

# PRACTICING SOCIAL ANTHROPOLOGY IN INDIA

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## ABSTRACT

After completing a BA in social philosophy from Mysore, I went to Bombay to do graduate work in sociology with GS Ghurye, who had been a student of WHR Rivers at Cambridge in the 1920s. At the end of eight years, I became disillusioned with diffusionism and unfocused ethnography. I then went to Oxford, where I worked first with AR Radcliffe-Brown and then EE Evans-Pritchard. At Oxford, I became a structural-functionalist, albeit a somewhat skeptical one. After teaching at Oxford from 1948 to 1951, I returned to India to teach sociology at the University of Baroda. Eight years later, I went to the University of Delhi as Professor of Sociology, and finally moved to Bangalore in 1972 to start, with VKRV Rao, the Institute for Social and Economic Change. As an anthropologist, I am somewhat of a maverick in that I study my own culture and not any distant Other.

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## INTRODUCTION

My becoming a social anthropologist was largely accidental. After passing the intermediate examination at the University of Mysore in May 1933, I was wondering which subjects to take for the BA course when fate walked in: TLA Acharya, a Marxist and journalist and a friend of my eldest brother, Parthasarathy. Acharya was working for the newspaper *Hitavada* in Nagpur, and

he was visiting Mysore on vacation. My brother had borrowed the university handbook so that Acharya would have an idea of the courses available. Acharya thumbed through the pages of the solid volume, and after a while, recommended to me the BA Honors course in social philosophy because, as he put it, it was “humanizing.”

The Honors course in social philosophy was not very popular, unlike Honors in economics or history. I applied for a seat and had no difficulty getting in. The program seemed to have been designed to train encyclopedists. It was divided into courses on so-called minor and major subjects. The minor subjects included, in addition to English and a second language, social psychology, social anthropology, comparative politics, and Indian economics, while the major subjects included philosophy of religion, ethics and history of ethics, political philosophy and its history, sociology, Indian social institutions, Indian (Hindu) ethics, Indian (Hindu) political theory, and finally, a course in which I had to write an essay on one of the three or four topics mentioned in a question paper.

I was only 16 when I entered the college in June 1933—too young and immature to benefit from the rich feast of knowledge spread before me. I was taught by conscientious—if underpaid—teachers. I was a diligent student but a poor examinee, passing the course at the level of a high second class.

I had vague ideas of doing graduate studies in sociology in Bombay under GS Ghurye, who had been highly recommended to me by MH Krishna, a historian and archaeologist at Mysore. Sociology, however, was neither popular nor prestigious in India until the 1950s, and anthropology was under a cloud because nationalist Indians regarded the subject as an instrument of colonial rulers who wanted to keep the tribals distinct from the mainstream population. I decided to do a law degree on the side as insurance against unemployment, even though law was poor insurance with too many lawyers chasing too few briefs in the depressed 1930s.

My ideas about pursuing sociology and law in Bombay took shape when the government of Mysore announced in the summer of 1936 that it would hold examinations to select two officers for the Mysore Civil Service. Recruitment to the officer cadre had resumed after a lapse of several years, which meant that everyone who was a graduate and had not crossed the age limit would compete for a position. I knew that many candidates who had obtained the first class level in the BA or BSc program would be in the fray, and that my own record as an examinee was not impressive. Above all, I was sick and tired of taking exams, and highly competitive ones at that. Luckily for me, my eldest brother supported me in my resolve to study sociology and law. I embarked on the long journey by train to Bombay to the disappointment of my mother, who wanted her son to be an officer in the Mysore government.

## GRADUATE STUDIES IN BOMBAY

I presented myself before Ghurye at the University School of Economics and Sociology (USES) on the afternoon of June 17, 1936. Ghurye seemed informal. He told me that he had been the examiner of the sociology paper at my Honors examination and that he had given me 66 marks, the highest among the five candidates. This was a surprise because I had answered only four out of the mandatory five questions, and besides, I thought I had done badly in sociology.

I told Ghurye the details of the Honors course, and he told me I should write a thesis. This was music to my ears because I knew I had a better chance at doing well with a thesis than with an examination by papers. Before discussing the topic of the thesis, however, he wanted me to write an essay on LT Hobhouse's *Morals in Evolution*. It was an immense and heavily footnoted tome, and I found it boring. When I met Ghurye after a month or so, he asked me how I was getting on. I told him that I found Hobhouse dull and heavy going. I expected that he would be annoyed, but he was not. Instead, he talked to me about the subject of my research. He suggested that I work on marriage and family among the Kannada-speaking castes of Mysore State. I agreed to undertake the research, in addition to beginning my law degree at the Government Law College.

In the lingo of the Research Hall, where I had been given a table, mine was called a "library thesis." I had to study the ethnographic surveys, gazetteers, and census reports and submit my thesis within three years. Ghurye wrote letters to the university librarian and to the librarian of the Bombay branch of the Royal Asiatic Society requesting that they allow me to use their libraries. I enjoyed my new status as a research student, wandering from one library to another after attending classes in the morning in the Government Law College, which was practically next door to the USES.

In addition to the books and reports recommended by Ghurye, I used material in Kannada folklore and fiction on marriage and family. I printed a questionnaire in English, which I later discarded after a social worker I knew picked holes in it. Fieldwork was fascinating to me, and I visited a village for a few days and witnessed a wedding, which I enjoyed. Village India was terra incognita to me even though my family, absentee landowners, annually received its share of paddy from our tenants. This brief encounter with our tenants had increased my curiosity about villagers and their life and culture.

I had to balance my time between studying law and working on my thesis. During the first year, I paid more attention to law than to sociology, passing the first law exam in May 1937. Then I went all out for my thesis but attended the law college and paid the fees to ensure my eligibility to take the final exam

when I would be free to do so. In September 1938, I submitted a 300-page thesis that Ghurye thought, for a brief while, was good enough to be submitted for the PhD.

Ghurye was keen that I should publish my thesis. He secured a grant from the university to pay for the cost and urged me to secure a foreword from the vice chancellor at the University of Mysore in the hope that it might help me to secure a lectureship there. After obtaining the foreword, I went to a local publisher who agreed to publish the book, *Marriage and Family in Mysore* (Srinivas 1942).

