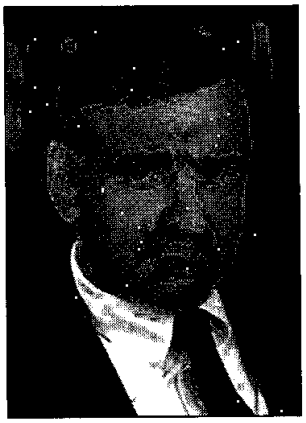
Center, Periphery, and Back

Karl W. Butzer



Courtesy of Karl W. Butzer

Karl W. Butzer (b. 1934) was born in Germany but escaped with his parents at the age of three as a Catholic refugee, residing first in Britain and then in Canada. In 1954, he received his B.S. in mathematics from McGill University and his M.S. in meteorology and geography the following year. He earned his D.Sc. from the University of Bonn in 1957. He taught at the Universities of Wisconsin, Zurich, and Chicago before his appointment as Centennial Professor of Geography and Anthropology at the University of Texas. His research has opened up the fields of paleoclimatology, ecology, and archaeology, and his fourteen books and more than two hundred research articles document his original field-based studies. He is the recipient of many awards across several disciplines, including honors from the Association of American Geographers and medals from the Royal Geographical Society, the Society of American Archaeology, the Geologists' Association of London, the Geological Society of America, and the Archaeological Institute of America. In 1984, he was elected a fellow of the American Academy of Arts and Sciences and in 1996 a fellow of the National Academy of Sciences. His extensive fieldwork includes research in Egypt and Nubia, South Africa and Namibia, Spain and Mexico. He is currently the R. C. Dickson Centennial Professor of Liberal Arts at the University of Texas (Austin).

With Open Eyes

My memory bank begins before age three, when I ran away from a nunnery in Belgium to navigate to the church, a half-mile away, where I knew I would find my grandmother. This was in 1937, when my family emigrated illegally from Nazi Germany. But that goes back to another story, when Prussia united Germany in the 1870s. It promptly began a culture war against the Catholic minority that mobilized Catholics as a political party, working to remove discriminatory laws. My father's family were Catholic activists who saw themselves as Rheinlanders and Europeans as much as Germans, and they resented the centralizing policies of Prussia as well as its militaristic and nationalist ideology. During the early years of the Great Depression (1929–33), German Catholics recognized the parallels between Prussian and Nazi goals or ideals, and studies in electoral geography verify that few Catholics ever voted for the National Socialist Party. After the Nazis took power in 1933, with only 35 percent of the vote, both my parents were implacably opposed to the increasingly totalitarian state.

Working as an engineer for a large industrial company, Father refused to make the Nazi salute or march in the political parades, until he was warned to take part on May 1, 1937, "or else." Shortly before that point, my eight-year-old brother, reflecting attitudes at home, had loudly refused to go along with a Nazi ceremony in the schoolyard. Non-Jewish Germans could not emigrate or even visit abroad without special permission, but my parents decided to leave—illegally. In March 1937, Father was allowed to attend an engineering conference in the Netherlands with nothing but a briefcase, a clean shirt, and ten Marks. He cabled to two Jewish German colleagues then in London, whom he had urged to emigrate two years earlier. They wired back, offering him a job, the paperwork for immigration to England, and money for the trip. A few weeks later my mother crossed into Belgium, ostensibly on a shopping trip, but went on to London. My brother and I were smuggled across the border to stay at the nunnery until our parents had found a place to live in England.

We were fortunate that my father had a marketable profession and connections, so he could escape. Many Jewish Germans could not obtain work permits for England or the United States and were arrested by the Nazis in the

Netherlands or France in 1940, thereafter to be shipped to concentration camps. We had some wonderful years in England, but after Dunkirk in 1940 my father was picked up early one morning by plainclothes police and sent off to an "internment camp." A few weeks later there was another knock on the door, and this time my mother, brother, and I were similarly rounded up. We spent six weeks on the Isle of Man with a minimum of necessities before we were released because my father's boss had connections and insisted that father was vital for the war effort. Again we were lucky because most of the other internees, wives, and children separated from fathers spent the duration of the war in camps in Canada or Australia—even though the great majority were Jewish German refugees.

In January 1941, our family was translocated to Canada, where my awareness of and exposure to ethnic dissonance was magnified. Because I have written about this elsewhere,[1] I will simply suggest that emigration, dislocation, and repeated changes of social environment made me aware of the world outside at an unusually early age.

I enjoyed my first "geographical" experience during the summer I was turning four. Accompanied by one of my father's colleagues, I flew in an airplane from London to Brussels. From a window seat in one of those small and wonderful passenger planes of the 1930s, I first saw the world from above. The images are still vivid in my memory. All the way to the horizon there were fields, lines of trees along winding roads, and villages. All the shapes and colors were different, as light clouds drifted by below us. It was simply beautiful! I could actually recognize human figures while we were ascending, and after we reached cruising altitude, I could still see the cars slowly moving on the roads. But everything seemed tiny, as small as toys. That miniature world from above amazed me most. It was the first time that I was actually aware of how things became smaller and smaller at a distance.

A few years later, when my brother introduced me to an atlas for the first time, I had no trouble transferring those images of the world from above to the different scales of maps or grasping the abstract reality that they repre-

1. K. W. Butzer, "Coming Full Circle: Learning from the Experience of Emigration and Ethnic Prejudice," in *Light from the Ashes,* edited by P. Suedfeld, 361–98 (Ann Arbor: Univ. of Michigan Press, 2001).

sented. At age eight, I counted that atlas one of my favorite "books," and at nine I could identify every country that my brother pointed to on our globe. In retrospect, I understood at how early an age a child could learn to love the beautiful world in which we live, grasping spatial relationships and differences of scale, and shifting from the reality "outside" to its symbolic abstraction on a colored sheet of paper.

I tried it out when my granddaughter, Maddie, was three, showing her a road atlas of Texas and pointing out Dallas (where she lived), Austin (where her grandparents lived), and Houston (where her cousins lived). I had her trace the roads from one to another with her small index finger, telling her that those red lines were the roads that the car traveled down. A few months later she was back visiting, and I got out the same atlas, pointed out Dallas, and asked her how she had come down to see us. That same pudgy finger traced the red road down to Austin, and then she smiled shyly at me. Next, my five-year-old grandson, Sebastian. He had been playing a "world" board game with his older brother that involved moving around tiny tanks, planes, and ships from country to country. Although he couldn't read yet, he could show me where Texas, Africa, and Asia were, and where his other grandparents lived in Germany. His attitude was, it's no big deal. China? Here. Russia? There. Our current lack of geographical literacy is a problem of parents who barely know about other countries and who fail to stimulate their children's minds. All children should grow up with an atlas and with a parent or sibling who will show them how to use it.

