensive analysis of the internal dimensions and organization of Tarangpur and toward the manner of its integration into larger networks. Such an intensive internal analysis could be done—even transactionally, in the manner of Marriott (1976)—but that would be another book. The anthropological investigation of choice and decision-making is still in its infancy, but such approaches all too often focus on unnecessarily narrow concerns—whether to use a new kind of seed or whether to plant one crop rather than another, for instance. What I attempt here, in contrast, is to understand why people choose one culture rather than another. The analysis that attempts such an understanding is incomplete, as all such cultural understandings intrinsically are. As in any anthropological enterprise, there are loose ends; in the spirit of Valery, I have not really finished the project, but only abandoned it.

Most of the book grapples with these analytical issues by placing them in the specific ethnographic context of Tarangpur. After the methodological interlude that concludes this chapter, chapter 2 outlines some of the basic geographic and historical facts that have shaped life in Tarangpur. The central point is that the evidence from all sides describes a land and people who are, above all, marginal—politically, geographically, logistically, linguistically, even mythically. Their cultural identity is up for grabs and must be chosen from competing models.

Chapter 3 sets out the fundamental ecological dimensions of the agricultural cycle on which the rest of the economy (and, I maintain, the rest of the culture) rests. The discussion—much of it a quantified and, I fear, tedious analysis of such items as field size, crop yields, and consumption patterns—establishes that a net grain

surplus is generated. I take pains to be clear on this question partly because precision is, *ceteris paribus*, a virtue, and also because if people are making choices in their lives, it has to be shown that there are choices to make. If the alternatives imposed by the environment are so harsh and restricted that sheer survival permits no other course, the basic economic and cultural decisions are preordained; transactions and choice would not exist.

Chapter 4 describes how the grain surplus has satisfied the demand for salt and become the basis for a long-established grain-salt-rice trading system. I describe how this cycle of transactions has led Tarangpurians out of their valley and into cultural confrontations to the north and south. Chapter 5 details a second, coexisting circuit—which has now largely replaced the first—of animals, wool, and manufactured commodities, and compares the cultural implications of following each cycle. Chapter 6 deals with forms of local wealth, generated mostly from the trading cycles (which rest in turn on the agricultural base) and how this wealth is stored, used, and circulated. Chapter 7 returns to the transactions theme and assesses the impact of the different spheres of exchange for the internal social, political, and symbolic life of Tarangpur. Chapter 8 is a summary that concludes with a discussion of ethnicity and interaction in this unusually complicated cultural shatter-zone.

THE PROBLEMS OF RESEARCH

It would be unwise to assess the data that follow without a methodological pause, since the resolution of the theoretical research problems is illuminated by considering the practical problems of research. Anthropological fieldwork is the type of undertaking in which the theoretical scaffolding as well as the tools of the trade must be displayed along with the finished product.

The field research was conducted in Tarangpur village, Dolpo District, Dhaulagiri Zone,² between October 1968 and November 1969.³ Because of Tarangpur's isolation from transportation services, we were never able to take a vacation—a trip to Kathmandu and back, for example, would have required a month in travel time alone. Added to whatever additional time would be spent in "rest and relaxation," this constituted a total far beyond what we

felt we could afford and still get the work done. The full year was therefore spent in the village and its vicinage, except for a survey trek to Jumla, a small bazaar town a week's walk to the west which seemed to us, after several months in Tarangpur, more like Times Square than anything else.

Despite fairly extensive treks in Nepal in the early 1960s, in the Mt. Everest area and to the west and south of Pokhara, I had never been anywhere near Tarangpur and did not even have any clear notion of how to get there. There were small airstrips at Jumla, a week's walk to the west, and at Dhorpatan, a week's walk to the south, but no scheduled service to either. We attempted at every point to keep the size of our expedition at a minimum, so that we would be, if not inconspicuous, at least not egregiously obtrusive. We were too small and impecunious a group to charter a plane, so we took the regularly scheduled Royal Nepal Airlines Corporation DC-3 to Pokhara.