I was concerned about how the book would be received, but, to my relief, all the reviews were friendly, including a longish one that appeared in *Nature* (October 1942). Ghurye, who held *Nature* in great esteem, regarded the review as a feather in his cap (Ghurye 1973, pp. 108–9).

After passing the final law exam in October 1939, I started thinking about a suitable theme for PhD research. When I met Ghurye to get his advice, he told me that the university had instituted a research fellowship in sociology and that he would offer it to me if I undertook a field study of the Coorgs. The Coorgs are the dominant ethnic group in Coorg, a montane district then covered with dense forests lying to the southwest of the princely state of Mysore in South India. The Coorgs have a strong martial tradition, and large numbers of them were recruited by the Indian Army during World War II. Their traditional dress, customs, and life-style are different from their neighbors, and their distinctive appearance and customs had been commented upon by outsiders. At the time of my study (1940–1942), the Coorgs were busy defining their collective identity in the Indian subcontinent. They considered themselves Indo-Aryans, descended from the early Aryan invaders, and they interpreted their customs and ritual as derived from the Vedas.

Ghurye's interest in the Coorgs was the result of his having read about their ancestor shrines (*kaimada*) in the *Manual of Coorg* (Richter 1870). He wondered whether the *kaimada* had been derived ultimately from the Egyptian pyramids. Far-fetched as this may sound to a modern anthropologist, Ghurye's Egypt fixation was inspired by Rivers, who, during the last phase of his undoubtedly remarkable career, had embraced an extreme form of diffusionism under the influence of the anatomist G Elliot-Smith and his colleague, WJ Perry. RH Lowie (1937) summarizes the basic ideas of the diffusionists: "(1) Man is uninventive; hence culture arises only in exceptionally favorable circumstances, practically never twice independently. (2) Such circumstances existed only in ancient Egypt; hence elsewhere culture, except some of its simplest elements, must have spread from Egypt with the rise of navigation. (3) Civilization is naturally diluted as it spreads to the outposts; hence decadence has played a tremendous role in human history" (p. 161).

I did not find any evidence to link Coorg kaimada to the Egyptian pyramids; if anything, they were more likely to have been influenced by the Veerashaiwas—popularly known as the Lingayats—who built graves over their dead. Lingayat rajas ruled Coorg from 1633 to 1834, and Coorgs, who formed most of the armies of the rajas, came under Veerashaiva influence. This was especially true of rich and aristocratic Coorgs (Srinivas 1952, pp. 159–60).

Some areas of main concern to Ghurye, during the period I was his student (1936–1944), were kinship, caste, social organization, and tracing the distribution of cultural traits, all of which were derived from Rivers. Ghurye, however, had studied Sanskrit before he took up sociology, and when he became a professor in Bombay he promoted the use of anthropological concepts to interpret ritual, legal, and other material in Sanskrit in the manner of the British Classicists. He did this in his own work and through the work of his students, but the value of his contributions was limited because he did not shake off diffusionism and become familiar with the theoretical advances made by Radcliffe-Brown, Malinowski, and others.

Ghurye, holding as he did a professorship in cosmopolitan Bombay, attracted students from all over the subcontinent, and he directed them, when he could, to update the ethnography of different regions. In this way he retrieved a great deal of information. Further, though he was an armchair anthropologist, he knew the importance of fieldwork in a country as culturally rich and diverse as India at a time when India was beginning to change. His efforts suffered, however, because he remained pre-Malinowskian in his approach. He had not heard of participant observation. Data were collected in interviews of knowledgeable informants, preferably during short field trips. A few of his students also carried out surveys of villages to determine their living conditions.

Ghurye regarded the whole of Indian civilization as his bailiwick, from the tribals of central India and the northeast to the study of Sanskrit literature, the Sadhus or renouncers, the worship of Ganapati, and even Rajput architecture. He realized that no student of Indian culture and society could afford to divide the study of India into two distinct academic fields: social anthropology and sociology. Even the remotest tribes in the subcontinent have some relations with caste Hindus and have been influenced by the ideas of Hinduism, Islam, or Christianity. Equally important but less acknowledged, however, is that tribal ideas and practices may be found among the high castes living in villages and towns, and that they surface particularly during crises.

### *Relations with Ghurye*

In hindsight, I realize that when I first met Ghurye in June 1936, he probably saw in me a student who could promote a few of the goals he had formulated

for sociological research in India. After my MA thesis was finished, Ghurye asked me to make an account of the feasts and fasts of the Kannada Brahmins, and then to make a collection of Tamil proverbs translated into English. All this was to be done before I began my fieldwork among the Coorgs in June 1940. My fellowship came to an end, however, in June 1942 and, much to my regret, my fieldwork had to be carried out in short trips because of a severe illness that I contracted during my first stay in Coorg. A few weeks before the fellowship concluded, Ghurye told me that he wanted me to be his research assistant, succeeding CA Hate. I accepted. The research assistantship was a step up from the research fellowship, and it lasted from June 1942 to June 1944.

During the first year of the assistantship I toured the Tamil-speaking areas of Madras Presidency to collect data on kinship and marriage among the various castes, folk songs, and photographs from the Madras Government Museum, which illustrated the varieties of dress worn at different periods in the history of the Tamil people. After I returned to Bombay, my first task was to write up the material on kinship and marriage and then to translate the folk songs. I wrote a paper on Tamil folk songs for publication in the university journal (Srinivas 1943, 1944). In addition to these duties, I had to be available to Ghurye during office hours to hunt up references for a paper or book he was working on. While I was his research assistant, Ghurye was working hard to finish one book so he could begin another.

I toured the Telugu-speaking areas of Madras Presidency in the academic year 1943–1944 to repeat the work I had done in the Tamil-speaking areas. Just before leaving for my tour in August 1943, an interview was held to appoint two lecturers in the department. At Ghurye's urging, I sent in my application. After finishing my tour of the Telugu areas and before returning to Bombay, I briefly visited Mysore, where I learned that I had missed the lectureship. Upon my return, Ghurye was all praise for my fieldwork, and he told me that I was now able to fuse data and theory. I did not show any pleasure at receiving the compliments, however, and further, I expressed my anxiety about not making any progress with my thesis. I told him I needed all my time for my thesis, after writing up the material I had collected in the field. Ghurye seemed to agree.