But back to my putative interest in geography as a child. When I was eight, mother bought me an incredible book, Richard Halliburton's *Complete Book of Marvels.* Halliburton was an adventuring pilot in the early days of flying, and he flew to all kinds of faraway places, talking to people, photographing them, and writing about them. He always combined some history with a vivid description of the country, and his *Complete Book* was written for youngsters and printed in big type for beginning readers. It also had a special part, namely a chapter on each of the "seven wonders of the ancient world." I don't know how many times I reread those descriptions of romantic sites in Egypt, Mesopotamia, and Greece, a sort of introduction to ancient history. There were large but slightly grainy photos of pyramids, temples, and monuments, always linked by maps showing how a person could travel from one to the

other. That same book is now displayed on the shelf of my younger son, an architect: he, too, grew up with it.

I was a late starter and still couldn't read near the end of the first grade. At that point, my teacher sat down with me after class every day for two weeks and patiently explained the "system" to me in a low-pressure situation. I finally grasped the code and soon became an avid reader, especially when I discovered that there were more interesting things to read than Jack and Jill primers. Halliburton greatly improved my reading speed and comprehension, in addition to illustrating the logical linkage of geography and history that stayed with me throughout my career.

In seventh grade, we spent the year studying the geography of Canada with the help of a prescribed book of that name. The descriptive text was a bit dull, with too much detail, but the older teacher was very good, and I learned how contour lines show topography. I also had a new atlas that included continental maps in three colors showing industrial zones, agricultural areas, and "nonproductive" land. That latter concept intrigued me, and with a pencil I superimposed the boundaries between those categories on a number of the political maps. It drove the point home that large parts of the continents were "nonproductive," and that the solid colors of a political map were misleading as to how much land there was that was either used or developed. By now, I was drawing maps of imaginary countries that had mountain ranges, hills, lakes, and deserts on them.

At this formative stage of my life, one other book was important. When I was thirteen, my mother picked up a remaindered copy of John Henry Breasted's *Ancient Times* for me. It was a 1913 high school text on the civilizations of the Near East, a quite rigorous study that was not dumbed down like many of today's schoolbooks. I had to push myself to get through it, but it introduced me to the Stone and Bronze Ages of prehistory, which were quite novel for me and made a lasting impression. Breasted also used the size and elaboration of burials or tombs to identify levels of civilization, my first introduction to social evolution. I remember being a bit puzzled, however, that burial goods and tomb chambers should be a measure of civilization.

But not all I learned was from books; my father taught me how to plant a vegetable garden. During World War II, the United States supplied less produce to Canada, and by 1943 people were encouraged to plant Victory Gar-

dens. There was a large tract of unused land behind our back fence, and father began to grow vegetables. It plainly was something he took great pride in, and as I later understood, he had learned as a boy from his widowed mother, who grew flowers to sell at market for a living. Before that, her father and grandfather had been in charge of the flower gardens at the *Schloss* in Benrath since the 1830s. Father used a spade to dig up a ten-inch tilth that was then built up into raised beds, with a six-inch ditch around. On top of the bed, he made three-inch ridges in which to plant the seeds. Beans were planted three together at intervals of four inches. He weeded regularly and, when necessary, dusted for bugs with some deterrent compound. In the fall, when everything was harvested, he would turn the soil over loosely, dismantling the mounds. The results were spectacular, the plant rows coming up thick and strong, rainwater collecting in the ditches but not washing out the seed rows. Our Scottish Canadian neighbor was visibly covetous; he used a hoe instead of a spade, simply working up a few inches of soil, and his rows of beans were thin and had many gaps. Four years later he bought the lot we used and ejected us, presumably thinking there was some hidden charm under our garden, but the results of his efforts in our old plot were no better.

Father had always explained to me why he did things the way he did. Now he told me with a wry smile, "It was time to apply fertilizer." At about ages eight to eleven, I often worked with Father in the garden or went to faraway nurseries with him to find transplant stock for ethnic veggies such as cauliflower and red cabbage. It was our first bonding experience because we didn't engage in organized sports. Years later, when my interests drifted into cultural ecology, I recognized that I had little more to learn about ditched fields or the intensive horticulture of market gardening. My own children didn't have this privilege, but they did know their Opa's rose garden, which was his joy. But my older son, an attorney weekdays, loves to spend a Saturday working in the garden, with the help of his little children, who are also learning about the life that springs from the soil.

Another thing I learned about in the garden was soil texture and how it affects fertility. We had bought an old country house in the Laurentian Mountains in 1945, and my brother and I tried unsuccessfully to garden there. The tomatoes were still small and green when they froze in mid-September. But our farm friends, the St. Pierres, had a model vegetable garden. The difference

was that they piled on manure from their barns. The explanation became intuitively clear to me as a builder of elaborate mud castles: in the Laurentians, I found that the soil was insufficiently cohesive to build a two-foot tower with battlements. The next spring I brought a bag of Montreal soil to mix with the Laurentian soil, and it worked. In my first year of graduate school, I would learn that Montreal Island was part of a late-glacial lake plain (with clay loam texture), whereas the Laurentians had a mantle of stoney glacial drift (a sandy loam), covering infertile granites.

As a child I had learned a variety of practical things about the world through direct experience, but it was still largely an imagined world, as if seen from a window. As an adolescent I became conscious of my love for the outdoors and of a strong interest to learn to know and understand the natural world in which I lived for three months each summer. The transition was gradual because I had frequently worked with the St. Pierre boys, cutting hay, milking cows, and forking manure. In fact, before I went to college, I had wanted to become a dairy farmer when I grew up. Back from the gravel road front and away from Echo Lake, the land still belonged to a half-dozen French Canadian farm families, getting caught up in the shift to service industries or moving to the city. The extensive hardwood forests were interrupted by occasional pastures, but I had found old apple trees smothering in the secondary growth. As I eventually surmised, they were testimony of abandoned farming ventures.

The incentive to begin exploring my environment came from the library. At fourteen, I had become addicted to reading Westerns when I was in Montreal, and I soon focused on the novels of Zane Grey. Apart from the need for better character development and historical context, his novels gave evocative and remarkably "geographical" descriptions of southwestern landscapes. In later years, I would become familiar with the mesquite brush beyond the Nueces River, the pine forests below the Mogollon Rim, and the purple sage of the mesas of southern Utah, and these landscapes turned out to be exactly like I remembered from Zane Grey's descriptions. During the summer of 1948, my self-confidence bolstered by my wire-hair terrier, a .22-caliber rifle, and a Stetson hat, I was out in the "bush" most of the time. An ornithologist had told me about the mysterious Mud Lake, and I eventually found it, locked in between some low hills and a perimeter of dense forest. It was more like a

large swamp, with a wide ring of aquatic plants, while the water surface was almost covered with lily pads and pond scum. But to me the lake seemed a big achievement, and it would be my first nonoutlet lake of many, although the others would be amid open and much drier environments.

I therefore attribute my avocation for field geography and my sensitivity to the nuances of the biophysical environment not to some academic icon, but to roughly fifty westerns by Zane Grey. Indeed, even before I finally found my Mud Lake, I had written a short, landscape poem about a lovely spot that I called "Seven Springs"; I submitted it for an English assignment, and the teacher sent it in to a poetry contest—it took second prize. Not surprisingly, I have never accepted the stereotypical put-down that physical geographers are less humane. It's just that we see the horizon as well as the foreground.

