Introduction

An Intellectual Autobiography

I was born in Vienna in 1923, at a time when the Hapsburg Empire had been dismantled and Austria had shrunk to a miserable remnant of its former glories, leaving the city as the hydrocephalic head of a dejected, economically depressed political unit. My father's family had been in Austria since about 1650, while my mother's family were Russians from the Ukraine, who, after participating in the 1905 revolution, were exiled first to France and then to the Mongol-Chinese-Russian borderland. My parents met when my father, an officer in the Austrian reserve army during World War I, was a prisoner of war in Siberia. He seized an opportunity to leave the barracks by volunteering to teach English (of which he knew very little) to my mother's brother, a Russian officer; rather than return to the camp, he made his way to the family home in Vladivostok. Both sides of my family were highly secularized Jews, and in my household it was the virtues of the Enlightenment that were extolled: the great German poets, morality without religion, progressive liberalism, playing the violin. In the Vienna of my childhood, violence and anti-Semitism became increasingly a part of everyday life, but I also benefited enormously from the outstanding elementary school education that had been developed by the Socialists, who controlled the city government.

In 1933 my parents and I moved to Tannwald in the Sudetenland, the site of long-standing interethnic conflict between Czechs and Germans, which intensified with the advent of Nazism. My father was sent

there as manager of a textile factory, charged with rationalizing the production process by introducing speedups, continuous shifts, and other such "improvements." The families that suffered included many of my schoolmates. Witnessing this led me to think of the class struggle not just as a theoretical construct but as something that is ever present, a sociological reality that comes in many different forms.

In the Sudetenland I went to a German *gymnasium,* but the better part of my education came from hiking and bicycle trips through the Central European countryside with my friend Kurt Löffler, son of a family of German journeyman tanners. One of these trips took us to Munich in 1937, where we watched Nazi parades and saw the exhibits both of approved "German Art" and of "Degenerate Art." Kurt was to die as a draftee in the German Army during the final retreat from Russia in 1945.

While my father was in Vienna on business in March 1938 the German Army occupied Austria, and the anti-Jewish pogrom that accompanied it made it clear to him that our European days were numbered. In the summer of 1938 he managed to get me to England, to the Forest School in Essex. In addition to learning English and adapting to the British character-building program that combined organized sports, military training, and memorization of Shakespeare, at this school I discovered natural science. An Anglican divine was brought in to teach biology to two of us, which awakened me to the idea that one could think systematically about natural phenomena. I also began to read Julian Huxley, J. B. S. Haldane, H. G. Wells, Lancelot Hogben, and other members of the so-called Invisible College, who were writing science "for the people" and conveyed to me the notion that science could be used to create a better world.

Early in 1940, when an invasion across the channel seemed imminent, the English rounded up all "enemy aliens," and I was sent to an internment camp at Huyton, near Liverpool. The inmates, most of whom were Jews or socialists, tried to build morale by organizing lectures and discussions on a variety of subjects. I gave a talk on "The Ideology of the Biologist." One of the lecturers was Norbert Elias, who spoke about "Monopolies of Power" and "The Network of Social Relationships." The idea that the individual is born into an established network of people, and in his or her very person is a social phenomenon, was a revelation and opened my eyes to social science.

In late June 1940, soon after Paris fell to the German Army, my parents and I boarded one of the last ships to the United States to start

a new, American life in Jackson Heights, Queens. As a resident of Queens, I was admitted to Queens College that fall and there began my American education. My education about the United States was greatly expanded the next summer, when I hitchhiked to Tennessee to volunteer for work on rural reforestation at the Highlander Folk School. Through this experience, I was exposed not only to the impoverished underbelly of the South but also to the potential of grassroots social activism and democratic idealism embodied in the school.

At Queens, inspired by the great British biologists, I first embarked on the study of biochemistry, but my poor showings in mathematics and organic chemistry soon persuaded me to experiment with other possible majors. I had always liked history and geography, had discovered the existence of sociology at the detention camp, and had made a nodding acquaintance with economics and political science by reading, while on vacation from school, the books produced by the Left Book Club. I thus tried, first, political science, then economics, and finally sociology. One day I walked, quite accidentally, into a class on the anthropology of Asia, offered by Joseph Bram. That course ranged from the history of the Chinese script to discussions of caste in India, and I suddenly became aware that there existed a discipline that dealt with all the things in which I was truly interested.