Not knowing whether any food would be available where we were going, we brought with us from Kathmandu what we hoped (optimistically, as it turned out) would be a year's worth of rice and *dal*, flour, peanut butter, sugar, tea, and a can of peaches to open for Christmas dinner. Our Sherpa assistant, Changchu, busily organized the packing of all these items and their distribution—along with our clothing, books, film, typewriter, paper, and other supplies—into what eventually amounted to thirteen porter loads of 35 kilograms each. By the standards of other expeditions, even scholarly ones, this seemed an alarmingly small amount of provisions to keep us going for a year. (The American Mt. Everest Expedition of 1963, for instance, needed more than 900 porters to carry supplies for three months.) Although we were not entirely sure of our ultimate destination—or that its inhabitants would even allow us to live there—we knew that wherever we settled would be so isolated that we could not afford the time (not to mention the energy) for a trip out to collect more supplies.

But there were some advantages, too, in being a small and mobile operation. We were not only less obvious as we passed through the countryside and into the fieldwork area but we were able to muster the few porters we needed. Changchu found thirteen able-bodied men, about half Tibetan refugees and half low-caste *Damāis* (Tailor caste) in Pokhara. Packed into conical wicker

baskets, the loads were hefted onto the back and stabilized by a rope under the basket tied to a leather strap pulled across the top of the forehead, so that the neck muscles provided most of the support.

We proceeded north of Pokhara over a ridge to the Modi Khola (Modi River) and up the Kali Gandaki River to the zonal capital of Bāglung, took the Myagdi Khola fork at Beni, then went over the pass to Dhorpatan. At Dhorpatan our Damais decided that they would prefer not to cross in their bare feet the snow-covered passes about which we had been hearing ominous stories along the way, and so they quit. We also paid off the Tibetans, and I belatedly regretted my earlier agreement to pay them at half-rate for their empty-handed return to Pokhara. Fortunately, Changchu was able to rustle up eight Tibetan ponies and their owners to take us, for an exorbitant sum, the rest of the way.

From Pokhara to Dhorpatan was a one-week trek, and from Dhorpatan to our destination was another week, but the two legs of the journey were quite different. During the first week we were never far from villages and the supplies and food we could buy from them. But during the second week we were traveling through high, alpine, largely uninhabited country, with only two adjacent villages in our path—Pelma and Yama. We slept in our tent, while the horses were hobbled to graze during the night. The two passes (Jangla Bhanjyāng) near the end of our journey, both close to 15,000 feet high, were already quite deep in snow, and we quickly learned to conserve our strength by letting the horses break trail for us. From the top of the last pass it was a long, grinding, knee-jarring descent to the Bheri River Valley. We headed for the first signs of habitation we saw and dragged ourselves into the village just as darkness fell, only to discover that we were in Gomba, not Tarangpur, which we belatedly saw beyond a stream and around the hillside, another hour's walk away. All of us—horses included—were so exhausted that we decided to sleep where we were and proceed the next morning to Tarangpur.

We were startled to discover that a police border checkpoint had been installed at Tarangpur just a few months before our arrival. This was an external presence I had not anticipated, would never have requested, and did not relish, but it did provide unexpected opportunities to observe the interaction of national and local levels

of government. A measure of the remoteness of the area was the fact that even after we had lived in the village for several months, one neighbor thought that we and the Nepali constables were from the same country—for many of the villagers, all people from outside the valley are indistinguishable. Toward the end of our stay, a friend excitedly ran to our house to tell us that five Americans had just arrived from over the pass; I rushed up to their campsite to discover five Japanese dentists on a vacation trek.