During winter months, before I started shoveling the snow off the front lawn in April, I would read. The greatest library discovery of my teen years was the twenty-volume family encyclopedia of 1894 we had at home, but it was in German and, to make matters worse, in that impossible Gothic typeface, so I couldn't use it. But I cajoled my father to read to me from it. The first article I selected was on Oman, a country I had not been able to find any information on. Each item that followed was the description and history of a place, but with a time-warp of more than fifty years. Without realizing it, I was becoming fascinated with an unusual kind of historical geography, namely the nineteenth-century Near East and Africa. Father's patience fizzled out after a few weeks, so that I had to learn to read the encyclopedia myself, first with his help, then on my own. That task proved to be quite important down the road because I began to master German, perfecting my reading skills over the next few years. Mother now gave me two incredible atlases, dating to just before the First World War, that had belonged to her favorite brother, Karl Hansen, who had fallen at Verdun in 1916. It was a symbolic act because I had been named after him. One atlas was topographic and economic, the other historical, and I used both intensively until I bought new editions in Germany a decade later. It is fair to say that those historical maps, still imprinted in my memory, significantly influenced my later interests and breadth of historical comprehension.

During the summer of 1953, I spent months reading the abridged edition of Arnold Toynbee's six-volume *Study of History.* It was my first exposure to

macrohistory, but presented and interpreted in a normative fashion. Because I was in a hard-science track at college, I was intrigued by the notion of patterns, such as the rise and fall of "civilizations," which I would later revisit in the framework of systems theory.

That fall I had to choose an elective for my last undergraduate year at McGill University. The chairman of the mathematics department suggested I try the two-semester course on human geography. I balked a little: What was that? But within weeks I knew that *geography* was what I had been devoting most of my spare time to for the previous twelve years. Moreover, it was an academic discipline with distinguished origins. That course proved a joy, as taught by two young and enthusiastic geographers. Theo Hills was a New Zealander, working on a dissertation in South America, and his tropical experience was just what I was looking for. My term paper was on development in the African savannas, an environment I so much wanted to see for myself. The other instructor, Hugh Thompson, had just obtained his doctorate at Oxford on subarctic glaciers. Although I didn't much care for the Arctic, he impressed the systematics of physical geography on me in a way that I could really appreciate.

After two months I told my parents that I wanted to go on to graduate school in geography. They were appalled, as if I had suggested I wanted to become a musician. How could I earn a living that way? Mother never quite believed that geography was a respectable field, but in the end both my parents were persuaded. If I completed my honors degree in mathematics, I could switch to McGill's master's program in meteorology and geography during the following year. I lived up to the bargain and was accepted—thrilled at the prospects as much as I was relieved to leave mathematics behind. But I could not so easily discard the logical structures and the abstract thinking inherent to non-Euclidean geometry, which had appealed to me.

The Best of All Possible Worlds

Yet another fortuitous event took me to Europe with no advance notice. Shortly after my last exams in the spring of 1954, we were visited by a doctor from a German ship that had just docked in Montreal. There was some brainstorming at home, and forty-eight hours later I was on a small freighter bound

for Germany, wondering how I had ever gotten myself into this situation. Any doubts I had quickly evaporated when I climbed up on deck next morning. We were sailing along the coast of the Gaspé Peninsula, a string of lonely villages in view, on an almost treeless landscape. Church bells calling to Sunday mass were the only signs of life from our distance. I was exhilarated.

The next two days we inched through a fog bank to the mournful tunes of our foghorn and then rode out a three-day Atlantic gale. I stumbled around on the decks, photographing waves breaking across the bow, enjoying every bit of it. The officers and crew of the *Franziska Hendrik Visser* were all German Frisians and very laid back. Because there were only two other passengers, they allowed me every reasonable freedom and let me spend hours in the control room. I was shown how the meteorological equipment worked and was logged, and how diurnal positions were measured and drawn in on the navigation charts. By the end of the trip, I could estimate Beaufort wind speeds from wave development to the satisfaction of the second officer, who became a good friend. I also copied the weather log for our voyage and later plotted those data on graphs. The experience proved to be a most useful workshop on weather recording as well as an opportunity to improve my conversational German before landing in Emden.

Traveling by train south to Düsseldorf in a prewar carriage with wooden benches gave me a small cross-section of changing landscapes outside and of passengers inside. At first there was picture-postcard scenery looking stereotypically Dutch, with quiet but friendly people. By the time we approached the Ruhr, the train had filled up with a more varied crowd that dressed, talked, and interacted differently. In a distance of three hundred kilometers, equivalent to that from Houston to Dallas, there was more cultural difference than between Texas and Minnesota. This was Europe.

I was puzzled by unfamiliar items of vocabulary. My parents used *billet* for a train or bus ticket, one of many French loan words current in 1930. A ticket was now called a *fahrausweis* (a neologism equivalent to "travel identification card"), however. Here was top-down language change in the style of the French Academy, but, instead of Americanisms, the Nazis had eliminated French loan words. My father always spoke standard German, but his family in Benrath (on the southern edge of Düsseldorf), where I stayed for three weeks, used only Rhenish dialect, which is close to Plattdeutsch or Low German. It's

a slow, rhythmic, and comfortable way of speaking, analogous to the contrast between southern and standard American English. But German dialects don't just differ in pitch and pronunciation; they also vary somewhat in grammar and vocabulary. At the scale of major dialect groups, Rhenish and Bavarian are as different as Dutch and standard German, or Russian and Serbo-Croatian. In picking up a working knowledge of Rhenish, I was beginning to understand the use of language and its nuances in social negotiation.

That summer I also spent time getting to know my mother's family in Aachen, some of whose suburban communities lay beyond the border in Belgium or Holland. My cousins crossed the borders regularly on shopping trips, and I began to grasp the rituals of ethnic behavior that come with such regular interaction. In fact, Aachen, a very old imperial city and the residence of Charlemagne's court in A.D. 800, was very much a border town with a transnational outlook. Even today, busloads of Dutch and Belgian visitors daily swarm inside Charlemagne's cathedral, which they see as part of a shared heritage. The national boundaries of much of Europe are more a matter of historical accident than they are delimitations of difference, particularly where they are drawn in the countryside. But each city has an intangible but distinctive ambiance, grounded primarily in the temporal and economic context within which it had once commanded wealth. That distinctiveness became intelligible to me only little by little, over the years, but I remember how my curiosity was first piqued during that incredible summer in Europe.

Next on the agenda was a trip to Spain. The most unforgettable moment was waking up in the first light of dawn as the train rumbled across the *meseta* of Old Castile. Wheat fields stretched flatly from horizon to horizon, glowing pink in the rising sun. Then I saw the silhouette of the great wall of Avila on the horizon. My brother, a cousin, and I got out there, spent hours walking around that huge wall, and then went inside, through the maze of narrow streets with their small plazas and medieval church facades. All the stone was colored buff and set off against a crystalline sky with a stark, understated beauty. It was my first image of an arid environment, one that would capture my lifelong professional interest. On the rickety local train from Avila to Madrid, we sat in third class with a crowd of chatting, sociable Spaniards, being made to feel right at home even though we didn't speak a word of the language. The warmth of those simple country people activated the powerful,

historical landscape that drew me back to Spain time and again across some forty years.