Three years of war followed this discovery. Because of my boyhood love of skiing and mountaineering, I volunteered for the Tenth Mountain Division and saw combat in the Apennines of Tuscany. The experience was important for me, both in proving to myself that I could be a good soldier and in giving me access to the G.I. Bill of Rights, which made it possible for me to go on to study anthropology. My father, for whom intellectual pursuits were something one did after a day of "real" work, took a dim view of this choice, but I took the gamble and with it entered a world that proved right for me.

I returned to Queens to finish my bachelor of arts degree in 1946, and then, on the recommendation of Hortense Powdermaker, I applied for graduate work at the citadel of Boasian anthropology, Columbia University. When I arrived there, Ralph Linton had just left, and with his departure culture history was once again taught as a dance of atomistic culture traits devoid of economic or political context. The main figure in the department was Ruth Benedict, who represented the culture-and-personality approach then dominant in American anthropology, which aimed to delineate a homogeneous, culturally shaped personality for each distinctive culture. I took several courses with Benedict

and also participated in her project of Research in Contemporary Cultures: I interviewed Austrians within reach of the New York City subway, under the watchful eyes of Ruth Bunzel. There was much that I admired about Benedict, especially her ability to pick up culturally phrased behavior or texts and use them as diagnostic metonyms of general cultural configurations, but at the same time she was concerned neither with history nor with placing cultural configurations in the ambience of their material circumstances. For her, cultures and personalities seemed to exist in some timeless no-man's-land.

The year 1947 saw the advent at Columbia of Julian Steward, who had served as editor of the multivolume *Handbook of South American Indians* at the Smithsonian Institution. Steward was in many ways Benedict's antithesis. He had long pursued research in what he called cultural ecology, focused on the comparative study of relations between environments and the technologies that permitted their human use. In developing the *Handbook,* Steward transformed what had begun as a culture-area compendium into a treatise on ecological adaptations in South America, working out their successive transformations into bands, tribes, chiefdoms, and civilizations.

Several of us graduate students had formed a study group to prepare for the qualifying examinations specified by the program. The group included Sidney Mintz, Morton Fried, Elman Service, Stanley Diamond, Daniel McCall, Robert Manners, Rufus Mathewson, and occasionally John Murra. We had in common that we were all veterans; we also shared sympathies on the political left and interests in expanding materialist approaches in anthropology. We discussed what we read and prepared papers for successive meetings of what in our enthusiasm we called, only half ironically, the Mundial Upheaval Society (MUS). During those years I read three landmark books which suggested that anthropology could gain much from the infusion of Marxian understandings. The first was Karl Wittfogel's *Wirtschaft und Gesellschaft Chinas* (1931), an extraordinary, ecologically oriented study of the Chinese economy, which dissented from the view that China was merely feudal and saw it instead as an instance of the Asiatic-bureaucratic mode of production. The second was Paul Sweezy's *The Theory of Capitalist Development* (1942), which helped me systematize my understandings of Marxian political economy. The third was C. L. R. James's *The Black Jacobins* (1938), on the slave rebellions in Haiti in the wake of the French Revolution, one of the first attempts to write a history of a people supposedly "without history."

Steward was generally supportive of the MUS, and he recruited some of us as field-workers in his Puerto Rico Project, a study of several communities that he thought would exemplify particular salient ecological adaptations in Puerto Rico. I had originally hoped to do fieldwork in Indonesia, preferably in Sumatra, but in those pre-Sputnik days of limited fieldwork funds a bird in hand was worth several in the bush. From February 1948 through August 1949 I worked in Ciales, a municipality in the Central Highlands that had grown the specialty Puerto Rican coffees but was then increasingly shifting to tobacco, in light of the declining demand for those coffees. I worked first in the town of Ciales itself and then in one of its more distant barrios, Pozas, in a neighborhood of coffee haciendas and allied small farms.

My colleague Sidney Mintz had chosen to work in the municipality of Santa Isabel among landless agricultural workers who found employment in Aguirre, the most technified, irrigation-based, rationalized, American-owned sugar plantation on Puerto Rico's southern coast. In an intense correspondence Mintz and I explored the contrasting characteristics of the decaying and undercapitalized estates dedicated to producing an ever-less-marketable crop and of the booming sugar *central* on the southern coast, which eventually led us to typologize the developmental and operational characteristics of these kinds of agrarian estates in the Caribbean world and Middle America (Wolf and Mintz 1957; Wolf 1959b). This collaborative effort provided the foundation for shared personal and intellectual interests thereafter.

Since the coffee haciendas on which my Puerto Rican fieldwork had focused stood in the middle of a neighborhood of smallholders, many of whom depended on them for seasonal work and occasional credit, they also constituted part of my study. In my thesis I referred to them simply as "peasants," a term that had been adopted by anthropologists writing on China, Ireland, Japan, and Mexico quite naively and unself-consciously for "rural folk" everywhere. I was then unaware that drawing analytical distinctions among different kinds of peasants and between cultivators and other rural dwellers would propel me in a major way into "peasant studies."