In any case, the checkpost commander was helpful in our search for accommodations. Between his efforts and our own, we finally found a newly rebuilt house right in the middle of the village. In addition to its convenient location, it was the only house in the village with the attractive feature of a ceiling high enough so that I (at 5 feet, 10 inches) could stand erect under it. Believing that ghosts cannot enter a house if they have to bend over, Tarangpurians had always built their houses with low ceilings. But as no ghosts had appeared for some time, our landlord daringly built a higher ceiling, to our considerable relief. The owner suggested a monthly rental of about \$5; we offered \$4 and the deal was closed. Our landlady and her two sons continued to live in another part of the house which had not yet been refurbished.

Our quarters consisted of a single large room, which we subdivided by hanging our tent fly down the middle. Tarangpurians sit, eat, and sleep on the hard dirt floor, so we hired low-caste *Kāmis* (Blacksmith/Carpenter caste) from a nearby village to make us a bed, an enormous desk, and a small table and two chairs for our "kitchen." On one side of the tent fly were our bed, desk, clothes, and medicines, and on the other were the cooking fire that Changchu designed from stones and empty kerosene tins, table and chairs, food, and—within a few days—our four chickens, which occasionally provided us with eggs. Opening onto our single room were four windows, each about the size of my fist; it was so dark inside that we usually needed a flashlight to search for things even during the middle of the day. A smoke hole in the ceiling over the fire added a little more light, but not much.

The first task I set myself was simply to map the village, but the houses are so closely clustered and piled on top of one another, with so many little paths separating them, that I found it impossible to make an accurate map until the Panchayat (the local govern-

ing body) secretary finally agreed to walk around with me and name the houses. I also wanted to take a complete census of the village, with such details as age, clan membership, place of birth, and preliminary genealogical connections. But I immediately discovered—as I was to find constantly throughout the fieldwork period—that people were reluctant to answer questions. Even if they were willing to answer a question, they would almost always insist—for perfectly good reasons, from their point of view—on initially asking why I wanted to ask the question in the first place. I answered by saying that I was a student (in the “21st grade”), that I had come to learn about their customs and history, and that if they didn’t tell me what was going on I would fail in school. They sympathized with my plight, but not to the extent of becoming enthusiastic informants.

Although we had excellent relations with the villagers, who came to appreciate not only our medicines and material goods but also our honesty in dealing with them, most never lost their suspicions of what might come of my knowing too much. Gathering data was frequently like pulling teeth. People in some cultures are very anxious for outsiders to know about them; indeed, I marvel at the good fortune of a colleague who worked in Sri Lanka and found that he sometimes had to ask his informants to stop giving him so much detailed information that he couldn’t record it all. My situation seemed more reminiscent of Evans-Pritchard’s account of working with the Nuer (1940) or Malinowski’s (1922) comments on the Amphlett traders,⁴ although I would not be so harsh.

There seemed to be three major reasons for the Tarangpurians’ reticence. First, because outsiders—including other Nepalese—are virtually unknown in the valley, there was an entirely reasonable suspicion and fear that whatever they said could be used against them. I was interested, among other things, in economic questions—amounts of land owned, taxes paid, and the like—and since the villagers shared the universal fear of higher taxes (a cultural universal, no doubt), they saw no advantage in handing out information that might fall into unfriendly hands, even if we ourselves were benign.

Second, a number of Tibetan refugees had passed through Tarangpur in the early 1960s following the political instability in

Tibet in 1959. Indeed, some still lived in Tarangpur or returned to spend the winter months there, and the stories they told of difficulties with the Chinese had made the Tarangpurians very apprehensive about foreign intervention in their valley. As it turned out, we did many of the things the Chinese did when they first arrived in Tibet—handed out medicine, gave candy to children, and were generally polite and interested in local affairs. Many people just assumed that we were the advance guard of an American invasion force, which would arrive after a discreet period to take over the valley. When we arrived, one of their first queries concerned the state of Chinese–American relations. When I said that I personally did not harbor any ill will toward the Chinese, this only confirmed their worst fears. When I later understood their concern, I told them that America and China were bitterest enemies (this was before Kissinger’s secret trip to Peking and the subsequent rapprochement), and my reputation and trustworthiness improved markedly.