Italy, which I now visited with a tour group, was somehow less dramatic. But it was here that I first saw Mediterranean orchards and began to take an interest in church architecture, conscious of my total ignorance in the subject. The Germans in my group were a delightfully wacky bunch, with a good sense of camaraderie—my first taste of living and working with research teams much farther afield and under more trying conditions, both complicated by a higher ratio of thorny personalities. Field-camp behavior quickly diverges from that at a university, sometimes for the better, often for the worse.

These formative impressions of that halcyon summer in Europe were rounded out by a "vacation" with the family of one of my uncles in the Alps. I insert qualifying quotation marks because my uncle was either hiking or negotiating mountain passes in a minibus every single day. I also learned a great deal about haymaking and the pungent application of liquid manure. It was an incredible introduction to southern Germany, Austria, and high mountain landscapes. When I returned to Canada to begin the graduate program at McGill, I had sampled much of the agenda of geography with open eyes, but still in the dark as to how one moved from travel experience to professional observation and understanding.

My master's advisor at McGill was Ken Hare, the departmental chair. He was British and had worked with the Meteorological Office during the war. A fine process climatologist, he had a broad interest in climatic change and in the interplay between climate, vegetation, and soils in boreal and subarctic Quebec. He inspired by emphasizing the importance of the question and the quest, and avoided simple answers. As a supervisor, he stimulated and facilitated rather than dictated. For my proclivities and needs, he proved the ideal choice, and his relaxed style impressed me. I owe my interest in environmental change to him above all others.

The course work was preceded by a week-long field trip to the glaciated landscape of southern Ontario, led by Brian Bird, an arctic geomorphologist. The trip was fast-paced and rigorous, an almost overwhelming eye-opener for a novice. Also aboard was a young Swiss glaciologist, Fritz Müller, whose company I enjoyed. Twenty-five years later he invited me to join him at his institute in Zurich.

After I tried out some sterile courses in botany, my revised curriculum for the year included three key pieces. The first was Hare's inspirational seminar on arctic ecology and Quaternary paleoclimatology. One of my two papers examined the work of Carl Troll and his students on ecozonation in the Andes, including the issue of ecological convergence of unrelated biota above the treeline in various tropical mountain environments. The second piece was a brilliant lecture course on modern European history by an English historian, Noel Fieldhouse, for which I did a paper on the ethnicities of the Austro-Hungarian Empire. The third piece was a self-directed reading course with Hare, which began with Ellsworth Huntington's books on climatic change and "climate and civilization," the latter a disappointment compared with Toynbee's sophisticated, nondeterministic approach. From there, I sampled the putative literature about climate and disease on human physiology before finding the trajectory for my master's thesis. I decided to explore the literature on historical climatic variation in the Near East in relation to nomadic migrations. Visiting people in other departments, I encountered my first polemic ideologue, an anti-Marxist anthropologist who assumed I was a Marxist because I had read Gordon Childe. My meetings with the orientalist Bible scholar on campus were more congenial, and he became my second reader.

I would be completing the thesis during the summer, so I was looking into options for the Ph.D. The McGill department had an obligatory Friday afternoon colloquium that almost without exception included speakers on arctic themes such as calving glaciers and permafrost coring. After six weeks of such a cold diet, it was clear to me that I had no interest in an arctic dissertation. An assistantship at McGill would have required me to spend months at the permafrost laboratory atop some self-heaving sphagnum moss in Ungava. Faced with that prospect, I instead wrote to Carl Sauer at Berkeley and explained what I'd like to do; he replied promptly and encouraged me to apply for a teaching assistantship. But then a Sri Lankan classmate told me about German government scholarships, which required an invitation from a German university. I wrote to Carl Troll, an obvious choice, and my letter elicited an enthusiastic, two-page, hand-written response. After my application for a Canadian government fellowship was denied—almost by return mail—I interviewed with the German consulate. Two weeks later the Germans offered me an exchange scholarship. The choice was clear: Troll, Bonn, Germany.

In September 1955, after a somewhat breathless year, I was aboard ship for Le Havre, France. On the train ride from Paris through Belgium, I became engaged in a conversation with a young passenger who I thought was Belgian because he spoke fluent French, without an accent. After half an hour of exercising my Canadian French, it turned out that he was a German and that we could actually converse with less effort. That would be a preview of the vibrantly international flavor I found at the University of Bonn. The Geographical Institute was housed in a renovated eighteenth-century palace, and the three spacious rooms for doctoral students had eighteen desks; of these at least five were assigned to foreigners like myself—a Spaniard, a Japanese, an Iranian woman, the Sri Lankan from McGill, who had also received an exchange fellowship, and a scholarly Englishman who spoke in such a strong regional accent that we had to communicate in German. At the university cafeteria, several dozen Norwegian students always sat in a closed group, while a hundred or more Iranians fluttered around any unattached women. At get-togethers organized by the Academic Exchange Service, one could also meet a variety of Commonwealth students.

The doctoral students, some of whom had served in the military, were enthusiasts for a united Europe without borders. The foreign students were all given warm welcomes and genuine support. Up to two hundred students from all over the world faithfully attended several long slide shows on Japanese rural landscapes by our Japanese friend. Six to nine hundred citizens of Bonn could be expected to attend any open lecture on a foreign country. African Americans, who were unable to sing in U.S. opera houses, starred in the town opera, and were enthusiastically applauded. Whatever unreconstructed thoughts lurked somewhere among an older generation, those were heady times not only to be in Germany but anywhere in continental Europe. The era of obsessive consumerism and guest workers had not yet dawned, and it seemed as if everyone had just come up for pure oxygen after the years of repression, war, and occupation. All this stood in contrast with the grim parochialism of Canadian society at the time.

My first course with Troll was on regional Germany, a subject in which I had little interest in principle, but I was impressed by the way he integrated physical and human geography. Most days, before class, he drew a detailed chalk map of the geomorphology of the area he would discuss. Later he would

talk about the settlement history and craft or industrial change in that context. The full power of his seamless integration of physical and cultural geography became apparent on the related field trips. The second course covered regional South America, but Troll never got beyond the Andes, and it became a systematic exposure to tropical mountain ecology, a subfield that he had pioneered and dominated for decades. From the autobiographical bits he occasionally threw in, I began to appreciate his grasp of Andean environments, a knowledge based on many years of working up that mountain chain from Chile to Colombia, mainly on mule back and sometimes on foot, climbing over Pleistocene moraines or contemporary glaciers.