After finishing my thesis on the Puerto Rico study I undertook a project of my own, in Mexico. During my graduate work I had written several papers on the problem of nation, and I decided to apply my ideas toward understanding the growth of Mexican nationhood. I centered my attention on the Bajío in the state of Guanajuato, where Miguel Hidalgo y Costilla had staged his rebellion on behalf of Mexican

independence from Spain in 1810. Since the silver miners of the village of Santa Ana had played a major part in the uprising, I conducted fieldwork there for several months. This proved problematic, for the government had curtailed national silver production in the U.S.-owned Valenciana mine, as elsewhere, while local miners followed an ancient practice of extracting silver illegally from the closed mine. The situation turned dangerous: the local priest provoked a knife fight with me to prove that Americans were cowards who always fought with pistols, whereas manly Mexicans preferred to fight *mano a mano* with knives. I abandoned direct fieldwork in favor of archival research to define the interplay of core and periphery in the history of the Bajío (1955a).

This work brought me close to another group of Mexicans, who were responding to the problem of how *lo mexicano* was being defined and a new Mexican identity was being shaped—not only a scholarly problem but also one of political urgency. Some were Mexican nationals; others, veterans of Spain's Civil War. Among them were the Aragonese architect and art historian Pedro Armillas and the Catalan ethnographer Angel Palerm, who, inspired by V. Gordon Childe and British-initiated aerial photography, defined a Mesoamerican civilization based on the interplay of irrigation centers underpinning cities and peripheries using alternative ecological arrangements. Palerm went on to work on the wider nature of Mesoamerican ecology and to explicate the role of Indian and non-Indian peasant settlements within it. The Mexican medical doctor and ethnologist Gonzalo Aguirre Beltrán contributed a major political strategy to break the political and market dominance of urban elites by introducing pro-"peasant" government promoters into the towns, in order both to accelerate the acculturative competence of "Indians" and to enhance their capacity for autonomous rule.

My interaction with these anthropologists, especially with Palerm, continued over the years. In the summers of 1954 and 1956 Palerm and I together explored the role of the irrigation system of the Tetzcutzingo near Texcoco in the development of the Texcocan Acolhua domain and contributed two papers (Wolf and Palerm 1955a, 1955b) on this topic. In our subsequent collaboration we pursued our work on Mesoamerican ecology as well as on plantations, which were then seen as the major form of encroachment on peasant agriculture.

Upon my return from Mexico in 1952, Steward invited me to become a research associate on his Project of Cultural Regularities at the University of Illinois. While I was dismayed at Steward's ahistorical move toward modernization theory, the people in the department—Robert

Murphy, Ben Zimmerman, Frederick Lehman, and others—made for lively anthropological discourse.

By 1955 I was eager to move to a new setting and accepted a position at the University of Virginia. Despite a heavy teaching load, the lack of committee work in an institution run by departmental heads and the availability of a good library that was scarcely used made my three years there a productive time for writing. During this time I also met the psychologist Arthur Bachrach and, on his invitation, joined a study group on cybernetics and systems theory.

A year in a replacement slot at Yale University (1958–1959) brought the benefits of conversation with Floyd Lounsbury and an exciting experience of team teaching, along with an ecologist, a psychologist, and a sociologist working on alcoholism, an undergraduate course devoted entirely to understanding the Hopi. From there I was recruited by the University of Chicago, which was then considered the summit of American anthropology. I found the department to be a gerontocracy, with long meetings devoted to trivia and an overload of ritual and obeisance to the ancestors. In 1960 I fled by taking up a new field project.

At this time I changed my fieldwork venue to the South Tyrol in the Italian Alps, initiating a long-term inquiry into the multiple interconnections between Alpine ecology and ethnicity. My strategy was to study two nearby villages differing in language, ethnic identity, and historical trajectory. In 1965 my student John W. Cole joined me in this enterprise. It came to involve a consideration of the historical development of political entities that exercised powerful pulls upon both ecological and ethnic processes in this area (Cole and Wolf 1974).