I worked very hard and completed my tasks, including the paper on Telugu folk songs (Srinivas 1944, 1945) in four or five months after my return. My expectation, however, that I would be allowed to work at my thesis proved to be wrong. Ghurye frequently called me to his room to do one chore or another. Even worse, he started to use me as his amanuensis. He saw that I disliked it, but it did not stop him. One afternoon I told him that I was very anxious about not completing my thesis and that I wanted to be able to devote all my time to it. He at once wrote to CN Vakil, the Director of the School, appointing a colleague of mine to take my place.

Ghurye's termination of my research assistantship was sudden and unexpected. I did not have any money to support myself in Bombay while completing the thesis. My brother, however, wrote from Mysore saying that I should not worry, and that he would send me money. I knew how hard up the family was and felt guilty about taking money from my brother. I worked very hard and managed to submit a two-volume thesis—888 pages—in December 1944. The external examiner was Raymond Firth (now Sir), and he not only passed me but wrote a very favorable report (Ghurye 1973, p. 115).

During the closing months of 1943 and throughout 1944, I was extremely worried about my future. I had no job, nor was there any likelihood that I would procure one. Even more disturbing, I was disillusioned with the kind of sociology I was doing: Tracing Coorg ancestor shrines to ancient Egyptian pyramids seemed to me as absurd an enterprise as tracing the origin of the Coorgs to the Vedas without historical and archaeological evidence. I was bitter that I had begun with the idea of becoming a sociological theorist but had ended up as an antiquarian under Ghurye's tutelage.

In short, I was unemployed and in the midst of an intellectual and moral crisis. It was at this juncture that I came across, quite accidentally, Ruth Benedict's *Patterns of Culture* (1934). I found the book fascinating, and its main idea of looking at cultures holistically and characterizing each culture on the basis of its dominant theme was a refreshing contrast to looking at cultures as assemblages of myriad discrete elements drawn from different parts of the world and across millennia. Benedict had also demonstrated the integral relation between a culture and its members, showing how in each culture some personality types are preferred while others are considered undesirable. It is possible that the marked preference for martial virtues in Coorg culture had made me sensitive to Benedict's thesis, especially because the Lingayats were a thorough contrast to the Coorgs, as were the Brahmins. Benedict appeared to offer me a more fruitful way of looking at culture and society. Looking back, I am surprised that as late as 1944 I was unacquainted with the functionalism of Malinowski or Radcliffe-Brown, even though I had read Malinowski's *Crime and Custom in Savage Society*.

Toward the end of 1943 or slightly thereafter, I began to consider going abroad to study for another PhD. I even mentioned the idea to Ghurye, who not only did not discourage me but recommended Oxford because Radcliffe-Brown was a professor there and because he was a "seeded functionalist," whatever that meant. However, I suspect that his preference for R-B, as Radcliffe-Brown was known, was probably due to R-B having been a student of Rivers. I applied to Oxford and Columbia, and I received an early reply from Oxford, even though England was in the thick of the war. The termination of the research assistantship strengthened my determination to go to Oxford,



though I had no clue about how to pay. I applied for a Tata loan scholarship but was turned down.

I did not know at the time I applied that R-B was away in São Paulo and that Daryll Forde was serving in his place. Forde wrote asking me to send him a brief proposal on the theme of my DPhil research. I wrote back stating my reasons for wanting to work on the idea of culture patterns, comparing available material on three South Indian communities—the Coorgs, the Todas, and the Chenchus—about whom C von Fürer-Haimendorf had published a monograph in 1943. Forde approved of my proposal, adding that I would be admitted initially to the BLitt degree with the prospect of transfer to the DPhil at the end of three terms, provided I was found fit for the higher degree.

I went home to Mysore in the first week of January 1945 and spent the time doing the various things necessary to secure a passport and passage to England. The family raised enough money to meet the cost of passage and my stay in Oxford for six months. Things started moving fast, and I learned that my ship would sail from Bombay on April 10, 1945.

I reached Bombay a few days before the departure date. I saw Ghurye and wished him farewell. I thought the meeting would be brief and formal, but it was not. Ghurye told me that I was going to England at the wrong time. There would be no students in British universities because of the war and no intellectual stimulation. He wanted me to stay back: He was going to create a lectureship for me in the department. I was taken aback by Ghurye's talk, and it seemed absurd to think of abandoning my plans on the eve of catching the boat. I told Ghurye that I would return if he created a lectureship for me in the department, and took leave of him.

## RESEARCH STUDENT AT OXFORD

After more than a month of wartime sea travel, I presented myself before R-B at the Institute of Social Anthropology in Oxford to learn what was expected of me as a research student. R-B appeared forbidding, and my initial encounters with him were far from propitious. There were many reasons for this, but the most important was probably R-B's antipathy toward Benedict's theoretical stance, of which I was totally ignorant until much later. In addition, even though I was totally disenchanted with diffusionism, my old habits of thought surfaced occasionally during his lectures or supervision sessions, which irritated him.

After the Trinity term ended in July, R-B left for his Welsh village, taking with him my thesis and asking me to write a paper on culture patterns to be shown to him on his return. I worked hard during the summer, becoming familiar with the various libraries in Oxford, and I prepared a longish paper on cul-



ture patterns. I discovered that there had been a fair amount of discussion on culture patterns in the United States. During my work I became concerned that I did not have a deep enough grounding in psychology to tackle culture patterns to my satisfaction.

I met R-B soon after his return to Oxford in October. He told me that he had read my thesis during the vacation and that I wrote very well. "Did you go to an English school?" he asked. I told him "no" and handed him the paper on culture patterns, wondering what he would think of it. A few days later he informed me that he was satisfied with the paper and that I could go ahead. I felt greatly relieved, and I then asked him what he thought of my proposal. Sharp came the reply: "It is a waste of time for a man of your scientific talents to work on culture patterns." I then asked him what he thought I should do. He said that there was a considerable amount of material in my thesis on ritual and religion and that I should look at it from the structural-functional point of view. Expecting that he might ask me to go back to Coorg to collect more data, I told him that I had no money, and further, given the war conditions, it was impossible to get a passage to India in the immediate future. He assured me that there was no need for collecting more data. There was plenty of information in my thesis, and he wanted me to look at it from a new point of view.