Troll always carried thermographs in the Andes to record various temperature changes at the ground and at the regulation level of 1.5 meters. Against that microclimatic data, he explored changing associations of vegetation and small-scale geomorphic processes, such as needle-ice displacement of soil particles. Over the course of his career, he used this approach from Spitsbergen to Antarctica, emphasizing the importance of diurnal versus seasonal temperature regimes as sensitive diagnostics of both zonal and vertical environmental types. I followed up this classroom exposure by carefully studying his many papers on periglacial processes and geomorphology, a subject that did not enter English-language texts until a generation later, after several of Troll's major reviews had been translated into English by U.S. government agencies. Eventually I was able to develop a corresponding field familiarity that was quite helpful when "born again" periglacial enthusiasts were wont to make spurious claims for Pleistocene periglacial activity in unlikely subtropical mountain settings, but in the 1950s I declined to take up this topic for the Ph.D.

The reason that Troll did his fieldwork in South America was that it was the only foreign venue where German scientists were welcome to work in 1925—German geographers were not even allowed to attend International Geographic Union meetings at the time. Troll always remained grateful for this opportunity, and he respected and was genuinely fond of South Americans. I remember when the Paraguayan embassy invited him over to award him with some medal of distinction. He was quite excited by the prospect, taking the honor at face value and enjoying the opportunity to expand his network of cordial relationships, even though he already had a drawerful of Latin Ameri-

can medals. His attitude contrasted with the cynicism of former intelligence service geographers who became engaged in the U.S. study of Latin America during the 1940s. Troll's field-oriented students learned from him that one doesn't patronize people of good will from less-developed countries. By extension, he also didn't question the quality of my Canadian training, however green I actually judged myself to be. He supported and encouraged me to concentrate on my dissertation without any preliminary hurdles.

During the semester break, I went to Egypt in the winter of 1956, traveling with a German student group as a matter or organizational convenience and minimal expense. After docking in Alexandria with a Turkish steamer, we ate Arab cuisine the first night. The next morning I got out of the train at El Alamein and set off on foot for the coast, walking through the not-yet deactivated mine fields with a roll of toilet paper in hand, following the narrow trails of local goatherds. Later on, in 1960, I wrote a paper on the area that continues to be cited because I first recognized a major fault zone interrupting the Pleistocene beach ridges at the western edge of the Nile Delta. While the other students were visiting mosques in Cairo, I was walking up and down dry desert valleys or wadis, observing arid-zone geomorphology that looked rather different than that described in the textbooks I had read.

I also had an opportunity to fly to Jerusalem via Jordan, and the Egyptian pilot let me sit in the cockpit to observe the panorama of desert landscapes over the Sinai and Dead Sea, which I was able to photograph from the air through the open window of the DC-3. In the Jordan Valley, traveling in an old taxi, I visited the high lake beds of the Dead Sea described in the geological literature. By now I was hooked on arid lands. Still later, after a train wreck, we, the student group, reached Luxor in Upper Egypt. There I walked all the way up the Valley of the Kings, examining alluvial sediments for the first time and finding Paleolithic artifacts in place within them. Afterward I climbed across the mountains above the tomb shafts, stumbling on the mass of talus debris and learning to appreciate the role of mechanical weathering in such an environment. I paid the price for my explorations with a slight case of sunstroke, but after a day in bed I was out again. The beauty of the total desert in Egypt, with its luminous limestone scenery and its deep blue sky, fascinated me. It was a strong stimulus to move on with the dissertation.

A year later, a paper that I had developed from my master's thesis but that

was informed by the Egyptian exploration was accepted for publication—my first publication—in the same week that I defended my dissertation. I felt I still had a great deal to learn, however, and decided to stay on at the University of Bonn for a while, taking courses that I knew I needed, especially in geology, and auditing others in history that I was interested in for less-tangible reasons. In 1958, Troll arranged a meeting for me with an Egyptologist who wanted to carry out a survey in the Nile Valley for late prehistoric sites. Just as Troll had found support for me as a research associate of the German Academy of Sciences and Literature, he now now steered me to a grant for fieldwork.

The idea in Egypt was that prehistoric sites had been reported from the desert edge of the floodplain in some places but not in others. The Egyptologist and I studied the sites that were known in order to understand in what geoarchaeological contexts they were found and preserved. With that in mind, we walked some five hundred kilometers up and down the desert edges of Middle Egypt, looking for new sites or developing an explanatory rationale as to why they were absent, which was usually the case. My associate was high strung and cantankerous, but he was genuinely interested, which helped in a strenuous undertaking, with poor local food and dirty hotels, sometimes without a lock on the door. Midway through the project, I moved out for two nights to an American mission in Asyut for a chance to take a bath and catch the fleas that insisted on traveling with me. There I heard the wavering strains of Puccini on short-wave radio, and my eyes grew moist. I can therefore relate to the soldiers listening to the military radio station in Belgrade signing off with "Lili Marlene" during World War II. Something you otherwise take for granted can become your last sane link to reality.

We came within a hair of being lynched at an isolated town called Dalga, west of Mallawi. After we were run down by an armed horseman, a growing hostile crowd dragged us to the cemetery, stood us up against a wall and picked up stones to throw at us. They desisted when we called over and over again for the *omdeh,* usually a respected civil and religious leader in rural towns. Instead they brought us to a small military post where the soldiers couldn't read our Arabic identification papers, but insisted on beating us up a bit before taking us in to the commandant. He could read and was upset, if only because several hundred people were shouting for our blood outside. We

eventually left under military escort. His explanation was that the townspeople thought we were British spies, a reasonable notion given that the 1957 war was still fresh in people's minds—British fighter-bombers had destroyed the entire Egyptian air force on the ground.

A year later, when I told a British geologist at Oxford about this adventure, I received a very different reading. In 1919, a British troop train with wounded soldiers had been boarded by rebelling Egyptians, fifteen kilometers away from Dalga, resulting in somewhat of a massacre. Dalga had been the scene of one of several British reprisals, and a decade later the Oxford geologist had been attacked here and was forced to "use" his shotgun. Middle Egypt has been a rough place. Some rival villages were still settling blood feuds with AK-47s in the 1970s.

By comparison, Spain was just this side of heaven. I had been there on various long excursions during an international congress in 1957. In 1959, I returned, this time on my honeymoon. The upbeat schoolteacher from Bonn whom I married knew me well enough to expect that it would become a working vacation. Elisabeth and I both loved Majorca and its deeply incised marine inlets, and I started a project on fossil beaches, dunes, and paleosols that continued until 1962. My bride wrote down the numbers derived from gravel-rounding measurements and giggled when I tried to use olive oil to avoid a sunburn. We also established what became lifelong friendships with two Majorcan scientists, one of whom later invited my wife and me to start our Spanish village study in 1979.

In 1959, Troll showed me a letter from the University of Wisconsin asking him if he knew a good physical geographer to recommend for a new position in Madison. Was I interested? Of course I was. Without an interview, I was hired, presumably because I had already published ten papers and my dissertation, "Quaternary Stratigraphy and Climate in the Near East." Little did I suspect that my years at the University of Wisconsin would be the most tumultuous of my career.

"On Wisconsin," or Whither Wisconsin?