Upon my return from the field in 1961, I accepted an appointment at the University of Michigan, where I was to remain for ten years. This time was important for me in a number of ways. There was a very good group of cultural ecologists, such as Roy Rappaport and Marshall Sahlins (in his earlier incarnation). The "new archaeology" was developing, with its interest in large social processes. I took part in a biweekly seminar organized by the psychoanalyst Frederick Wyatt, who brought together people from anthropology, history, literature, philosophy, and psychology. I learned a great deal from my interaction with historians through coediting, with Sylvia Thrupp, the journal *Comparative Studies in Society and History*. I worked with a talented group of students who carried out fieldwork in different parts of the Mediterranean under a Project for the Study of Social Networks in the Mediterranean Area, which I co-organized with William Schorger. And then, increasingly,

there was an engagement in political issues—first civil rights, then the Vietnam War—and a sense that anthropology spoke to the issues. The teach-ins (which I helped to initiate in 1965) had a special influence on my professional work: my interest in peasant movements began with a briefing paper on Vietnam, in which I tried to figure out for myself what was going on there.

In my writings since the 1950s, the "peasant question" has constituted a major focus of my concerns. Through my work with Steward and my research on Mexico, I contributed to the typological bent of peasant studies. In the absence of adequate historical information about changes in peasant stratification and alignments, I emphasized the institutional framework through which peasant communities confronted the demands of the conquerors. Gradually, however, this emphasis grew more flexible. In *Peasants* (1966) I dealt with modes of peasant organization as coalitions and associations, thus incorporating a notion of maneuver. I then explored the roles of friendship, kinship, and patron-client factionalism. The political turmoil of the 1960s, in which peasants played a singular part, offered an opportunity to think more systematically about peasant participation in political violence. This led to studies on this topic (1969a, 1971a, 1971b, 1973, 1975; Wolf and Hansen 1967, 1972) as well as to a book, *Peasant Wars of the Twentieth Century* (1969b), on peasant participation in a number of the great political uprisings of our time.

In 1971 I moved to the City University of New York (CUNY), where I taught undergraduates at Herbert H. Lehman College in the Bronx and doctoral students at the Graduate Center. This move was prompted by a change in my personal life: my marriage to Sydel Silverman, who became my life partner, editor, and anthropological counselor. At the same time, returning to the City University was fulfilling for me as a lifelong champion of free public education. The CUNY years brought me an important new circle of colleagues in New York City, who shared a commitment to anthropology both as a comprehensive scientific and humanistic discipline and as a critical tool with which to address social concerns. These years also brought me a new generation of graduate students, who taught me a great deal. The ideas for my last books were developed through teaching graduate courses and participating in seminars at the Graduate Center. Especially useful for me were the faculty-student program seminars, devoted to year-long discussions of such themes as long-distance trade and world-systems theory.

In the 1970s I moved away from my earlier quasi-architectural approach to complex societies. I began to think more systematically about the genesis and spread of forces in the world-system as a whole that underwrote the development of sociocultural entities and provided them the capacity to articulate with one another. I saw these forces as acting to build wider-ranging systems based on what I called kin-ordered, tributary, and capitalist modes of production. These ideas formed the premise of *Europe and the People Without History* (1982).

That book was an outgrowth of anthropology's increasing awareness of global politics in the 1960s. It was said by many that one had to understand nations and states in relation to one another and to the expanding circles of capital that have transformed the world. But little was actually being done to rewrite anthropology in this way. I set out to write a kind of anthropological history of the world, to place the micropopulations studied by anthropologists within this new understanding. It proved to be far more difficult than I had anticipated, because it required major theoretical rethinking and because I found that the histories of all the different areas, which I assumed would be there for me to draw on, had not been written.

The book ended with a discussion of the cultural forms of insertion into the different modes of production, and in so doing it raised new problems of ideology. It became apparent to me that each mode required an ideological definition of who may do what to whom in the operations of the mode, which translated into aspirations to and assertions of asymmetrical power. This became the subject of my last book, *Envisioning Power* (1999). In each of the cases analyzed, an elite caught up in the struggle for power extrapolated from the culturally available stock of ideas and practices a body of specifically ideological conceptions, which underwrote their claims to be masters in the struggle for the promise of an enhanced life. For each case I asked: What kind of historical trajectory accounts for the society's grasping certain events as crisis points, and how do ideological representations respond to these purported crises?

For several years I had been putting together material on a half-dozen different cases that I thought could show how ideological involvement actually operated. Eventually I settled on three, which represent the major threads of my work: the Kwakiutl, which took me back to the roots of American anthropology; the Aztecs, whose practice of human sacrifice had remained a puzzle for me throughout my research on Mexico;

and National Socialism, which formed the backdrop to my early life and left me with a sense that it had to be—and could be—explained. The book fell far short of my ambitions, but it stands as an expression of my central reason for being an anthropologist: to seek explanation for the world as I encountered it.