I then read Durkheim's *Elementary Forms of Religious Life* (1954), R-B's *The Andaman Islanders* (1933), Fustel de Coulanges's *The Ancient City* (1956), Robertson Smith's *The Religion of the Semites* (1927), Evans-Pritchard's *Witchcraft, Oracles and Magic Among the Azande* (1937), and Bateson's *Naven* (1936). I made myself familiar with R-B's many papers on kinship and Evans-Pritchard's *The Nuer* (1940). I read a lot of other literature, too, in an attempt to catch up with recent developments in social anthropology.

The task of looking at the Coorg material from a structural-functional point of view proved to be exciting. It looked as though the material was crying out for such an approach and analysis. I also had the satisfaction of fleshing out a few simple-sounding but key concepts of R-B's, such as "ritual idiom" and "spread" in the analysis of Hinduism. The social structure of Coorgs, when analyzed, fell into clear, distinguishable units, each with its own cult. The cults formed a hierarchy, from the lineage to Pan-Indian Sanskritic Hinduism.

Coorg ideas of purity and impurity were elaborate, and pervasive in their religious and social life. (It was difficult to draw a line between the religious and social.) I regarded ideas of purity and impurity as part of the "ritual idiom" of the Coorgs, along with *mangala*, a complex of ritual acts that was performed on all auspicious occasions. I accepted for heuristic purposes R-B's idea that ritual was a language, and I applied the few rules he had formulated to decode *mangala*. After I completed my analysis, I felt that I had not probed deeply enough. The feeling remained with me, but two recent verdicts on my effort

have been more positive than I could have dared to hope. Milton Singer has commented that my (Singer 1996, p. 20–54)

chief interest...was in the application of Radcliffe-Brown's theory of ritual to Coorg religious practices and beliefs and their relation to all-India Hindu practices and beliefs. The application goes considerably beyond Radcliffe-Brown's redefinition of the "sacred" in terms of "ritual value," and an analysis of rituals and myths in terms of "symbolic action" and "symbolic thought." It also includes a detailed ethnographic description of the "ritual idiom" and cults of different social units in Coorg—joint family, village, caste; and of the diacritical symbols of age, sex, marital status, caste, sect and other social divisions. His most original contribution to a theory of ritual is his analysis of the hierarchies of ritual purity and ritual pollution among different castes, occupations, and age and sex groups. Srinivas' linking of his analysis to social strategies that different groups adopted for changing their social and "normal ritual status" by changing their ritual practices and beliefs generated the famous theory of "Sanskritization" and "de-Sanskritization" and his later theory of secularization and Westernization.

Jack Goody has written recently that he found the analysis of ritual developed by R-B and me helpful in the analysis of the religion of the LaDagaa in West Africa: "It seemed hardly possible to deal with a rite of passage without building upon Van Gennep's pioneering work, nor did it seem useful to discuss 'symbolic' meanings without recourse to the classic but equally simple techniques developed by Radcliffe-Brown and Srinivas, that is, without being concerned with 'action'" (Goody 1995, p. 125).

Diverse and conflicting elements have gone into the making of Coorg religion, and in my effort to analyze it I used R-B's idea of spread. I had to flesh out the concept, and in the process there emerged such distinctions as local, regional, peninsular, or Sanskritic Hinduism. Sanskritic Hinduism is now widely used by Indianists notwithstanding objections to its usage by some (Fuller 1992, p. 24–28).

The Coorgs regard themselves as a people distinct—even unique—from their neighbors, but they also consider themselves Kshatriyas. In studying their religion I was extending the use of the anthropological approach and concepts to try to understand a world religion and a historic civilization. I embarked on this enterprise without any thought for the difficulties that would be involved, but my naïveté, if not ignorance, paid off. In his foreword to the Coorg book, R-B wrote, "For the student of comparative religion, and particularly for those interested in the sociological study of religion, this book of Dr. Srinivas offers material of exceptional value" (Srinivas 1952, p. ix). Meyer Fortes went further when he said (Fortes 1955): "Even more impressive is Professor

M.N. Srinivas' recent book on *Religion and Society Among the Coorgs of South India* (1952). Here Radcliffe-Brown's methods and theories are ably applied to the religious system of a complex Oriental society. Professor Srinivas shows that there is as close an interlocking of religious institutions, which Frazer would certainly have placed at a higher 'stage' of evolution than Australian totemism, with the family and kinship system, the laws of inheritance and succession, and the principles determining the status of persons, among the Coorgs as among the Australians and Andamanese. He shows how the study of the 'higher' religions can be set free from the crippling trammels of theology, metaphysics and philology and brought within reach of social science." I would, however, qualify Fortes's statement by adding that anthropology ought not to claim that it renders metaphysics, theology, and philology redundant.

R-B retired from his professorship in July 1946, and E-P, as Evans-Pritchard was known, succeeded to the chair in October 1946. I discovered that R-B had spoken well of me to E-P and had suggested that E-P should take over supervising my thesis, which resulted in a brief misunderstanding with Fortes. By October, when E-P took charge as professor, I had more or less broken the back of the conceptual part of the thesis, and only the writing remained. E-P found me too much under the influence of Durkheim and tried to instill some skepticism in me. Apart from this, he approved the draft of my chapters. I submitted my thesis in May 1947, and E-P was present when the DPhil was conferred on me in June. I was his first doctoral student and R-B's last. I regard this as a distinction.

No two persons could be more different than R-B and E-P: E-P was extremely informal and took the trouble to put you at ease, whereas R-B's shyness erected a barrier between himself and others. R-B appeared aloof and distant to most people. He never made small talk, and with anthropological matters he gave the impression that he knew all the answers. E-P had a keen sense of the absurd, which, along with his engaging informality, brought me out of my shyness and diffidence. I could talk to him like an equal across barriers of race, religion, and diversity of cultural background. After I received the DPhil, he congratulated me and then told me that I was his equal(!), and that I should no longer call him "Professor." He requested that I call him E-P.

A few weeks before I was due to sail for India, E-P told me that R-B was visiting Oxford and that he would like to see me in his rooms in All Souls in the evening. When I went there I found R-B and E-P engaged in animated and friendly conversation. Soon after I sat down, R-B said that E-P was planning to create a lectureship for me in Oxford, but before going ahead he wanted to be sure that I would accept it. I was not prepared to hear anything like this, and my initial reaction, I regret to say, was bewilderment. I do not know whether it was

interpreted as hesitance. R-B went on to tell me that I was not yet ready to teach in India and that I needed to spend a few more years at Oxford. I then expressed my thanks to E-P and R-B and walked out into the street, happy and confused.