At McGill, critical discussion had been something that graduate students could engage in even if they argued with a faculty member, but at a German

conference in 1958 I found out that such argument wouldn't go over as well when I contested a claim by a senior geomorphologist with a counterexample from Canada. He promptly wrote a letter complaining of my behavior to Troll, who was genuinely embarrassed because it wasn't the first such letter he had received. A year earlier I had made some offhand comment at supper to some German professors on one of the Spanish excursions about the unattractive lodgings we were in. That comment prompted a letter to Troll noting that if the accommodations were good enough for German professors, they should be good enough for me. It had become apparent that I didn't want to stay at a German university permanently because I was temperamentally disposed to call a spade a spade. I also was under the illusion that Wisconsin would be much like McGill, only bigger. Wisconsin, as a state, and Madison, as a city, were friendly and indeed welcoming to newcomers, but the Department of Geography at the time was smug and authoritarian.

One problem was that I was only twenty-five, younger than most of the graduate students, and the next youngest faculty member, Fred Simoons, was thirty-seven. Most of the senior faculty had worked for the intelligence services during the war and had known each other well for at least fifteen years. They formed a good old boys' club, with a strong military flavor and redolent of rank. The chairman, Andrew Clark, was a Canadian. At the institute in Bonn, there had been an explicit hierarchy that comfortably structured formal relationships, but did not inhibit periodic fraternization at Carnival parties in the institute or all-night story-telling at Troll's home, with the participation of a number of his nine grown children. His authority was based more on his charisma and on the affection of students and staff than on the institution. Not once did I see him act overbearing or wield his authority to patronize a subordinate; his critical mode was paternal and constructive, never ad hominem. The Madison department worked the other way around, with a fictive egalitarianism and all decisions made in private by only three to five power brokers, despite obligatory weekly faculty meetings that ran all of each Tuesday afternoon. Subordinate full professors either remained inconspicuous or were sarcastically put down. The graduate students were nervous and uptight, and displayed no spontaneous adulation for faculty.

The worst human traits appeared each winter when everybody had to "vote" on everybody else's salary increase. For weeks before the vote, faculty

would go around lobbying from office to office, bemoaning how hard up they were. Salary-increase increments were $100 for assistant professors, $200 for associate professors, and $300 for full professors, and one could recommend anywhere from zero to three increments. The year I arrived I had a salary of $6,250, whereas the four senior professors were paid three or more times that amount. The inequities were self-perpetuating because, throughout my seven years at Wisconsin, Andrew Clark and Arthur Robinson constituted the salary committee and thus set their own salary increases, which moved in tandem by three increments each year. Just how fictitious the process was became clear the year a new instructor turned in his recommendations to Clark and was forced to explain and argue each case, with encouragement to criticize his colleagues. When the ordeal was over, we were told that the salary committee didn't count the votes of untenured faculty anyway, but it did want to know how that faculty voted. That process strengthened the position of the ruling oligarchy by making advancement conditional on "loyalty" and by dividing the lower-status faculty, with the unfortunate result that it took months before the department regained some sort of equilibrium.

By refusing to let myself be bullied, I had opted for a painfully slow advancement. My first confrontation with Andrew Clark came within a month of arrival. He had written a letter to all faculty for ideas or recommendations for a possible new position in Soviet geography. After again being asked directly, I wrote a note mentioning a Czech colleague I knew from McGill and who had a doctorate from Prague. Clark thereupon sent my Czech friend a condescending letter, questioning his qualifications and even the validity of his Ph.D., but concluding that he could apply if he wanted. I protested this cruel and unnecessary letter because if we didn't think him good enough, we didn't need to write him to tell him so. Clark exploded. My voice was just as loud when I responded.

Three months later I received an invitation to give some talks at Minnesota, where several geography faculty pressed me relentlessly to say something negative about Wisconsin. Inexperienced as I was at such games, I eventually did make some cautious but limited negative comments. The telephone was faster than my return flight, and I was accused of "running down the department."

At Bonn, there was a free market for ideas and approaches to geography, and I never sensed a prescribed way of thinking about anything. Not so at

Wisconsin, where I was told, reminded, and eventually warned that my class presentation of physical geography was to be descriptive rather than processual. I had to sit in on Clark's and Simoons's presentations of the elementary physical (bread-and-butter) course, as well as Glenn Trewartha's climatology courses, so as to see how they were done. I also *had* to use the departmental textbooks. I wasn't necessarily being difficult about these points, and I was still experimenting with teaching methods and had no fixed ideas, but the departmental ideology was gratuitously expounded to me as if I were some sort of subversive force. I was first allowed to teach an advanced climatology course in 1962, but I didn't use Trewartha's books. It was 1965 before I could teach an advanced course in "landforms" (*geomorphology* being a taboo word), after Edwin Hammond left the department.

It bears noting that all the established Wisconsin faculty either explicitly or implicitly adhered to the maxims of Richard Hartshorne's *Perspectives on the Nature of Geography* (1959), even though Hartshorne himself never questioned whether what I did was "geography."[2] That spirit of a bounded geography, retreating behind disciplinary fences to focus on global categorization and regional description, had deep roots at the state universities of the midcontinent, but it was disseminated and championed in the postwar years mainly by the geographers who had once worked in the Office of Strategic Services (OSS, the forerunner of the CIA).

Although I was told to my face that I could either teach well or do good research, but not both, my response to the challenge was to try to do both. I wrote four new papers, which were accepted for publication in the first nine months I was at Wisconsin, but I received no salary increase, although the dean gave me the semester research leave originally promised. I spent that time working in Majorca and later doing the backup soil-and-sediment laboratory work in Bonn under the initial guidance of one of my old graduate classmates. At about this time, I learned that my doctoral defense three years earlier had become enshrined in legend. It was the first time that a Ph.D. candidate had sat down on top of the desk while answering questions in Rhenish

2. K. W. Butzer, "Hartshorne, Hettner, and the Nature of Geography," in *Reflections on Richard Hartshorne's* The Nature of Geography, edited by J. N. Entrikin and S. D. Brunn, 35–52 (Washington, D.C.: Association of American Geographers, Occasional Paper 1, 1989).

with an American accent. My strategies for scientific networking were more mundane, but were beginning to pay off. In the 1950s, there were no photocopying machines, but reprints were still affordable, and so I had started off writing for reprints of others' work even before I had the degree. When my first papers came out, I reciprocated by sending out reprints of my own. By the time my dissertation was published, I sent out notices of its publication to the 110 people on my reprint exchange list. The edition of eight hundred copies was sold out in two years and was then reprinted in New York.

Shortly after arriving in Madison, I was contacted by the Chicago archaeologist Robert Braidwood, whom I had already met at a conference in Hamburg. He invited me to submit a paper on my geoarchaeological work on prehistoric sites in Egypt at the American Association for the Advancement of Science (AAAS) meeting in Chicago that December. A reception was held for me in Chicago, where I met F. Clark Howell, a rising star in paleoanthropology. We had many common interests, and he invited me to a prestigious Wenner-Gren Symposium at Burg Wartenstein, Austria, in July 1960. The intense ten-day workshop was devoted to stratigraphy and environmental change in the Mediterranean Basin as a background to the archaeological record. More than half of the twenty-four participants were French scientists, again expanding my network of acquaintances in this interdisciplinary arena. In May 1961, Braidwood invited me to an informal symposium in Indiana, where just about everyone working on agricultural origins in the Near East was present—from palynologists and zoologists to archaeologists. Two months later Howell co-organized another Wartenstein symposium, this time on hominid evolution, ecology, and Africa, and I was aboard again. It was an unrivaled learning experience, from watching films on baboon behavior to seeing Louis Leakey and Desmond Clark explain the making and use of stone tools to hearing well-known geologists describe ancient lake beds and cave fillings. The people I met at the AAAS and the two Wartenstein affairs were excited by their research, eager to learn more about anything that might be pertinent, and most were pleasant and personable. They indeed were world-class professionals, driven by their quest for knowledge, irrespective of disciplinary boundaries or training. By comparison, the Wisconsin geographers seemed more and more like burned-out teachers at a second-category British public school.