Third, Tarangpurians believe that natural and human objects are endowed with certain kinds of powers (*shakti*), which must be carefully guarded against dissipation. My request for soil samples met with a refusal that was irate as well as adamant, since taking a sample away would place the power of the soil in jeopardy—a not inconsequential consideration in an agricultural community. Later, even my short-term research assistant, Chandra Man Rokaya—who had been born and raised in Tarangpur and still had family and property there, but who was at the time earning his B.A. in agriculture at a college in India—returned for a short vacation and aroused the collective wrath of his fellow villagers when he too wanted to take some soil samples for testing. I encountered the same difficulty when I tried to inventory the Tibetan books (handwritten or printed from woodblocks) kept in most houses. My efforts to copy just the titles met with great resistance, because it was felt that copying the title would drain away the power of the book.

Still another problem I encountered—which I have not seen discussed in the literature of fieldwork—is the problem of the pathological liar. I discovered that two or three people in the village consistently gave me answers that had nothing whatsoever to do with anything that might be called truth. They gave informa-

tion which seemed reasonable, or at least plausible, but which after cross-checking turned out to be fabricated out of whole cloth. Other informants then admitted candidly that one could not believe anything these people said, but it took me a painfully long time to become aware of that fact.

Under these conditions, the intensive fieldwork techniques I was using—insistence on obtaining information directly from individuals without using special informants—were yielding frustratingly little information. Later, I discovered that working with a young man who was a respected village leader, Takla Tsering Budha, opened doors that I could never have opened myself. Once it became clear that it was acceptable for me to find out certain kinds of information, the data began to flow much more easily. Thus I owe much of my information to the assistance of Takla, whom I paid for his help. I avoided paying other villagers, preferring to interact with them as a friend who could sometimes be helpful—for instance, by treating medical problems or by loaning money without interest. As our landlady's son told us early in the fieldwork, if we gave him ten cents for every relative he named, he would give us an unprecedentedly detailed and extensive genealogy.

I tried—on most occasions successfully—to type up my notes every evening, so that gaps and inconsistencies could be noted immediately instead of after my return to Chicago. At night during the winter, it was frequently below freezing inside the house, so I sometimes found it too cold to type (on such occasions my wife and I read nonanthropological books⁵ aloud to each other by the fire) and so delayed consolidating notes until the warmth of the following day. I made two copies of everything: one was kept in a topological file (social structure, religion, etc., although the categories constantly shifted according to my changing perceptions of what the relevant local categories were), and the other was filed in a chronological sequence. I also kept a separate diary in which I recorded my more impressionistic feelings and reactions.

The standard methodological chestnut handed out to anthropologists about to embark for “the field” states that one must learn the local language. In the trilingual case of Tarangpur, this advice was not very helpful. To master all three languages—and acquire enough usable data at the same time—was beyond my linguistic

competence (with one totally deaf ear, I have trouble enough understanding all the English I hear). Of the three languages, I already knew Nepali, had obtained a smattering of Tibetan in London, and of course knew no Kaike, which is spoken only in Tarangpur and two neighboring villages by about a thousand people altogether. The compromise I had to settle for was to collect extensive linguistic data in all three languages (see Fisher 1973, for example), but to rely primarily on Nepali as the main research tool.

My Sherpa assistant, Changchu, was fluent in Tibetan (he had been born in Tibet) and was able to assist in my difficulties with that language. My Nepali was about as good (or as bad) as that of the villagers, so we felt at home conversing with each other. I therefore worked without any interpreters or research assistants. I had planned to bring one research assistant from Kathmandu and so had engaged a young man who had just completed his M.A. in geography to spend the year with us. He unfortunately had to put his own affairs in order before he could leave, he told us, so we left Kathmandu with the understanding that he would join us in a few days. I gave him enough money for his plane ticket to Pokhara and his expenses until he could catch up with us on the trail or in Tarangpur. I never saw him again.