I returned to India in August 1947, a few days before the 15th, when it celebrated its independence. In mid-November, I received a letter from E-P informing me that I had been appointed university lecturer in Indian sociology, beginning January 1, 1948, and that I could spend the first year of my lectureship studying a village of my choice in India. I had mentioned to E-P that I wanted very much to study a multicaste village using the method of participant observation. E-P had remembered it and had generously provided for it. A formal letter of appointment from the registrar of Oxford University soon followed.

I started looking for a suitable village some time in December but settled in one only in February 1948. The assassination of Mahatma Gandhi had delayed my move by a fortnight. The villagers insisted that I should not start my fieldwork during the mourning period for the Mahatma.

Fieldwork in Rampura was a profound experience, influencing my thinking not only about Indian society and culture but about all societies, and even about the nature of sociological understanding itself. I enjoyed my field experience, and it was with reluctance if not sorrow that I bid farewell to the villagers toward the end of November.

## TEACHING AT OXFORD

I returned to Oxford in mid-January 1949, in time to start teaching during the Hilary Term. Much water had flowed down the Isis during my absence, and I found the university crowded with students and bustling with excitement. There were several graduate students in the department, including a few from the United States and Africa. A few of E-P's students at Cambridge had followed him to Oxford. The most prominent were Godfrey Lienhardt, Emrys Peters, and David Pocock. Among the students were Mary Douglas, Jack Goody, and John Middleton—all future leaders in anthropology. The Friday seminars were open to outsiders, and after animated discussions in the smoke-filled seminar room, many participants adjourned to the King's Arms to continue discussions in small groups over mugs of beer.

Unlike R-B, E-P was an Oxford man who felt at home in the university, and who knew his way around it in more senses than one. He tapped the Scarborough and Colonial Development and Welfare funds to create lectureships and support field research. He was very keen that anthropology should study non-Western civilizations. E-P's own background in modern history at Oxford and

his study of the Sanusi of Cyrenaica (1949), undertaken during the War years, had strengthened his belief in the need for such studies. He was also aware that Oriental Studies commanded considerable prestige at Oxford, and he valued association with the Orientalists.

About these early Oxford years, Jack Goody has written in his memoir on Meyer Fortes: "Together with Evans-Pritchard, Gluckman and Srinivas, they built up a powerful department which attracted students from all over the world at the moment when anthropology, and particularly African anthropology, was expanding rapidly" (Goody 1993, p. 283). Significant contributions to Indian anthropology at Oxford were also made by Louis Dumont and David Pocock. Gluckman went on to Manchester to build an outstanding department, where I was a welcome guest.

It is ironic that Oxford soon proved to be the place where the basic postulates of Radcliffe-Brownian structural-functionalism were rejected. The leader of this move was E-P, who had acknowledged earlier, in his preface to *The Nuer*, Professor AR Radcliffe-Brown, "whose influence on the theoretical side of my work will be obvious to any student of anthropology" (1940, p. viii). The rejection that began in classrooms and seminars found public expression in the six talks on social anthropology that the BBC invited E-P to give. First came the rejection of the idea that social anthropology was a "natural science" (Evans-Pritchard 1951): "Up to the present nothing even remotely resembling what are called laws in the natural sciences has been adduced—only rather naive, deterministic, teleological and pragmatic assertions. The generalizations which have so far been attempted have, moreover, been so vague and general as to be, even if true, of little use, and they have rather easily tended to become mere tautologies and platitudes on the level of commonsense deduction (p. 57)....[S]ocial anthropology studies societies as moral or symbolic systems and not as natural systems, that it is less interested in process than in design, and that it therefore seeks patterns and not laws, demonstrates consistency and not necessary relations between social activities, and interprets rather than explains. These are conceptual and not merely verbal differences (p. 62)." I had reservations about E-P's view that anthropology was "less interested in process."

E-P then went on to reject the other major postulate of functionalism, namely, the irrelevance of history for arriving at a sociological understanding of phenomena. E-P asserted that history was essential for a "fuller understanding of social life" and that social anthropology was "much more like historical branches of scholarships—social history and the history of institutions and of ideas as contrasted with narrative and political history—than it is to any of the natural sciences....[E]ssentially the method of both historiography and social anthropology is descriptive integration, even though anthropological synthesis

is usually on a higher plane of abstraction than historical synthesis and anthropology more explicitly than history aims at comparison and generalization” (Evans-Pritchard 1951, p. 61).

E-P returned to the subject in his Marett Memorial Lecture (June 1950), where he was even more scathing in his attack on the natural science view of social anthropology: “The concepts of natural system and natural law, modeled on the constructs of the natural sciences, have dominated anthropology from its beginnings, and as we look back over the course of its growth, I think they have been responsible for a false scholasticism which has led to one rigid formulation after another. Regarded as a special kind of historiography, that is one of the humanities, social anthropology is released from these essentially philosophical dogmas and given the opportunity, though it may be paradoxical to say so, to be really empirical and in the true sense of the word, scientific. This I presume is what Maitland had in mind when he said that ‘by and by anthropology will have the choice of being history and being nothing’” (Evans-Pritchard 1962, p. 26).

Where did I stand in this controversy? To me E-P’s argument that social anthropologists had produced nothing remotely resembling laws in the natural sciences was self-evident; in addition, I had never really believed in the irrelevance of historical data for sociological explanation. Real history, however, had to be distinguished from conjectural history, and the latter had to be rejected. In addition, sociological explanation did not consist in tracing the origins of institutions. I accepted, however, the functionalist idea of interdependence of institutions and that such interdependence enabled the anthropologist to talk of “social systems.” I accepted them only as heuristic devices that enabled me to understand and better analyze social phenomena. Indeed a major consequence of anthropological (or sociological) training ought to be to enable the anthropologist to view institutions in relation to one another, and in relation to the whole, even if the whole happens to be, or is assumed to be, the anthropologist’s own construct.