Andrew Clark did inadvertently do me one good favor. After I arrived in

Madison, he informed me that I would never be able to teach geomorphology but that I could have his course "Introduction to Historical Geography," which I could teach as a sort of prehistoric geography. Beginning in 1960, I offered it each spring, and it became the crucial link between my teaching and my research. Its topics ranged from methods of paleoenvironmental research to reconstruction of Pleistocene environments, and from human-environmental interrelationships to the beginnings of agriculture and floodplain settlement in the Near East. As my understanding and sophistication grew, the mimeographed lecture notes that I circulated in my classes were gradually transformed into a manuscript on which I sometimes worked until three o'clock in the morning. The resulting book appeared as *Environment and Archeology: An Introduction to Pleistocene Geography* in 1964.

According to Fred Simoons, the department met a half-dozen times during 1961–62 to debate whether or not to renew my appointment for another three years. In the end, they promoted me to associate professor instead, which completely surprised me because I expected to be terminated. In retrospect, now that I understand better how mobile the job market had become by 1962, my surmise is that they decided they couldn't afford to lose me.

My outside professional work continued to evolve and intensify. In 1961, I first joined Clark Howell to decipher the burial circumstances of extinct elephants at Torralba and Ambrona in central Spain. They had been killed and dismembered approximately four hundred thousand years earlier by Paleolithic hunters at the foot of a periglacial slope at the one site and in a valley-margin swamp at the other. It was a most productive intellectual exchange from one day to the next, and the work continued during the following two summers, then later resumed in 1980–81. In 1962–63, I was on leave as co-principal investigator on a National Science Foundation grant, spending seven months in Egyptian Nubia, where I and my Ph.D. student Carl Hansen mapped the Quaternary record and examined the geoarchaeological contexts of prehistoric settlement.

When *Environment and Archeology* appeared, Andrew Clark began to lobby the Wisconsin faculty that it was an eccentric book, more an embarrassment than an asset to the department. In April 1965, I put copies of a 1,300-word lead review from *Science* into everyone's mailbox, thus ending this speculation but not the machinations. In the fall of 1965, I visited Louisiana

State University, where I was made an offer as full professor. During the interview, the former director of the Coastal Studies Institute, Richard Russell, told me that he had already received an unsolicited letter about me from the University of Wisconsin. That letter apparently described me as a troublemaker, among other things, but he had given it no credence.

The chair at Louisiana State, Jesse Walker, agreed to give me three weeks to digest and consider the offer, but after only a week he was on the phone, pressing for a decision. Because I don't like to be pushed in such fundamental decisions, I declined the offer with regrets after the third call. The dean at Wisconsin promptly authorized my promotion to full professor, with a substantial increase in salary. Unfortunately, it wasn't quite that simple. At a February 1966 faculty meeting, the figurehead chairman informed us that there would be little of a salary increase the next year because most of the money would go to *my* salary. Some of my colleagues looked down at the table with discomfort, others stared or glared at me. I didn't say a word, although his statement was a lie; the money for counteroffers came from the dean's discretionary funds, not the department's existing allocations. After the meeting, I walked down the hall to my telephone and called Clark Howell in Chicago. He had already hinted to me that Chicago was interested and now promised to get the gears in motion.

In March, I interviewed at the Department of Anthropology at Chicago, where I talked at a formal dinner attended by the whole department. Several sociocultural anthropologists expressed their delight that I was joining them. By contrast, when I visited the geography chair the next day, Wesley Calef smiled at me and said, "You know, Karl, we aren't really interested in you in this department, but if the anthropologists want you, that's certainly all right with us." I smiled, too. I really did understand.

The terms were generous: teaching only two quarters per year with a total of three courses; a huge, combined laboratory and office in the paleoanthropology wing; moving expenses; and a 50 percent salary increase over Wisconsin. I accepted without hesitation. At Wisconsin, I was removed from all my committees and "shunned" by the senior faculty. My only regret was my initial perception that all the effort I had put into that department had been a total loss. But of course it wasn't because the students and not the institution are what matters. I had four Ph.D. students, two finishing after I left, and one

joining me in Chicago. There also were other students who had taken or audited my prehistoric geography course and who later told me that I had made a difference: Alfred Siemens made his reputation as an innovative geoarchaeologist; Cole Harris moved on to make a second career in ethnohistory; and Stanley Brunn, as editor of the *Annals of the Association of American Geographers,* chose me as editor of the special *1492* issue. Wisconsin colleagues such as Fred Simoons, Bill Denevan, and Jonathan Sauer remained friends for life, and the map librarian, Mary Galneder, continued to help me find items years after my departure.

The underlying ironies of the Wisconsin experience crystallized for me some years later at a meeting in Milwaukee when I overheard an Andrew Clark student sarcastically say to one of his peers, "There goes the world's only Pleistocene geographer." It was Wisconsin that was out of step, bemired in a conservative, postwar paradigm of geography, unable to grasp the ongoing theoretical revolution and to open up to more versatile social sciences beyond the fence. Yet the "national departmental ratings" system continued to reward the strongholds of orthodoxy and stasis with billings among the top three or four. That lagging ability to move from derivative teaching to engagement in an open, intellectual discourse would eventually cost the discipline of geography dearly.

Chicago, Africa, and Beyond

My first years in Chicago in the late 1960s were an exhilarating experience as I interacted with the incoming graduate classes of anthropologists (in the team-taught "Human Career" seminar) and geographers (in my physical geography course), some thirty in each group. The anthropology faculty were interactive, and there were regular faculty-student lunches at a local pub, at which good discussions were common. I sat in on some of Clifford Geertz's seminars in cultural ecology, held in his home. In the spring, I would take excursions up to Wisconsin, visiting many of the physical features to which I had taken my Wisconsin classes. In the meantime, Clark Howell was preparing a multiyear expedition to the Omo River of southwestern Ethiopia to search for early hominids, and I would be a participant.

East Africa turned out to be everything I thought it would be and more, and my teen experiences in Canada proved to be the right preparation for bush bashing, whether in the riverine forest or the adjacent tall-grass savannas. The tribal peoples along the Ethiopia, Kenya, and Sudan border were fascinating. I made my first, unexpected encounter on a woodland path not far from the Chicago camp, running into a six-foot man wearing little or nothing, with an equally tall spear in his hand, walking with his mate, who was decorated with a bark skirt. When I stopped abruptly and my jaw dropped, he grinned. We both started laughing and shook hands. Africans, I soon discovered, have an incredible sense of humor. Eventually I learned to work comfortably at sedimentary exposures with a half-dozen armed warriors watching me with great solemnity.

Camp life was pleasantly convivial, and I spent some time with Richard Leakey in the Kenya camp on the other side of the river. Richard had found an anatomically modern cranium, eventually dated at 130,000 years, by far the oldest such specimen known. I mapped and interpreted the sediments that secured the context for this find and a more primitive fossil (Kibish I and II) that set in train the "out of Africa" hypothesis for modern humans. The hypothesis would be supported by biomolecular evidence that contemporary people are all descended from an African "Eve," very roughly 150,000 years ago.

In 1968, I was back again at mapping, by helicopter, the surficial sediments of an area the size of Rhode Island. This project included a study of the meander belt and distributaries of the Omo Delta, as analog data for the Pliocene fossil beds being concurrently explored by others. Lake Rudolf (Turkana) had been sixty to eighty meters higher 10,000 to 3,000 years ago, at times overflowing into the Nile drainage through the swampy flats of southeastern Sudan. In fact, the Omo River in flood and indigenous cultivation next to its banks provided a useful model for the primeval Nile in Egypt because it derived its waters from a watershed abutting that of the Sobat and Blue Nile. Upriver reconnaissance by light aircraft was equally exciting, and we regularly flew in and out of the study area via the Rift Valley from Nairobi.

Even after I began to work in southern Africa in 1969, I continued to extend my East African experience. In the Serengeti, I studied savanna landform evolution, and in the old center of Aksumite civilization I was able to show

that soil erosion had seriously impaired the productivity of this sector of upland Ethiopia. Despite a helicopter crash and shooting scrapes, my experiences in East Africa remain among the most memorable of my life.

Then I began research in South Africa that would eventually cover nine seasons, spanning thirteen years. My interest in the area was based on its potential parallels with the arid and semiarid environments of the Near East and Mediterranean Basin, and comparatively few researchers were qualified for such work. I studied ancient cave breccias in the Transvaal, younger cave sediments and beach accumulations on the spectacular south coast, old playa lake beds of the High Veld planation surface, waterfall tufas on ancient dolomite escarpments, and alluvial fills in the Vaal River Valley and in several of its tributaries. This work was focused on some three dozen key archaeological sites and on a variety of rock-engraving clusters, and was complemented by a season of archaeological surveying and excavation.

Here, even more than in East Africa, I worked in a wide range of topographic and environmental settings, with sites that ranged from fossil beds to stone artifact horizons, rock art, and architectural features. Observational skills are best honed not by endless repetition under similar parameters, but by comparison and contrast. Equally so, different kinds of sites record change with respect to different thresholds to provide a more complex view of the nuances and amplitudes of environmental change, while helping to identify different patterns of climatic deviation in specific parts of a region. In South Africa, I was also able to show that early modern people were present before 100,000 years ago.

Given the regional data base that I had established, it was apparent that later Pleistocene groups left extensive areas of marginally productive environments unoccupied for tens of millennia at a time.[3] I interpreted this phenomenon as a risk-avoidance strategy in drought-prone and low-predictability environments because highly dispersed bands of foragers would intersect with other groups at intervals too long to provide current information on animal migrations or where plant foods were available. In isolation, a lack of information is as hazardous as an inadequate gene pool. Such a "marginality model"

3. K. W. Butzer, "A 'Marginality' Model to Explain Major Spatial and Temporal Gaps in the Old and New World Pleistocene Settlement Records," *Geoarchaeology* 3 (1988): 193–203.

implies long periods with discontinuous occupation of the African continent, which would accentuate human evolution through alternations of gene flow and genetic drift,[4] which in turn could lead to genetic "modernization" without invoking directed migration.

I had always been interested in the interface between the environmental and social sciences, beginning with Toynbee's notion of challenge and response as a prime mover. To avoid the trap of determinism I knew that I would need a great deal of sophistication in the social sciences, which I acquired during years of interaction with anthropologists of all kinds who commonly were on the cutting edge of social theory. I had discovered early on that environmental history, whether short or long term, had to be generated inductively and deductively through primary research. By the 1970s, I felt comfortable enough to begin examining those relationships, starting with seminar courses on settlement geography and archaeology. I wrote a small book on irrigation ecology in ancient Egypt, based heavily on historical sources, and continued to pursue the subject of Nile failures and political devolution in successive approximations. My lectures at this time explored unusual combinations of positivistic and humanistic themes, borrowing heavily now from modern geography.

My departmental homes at the University of Chicago were in decline by 1970, when the anthropology department lost Clark Howell and a chain of other brilliant men, replacing them with vulgar poststructuralists. Geography lost three of eight full professors in 1968–69, without even attempting to replace them. After 1971, endowments and scholarships dried up, as did the influx of top-caliber students. But I had learned at Wisconsin not to try to change the academic establishment. Eventually I sought most of my stimulus and feedback off-campus or generated it in experimental courses. In 1981, I took a cautionary leave and accepted the chair of human geography at the Swiss Federal Institute of Technology in Zurich, Switzerland. That caution proved wise because within days of my arrival I tangled with the university president there in regard to his unexpected plans for the Geographical Institute. I stayed for only two semesters but developed a course sequence that em-

4. K. W. Butzer, "Environment, Culture, and Human Evolution," *American Scientist* 65 (1977): 572–84.

phasized historical human ecology, including such themes as demography, long-term population cycles, famine, the Industrial Revolution, social justice, war and genocide, and Third World exploitation. Such an agenda was perceived as radical in the conservative framework of Swiss geography.

Meanwhile, in 1980 my wife and I had begun a totally new, long-term project in Spain, studying a group of mountain villages across the last nine hundred years, from the Muslim past to the Christian present. Elisabeth worked in the archives and did ethnographic work in "our" village, while I excavated Islamic sites and studied traditional land use. This intensive historical and cultural ecology marked a new career shift for me that incorporated ethnicity and decision making. But I saw it as the logical closing of a long circle, during which I acquired the necessary skills, experience, and understanding to work comfortably at the interface between physical and human geography.

After I returned to Chicago, it was only a matter of time before I made an alternative career move. It came in 1984 when I accepted a university professorship at Texas. In Austin, I was once more able to interact with superior students, and my teaching evolved with my research, increasingly focused on ethnicity and on what I call the intellectual encounter of the Old and New Worlds in colonial Mexico. Some of my Ph.D. students at Texas have been geomorphologists, others have been human geographers, but none are narrowly subdisciplinary.

Just as my old Wisconsin department was later reconstituted anew, American geography has moved from a closed to an open system. Perhaps it now qualifies as one of the least inhibited of the social sciences, so much so that the Sturm und Drang of my own generation will seem like a tale from the Iron Age to the irrepressible baby boomers who simply ignored the military uniforms in the closet. In their wake, I, too, found freedom. Not that I rediscovered geography. It seems, though, that I have been reclaimed, and that is a good feeling.