E-P had spent the war years in Sudan, Egypt, and Libya. Freed from the compulsions of academic routine, he had time to think about all aspects of his work, including the postulates of functionalist anthropology. It was during the war years that he became a convert to Roman Catholicism, which profoundly affected his *weltanschauung*, including his views about the nature of social anthropology. He rejected all forms of determinism, Marxian as well as Durkheimian. He also rejected rationalism, which equated religion to superstition. (He once told me that he felt that he had more in common with the Indian philosopher Sir S Radhakrishnan than with two other Fellows of All Souls, the socialist GDH Cole, and the logical positivist, AM Quinton, both of whom were antireligious.)

## RETURN TO INDIA

I left Oxford in June 1951 to take up professorship of sociology in the MS University of Baroda. I was leaving one of the oldest universities in the world to occupy a newly established chair in a new university. Baroda University was a new world to me, but I slowly learned how to go about my work of building a university department that was active in teaching and research. I paid attention throughout my academic career, not only to attract promising students and committed colleagues but to evolve a syllabus in comparative sociology suited to Indian conditions, avoiding parochialism. I also ignored making a distinction between social anthropology and sociology, to the annoyance of some of my anthropological colleagues. I did my utmost to encourage field studies using the method of participant observation. India was changing fast, and chronicling the rich and diverse culture of the country was a major objective of mine.

I had modest success in my endeavor, and I left Baroda early in 1959 for Delhi, where again I was appointed to a newly established chair. Delhi was a far more cosmopolitan city than Baroda, and the atmosphere in the university was propitious for building a first-rate department. Delhi attracted students from all over India, and I also had visiting students and faculty from abroad who added to the liveliness of our weekly seminars. In less than 10 years, my department was recognized by the University Grants Commission as a Center of Advanced Study in Sociology, which meant I got more money for expansion of the faculty, visiting fellows from other universities, scholarships for students, and strengthening the library. I left Delhi in May 1972 to start, along with the distinguished economist and educationist VKRV Rao, the Institute for Social and Economic Change in Bangalore. I am happy that today the department in Delhi continues to be a center of excellence for sociological studies. A great deal of my time and energies have been devoted to building up departments and institutions and promoting my kind of sociology. My own research has suffered much as a result.

## SANSKRITIZATION AND WESTERNIZATION

In the late 1940s and early 1950s, Milton Singer, along with Robert Redfield, started to work on an "extended" method for a social anthropology of civilizations. According to Singer, "the suggestion that the field methods and concepts of social anthropology might be extended to a study of the social organization of the Great Traditions of these civilizations was greeted with skepticism. Some Sinologists, Indologists, and Islamicists expressed more confidence in the desirability and feasibility of such a development than did anthropologists. In this climate of opinion the appearance of M.N. Srinivas' *Religion and Society among the Coorgs of South India* in 1952 was a decisive event. It was the



first anthropological monograph to show us how a Great Tradition (of Sanskritic Hinduism) could be analyzed within a social anthropological framework, and it set going much of what follows in this volume” (Singer 1972, pp. xii–xiii).

Singer wanted me to prepare a paper on Sanskritization for a seminar he was organizing in Poona in July 1954, and the outcome was “A Note on Sanskritization and Westernization” (Srinivas 1994, pp. 42–62). As it happened, I could not attend the seminar, and I met Singer only much later in Madras. One of the purposes of Singer’s stay was to prepare the ground for Redfield’s visit to India in 1955, and Singer and I planned, with the help of A Aiyappan of Madras, a seminar to be held in Madras in October 1955. Leading Indian anthropologists, Sanskritists, and others were invited to the seminar. Redfield was present and took an active interest in the discussions. My paper proved incendiary: The Sanskritists condemned my use of the word “Sanskritization.” The only people using it at the time were Aiyappan, a non-Sanskritist, and Ed Harper, a young anthropologist from Cornell who was studying a village in Western Karnataka, who remarked that the concept helped him to understand what was happening around him. The term has been uniformly condemned by Sanskritists everywhere, but in spite—or because—of that, Sanskritization has been used extensively by those writing on social and cultural change in South Asia. It has even made its way into the *New Shorter Oxford Dictionary* (1993).

I first used the terms Sanskritization and Westernization in the context of cultural and social change and social mobility. When an individual *jati* (a local endogamous unit of the caste system), or a section of a local *jati*, captured political power or became wealthy, over a period of time he or the group emulated the customs, ritual, and life-style of a higher caste. Eventually a myth, or *purana*, came into existence claiming noble origins for the caste and changing the caste’s name by adding a suffix characteristic of one or another twice-born *varna* [a hierarchical division of Vedic society into four orders: Brahmin (priests), Kshatriya (warriors), Vaishya (traders), and Shudra (menials)]. This is the classic form of Sanskritization.

Sanskritization occurred right from the earliest times in Indian history, and the *varna* order of Kshatriya has been generally filled by groups that captured power by taking advantage of the fluidity that was characteristic of the political system until the establishment of British rule, which precipitated the end of local and regional political struggles. Similarly, with the development of trade and urban centers in parts of the country, rich traders claimed the rank of Vaishyas, the third twice-born *varna*.

Thus individual *jatis* or sections of *jatis* were able to move up in the caste system by Sanskritizing themselves. That there was provision for individual castes to move up contributed to the survival of caste through millennia. Al-

though individual castes moved up (or down), the system as a whole was stationary. Further, Sanskritization proved to be an instrument for absorbing individual tribes into the Hindu fold, many of them entering the Kshatriya order. Such absorption of tribes occurred everywhere in India, except in the northeast.

In the context of modern India, mobility involves not only Sanskritization but also Westernization. In several parts of the country, the higher castes took the lead in Westernizing their life-style, and while the higher castes were Westernizing, the so-called lower ones were Sanskritizing. This should not be interpreted to mean that the upper castes were throwing out their traditional culture or that the lower were not Westernizing. Both were occurring in each category, but since Western education had spread more widely among the upper castes and more of them were in white-collar jobs, Westernization was more conspicuous among them. The improvement of communications, the activities of holy men, the popularity of pilgrimages, and the spread of education in rural areas all contributed to the increased popularity of Sanskritization.

Since my ideas of Sanskritization and Westernization had created considerable interest among anthropologists and Indianists, I decided to make an overview of social change in modern India using the concepts of Sanskritization and Westernization (Srinivas 1996). One of the features that stood out in my new study was the crucial role played by dominant, landowning castes in the transmission of cultural forms, ideas, and patterns of behavior to the people living within their jurisdiction. They favored the spread of some forms and ideas while they frowned on certain others, and these elements varied from region to region. However, the influence of the dominant castes was occasionally circumscribed by the presence of a nearby monastery, great temple, or center of pilgrimage.

Westernization is a multifaceted concept, and different groups might choose a facet congenial to it. Common to all facets, however, is secularization. Because of Western education, urbanization, and an occupation that required a journey to work and regular working hours, life-styles have undergone rapid changes, along with the decline of ideas of purity-impurity in extramural contexts and the enclaving of ritual. Simultaneously, *sanyasis* (individuals who renounce the world, wearing traditional ochre robes) and heads of monasteries are engaged in social welfare activities promoting education, providing medical relief, undertaking rural development, and building old-age homes. Some of them mix freely with politicians, cultivate the media, and travel abroad to meet their followers and promote religious/sectarian activity. The role of the renouncers seems to have undergone a revolution in India, this world claiming their attention more and more.

India's large and growing middle class is becoming increasingly Westernized. Westernization in one form or another is seen as essential to upward mo-

bility. Consumerism and gadgetry have become tangible symbols of a Westernized life-style. Sanskritization by itself, unaccompanied by a few symbols of Westernization, is considered synonymous with backwardness and poverty.

A most interesting and important feature of recent assertions of equality by the Dalits (ex-Untouchables) is, ironically, through Sanskritization. For instance, Neera Burra, who studied the Mahars of Maharashtra, reports that Mahars “deliberately practiced hitherto forbidden rites and rituals as a means of asserting their right to equality knowing full well that they [the higher castes] could do nothing about it. There was a certain pride in this [assertion of] equality.” It is ironical that “As far as upper caste Hindus were concerned, conversion to Buddhism was not resented as much as attempts by Mahars to emulate their customs, ritual and dress” (Burra 1996, p. 168). GK Karanth (1996) reports a similar situation in rural Karnataka.

The Constitution of free India became effective in 1950, and it affirmed the equality of all citizens before the law, abolished Untouchability, and declared its practice in any form an offense punishable by law. The Constitution also provided for the representation of the ex-Untouchables (Scheduled Castes) and tribes (Scheduled Tribes) in legislatures, by reserving seats for them in proportion to their population. The SCs and STs also enjoyed reservation of jobs in government and places in educational institutions. Because of pressure brought in by leaders of the backward castes, the Constitution provided for reservation of places in educational institutions and jobs in government, for the “socially and educationally backward classes” (also referred to as “other backward classes”). The courts have consistently interpreted backward classes as backward castes. In November 1992, in a landmark judgment, the Supreme Court of India declared that the total quantum of reservation of jobs and educational places should not exceed fifty percent of the total.

## WHITHER CASTE?

During the early 1950s, the assertion of the equality of all citizens before the law, the abolition of Untouchability, affirmative action on a large scale for “the weaker sections of the society,” and the undertaking of a massive and comprehensive program of development all produced a sense of euphoria particularly among the urban intellectuals, who believed that their country was becoming modernized rapidly and that caste, along with Untouchability, was on its way out.

It was in this context that I delivered, in January 1957, my presidential address on “Caste in Modern India” to the Anthropology and Archaeology Section of the Indian Science Congress meeting in Calcutta (Srinivas 1994, pp. 15–41). The speech was read for me because I was at the University of Califor-

nia at Berkeley at that time on a Rockefeller Fellowship. I argued in the address that caste was not on its way out but that it was playing an active role in politics, and that there was rivalry between important castes to capture political power and secure government jobs. I also pointed out that castes were being mobilized by politicians for securing votes. Two years before the address, I had written that "the coming of elections has given fresh opportunities for the crystallization of parties around patrons. Each patron may be said to have a 'vote bank' which he can place at the disposal of a provincial or national party for a consideration which is nonetheless real because it is not mentioned" (Srinivas 1955, p. 31). It is interesting to note that the phrase "vote bank," which emerged from the anthropological study of a village in South India, has become so popular that no journalist reporting an election fails to use it.

Because of the various forces acting on caste during the past hundred years or more and, in particular, since independence, jatis or sections of jatis have broken free from their village or other local cages to form large jati categories straddling large tracts of the country to better compete for such scarce resources as political power, economic opportunity, and education. Success in such efforts results in upward mobility for the jatis that come together. I termed this process "horizontal integration" (Srinivas 1996, p. 115). As political mobilization proceeds apace and more and more castes become aware of the opportunities available to them for securing scarce resources, competition among them will become even more fierce. Such competition offers a total contrast to the cooperation between jati sections living in a village, which so essential to producing the basic needs of the people. Indications are that this cooperation is likely to weaken further. However, with the emergence of large castes competing with one another to secure secular benefits, the weakening of purity-impurity ideas and, finally, the ideological rejection of hierarchy, both in the Constitution and by large sections of the people, all point to a systemic change. As caste as a system begins to break down, individual castes are likely to continue as they secure a variety of benefits for members in addition to giving themselves a sense of identity. As India becomes more urban and heterogeneity becomes the norm, ethnic—including caste—identities are likely to assume much greater importance.

## VILLAGE STUDIES

Village studies became a major interest of anthropologists (and sociologists) in India in the 1950s. They greatly enlarged the field of anthropology, and anthropologists presented a more rounded picture of the peasant and his world, unlike other scholars who produced specialized but partial studies of agricultural practices and crop patterns, land tenure systems, subdivision and frag-

mentation of holdings and their effect on production, deleterious effects of the joint family system, wasteful expenditure at weddings and funerals, and slavery to custom. Again, unlike scholars in other disciplines, anthropologists spent long periods in villages learning the local language, winning the trust of the people, and confirming their facts by carrying out censuses and canvassing questionnaires when necessary. Among the more sensitive accounts of anthropologists there was an attempt to look at the world from the point of view of the villagers. However, what was happening in India was part of a worldwide phenomenon: As Raymond Firth has noted, “the vast middle ground of the peasantry, once the province of the economic historian, is almost overrun by students of modes of production, local and national collectivities, ideological systems, theories of exchange and the politics of reputation, religious movements and cults of saints” (Firth 1975, p. 7).

It is pertinent to note that India had begun to change fundamentally when anthropologists embarked on their intensive studies of villages and tribes. Their studies provide future historians of India with information of a depth and quality not available from any other source. Again, according to Firth, “the significance of anthropological field materials as historical record seems often to be undervalued. It may be that future generations of the peoples we have studied will prize our books, not for the brilliance of our theoretical analyses but for the honesty of our descriptive accounts of their society at a given period” (Firth 1975, p. 20).

Village studies also provided a unique window to the study of Indian civilization. For example, one of my interests when studying Rampura in 1948 was to obtain an accurate account of the ranking of local jatis. I found to my surprise, however, that the rank of each jati was not only not clear but was frequently a matter of dispute. The rank claimed for the jati by a member seemed to be at variance with the rank that a nonmember assigned to it. Further confusion was caused by the peninsular grouping of local jatis into right- and lefthand categories. The former regarded the latter as inferior. In the lefthand category was an artisan caste that wore the sacred thread and considered itself to be Brahmin, but its claim was not conceded by any righthand caste. One particular caste of Brahmins in southern Karnataka was regarded as so low that not even ex-Untouchables would accept cooked food or water from them.

In other words, ambiguity of rank is not confined only to the middle ranges of the system, as Louis Dumont has argued in *Homo Hierarchicus* (1971, p. 74, 78), but is pervasive. While there is no doubt whatsoever that caste represents a hierarchy, at the jati level at least, the rank-order is not clear and is the subject of contentious debate. It was only in the Vedic varna system, which comprised only four caste categories, that mutual rank was not only clear but immutable.

Faced with ambiguity in the ranking of jatis, I had to ask myself the reason behind it: While a clear-cut rank order assumes immutability, ambiguity is a precondition for mobility. What then were the sources of mobility in pre-British caste? A protean source was the pre-British political order, in which fluidity characterized the lower levels, providing opportunities for able and ambitious leaders of dominant castes to capture power during periods of political confusion, and then lay claim to Kshatriyahood. While the political order and opportunities for making wealth were both sources for mobility, Sanskritization provided both the idiom of mobility and its legitimization.

Clearly, then, there was dissonance between varna and jati, between the view of caste obtained from the sacred books and that from the field. I assumed dissonance between the "book view" and "field view" to extend to other institutions and areas, particularly when the books were normative in character. It is not unlikely that such dissonance is not confined to Indian civilization but extends to others as well, though it is probably greater in the former because of the unique character of Hinduism, which does not have a single scripture with overarching authority, one prophet, and one god. In the last 25 years or so, however, the book view has come back with a bang, and complementarity between the book view and field view is taken for granted, whereas it ought to be something to be proved in each empirical case.

## THE STUDY OF SELF-IN-THE-OTHER

I am aware that I am an oddball among anthropologists in that all my fieldwork has been carried out in my own country. According to conventional wisdom, anthropology is the study of the Other, and it is widely believed that the study of one's own society is far more difficult than studying an alien society. Another linked proposition is that the study of an alien society enhances the anthropologist's understanding of his own society, if not himself. Finally, if one must study one's society at all, it should be undertaken when one is a seasoned anthropologist with one or two field studies of alien societies under his belt, not when one is a neophyte.

Edmund Leach has argued that the desire to study one's own society, while laudable, is "beset with hazards. Initial preconceptions are liable to prejudice the research in a way that does not affect the work of the naive stranger....[W]hen anthropologists study facets of their own society their vision seems to become distorted by prejudices which derive from private rather than public experience" (Leach 1982). In support of his argument Leach cites the work of four Chinese anthropologists. According to Leach himself, however, three were not studies of own society. The fourth study was by Fei Hsiao Tung, who spent two months (July and August 1936) in a village in the

Yangtze delta about 125 miles southwest of Shanghai. Leach wrote that “the merit of Fei’s book lies in its functionalist style. Like all the best work done by social anthropologists it has at its core the very detailed study of the network of relationships operating within a single small-scale community. Such studies do not or should not claim to be ‘typical’ of anything in particular. They are not intended to serve as illustrations of something more general. And yet the best of such monographs despite the concentration upon a tiny range of human activity, will tell us more about the ordinary social behavior of mankind than a whole shelf-full of general textbooks labeled Introduction to Cultural Anthropology” (Leach 1982, pp. 124–27).

Fei was able to produce within two months a first-rate study of a village in South China, an enterprise that would have taken an outsider several years. Leach has thus contradicted his own thesis about the difficulty of studying one’s own society. Was Leach even aware that he was contradicting his thesis?

When an Indian anthropologist is studying a group or community other than his own local group, he is undertaking a study that is both familiar and strange. Over millennia, Indian civilization has penetrated practically every corner of the country, and the influence of the epics, *Mahabharata* and *Ramayana*, has been ubiquitous (they have also influenced people living beyond South Asia). Indian culture, however, is also extraordinarily rich in diversity of all kinds, including regional, linguistic, religious, ethnic, and caste. Over millennia there have been innumerable movements of groups from one part of the country to another, adding to the diversity of each region. One’s own backyard often displays such similarities as well as diversity. I once summarized this situation: “[A]s an over-protected Brahmin...boy growing up on College Road, I experienced my first culture shocks not more than fifty yards from the back wall of our house....[T]he entire culture of Bandikeri (the area behind our house where lived a colony of Shepherds, immigrants from their village, located a few miles from Mysore) was visibly and olfactorily different from that of College Road. Bandikeri was my Trobriand Islands, my Nuerland, my Navaho country and what have you. In retrospect it is not surprising that I became an anthropologist, all of whose fieldwork was in his own country” (Srinivas 1992, p. 141).

When an Indian anthropologist is studying a different caste or other group in India, he is studying someone who is both the Other and also someone with whom he shares a few cultural forms, beliefs, and values. That is, he is studying a self-in-the-Other and not a total Other, for both are members of the same civilization, which is extraordinarily complex, layered, and filled with conflicting tendencies.

I consider the study of one’s own society not only feasible but essential, for it is best that a culture is studied by both outsiders and insiders. Alone, neither is complete. Each may have its own biases, but the two together may provide a



more complete account. Given that anthropologists have no choice but to study other cultures through the prisms of their own, the need for anthropologists from at least two different cultures to study a single culture becomes a necessity. Anthropology has reached a state when such studies ought to be undertaken. The clash of multiple subjectivities would, to my mind, be better than a single subjectivity, whether that of the insider or outsider.

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