MY first encounter with academic geography occurred in the fall of 1919, when I entered Central Michigan Normal School, now called by the presumably more impressive name Central Michigan University. Aside from satisfying my avid intellectual curiosity, my object, commensurate with my circumstances at the time, was to obtain a high-school teaching certificate. The requirements included a course or two in geography, in which I enrolled with little expectation of intellectual stimulation. The professor of geography at Mount Pleasant at the time was R. D. Calkins (1873–1955). He had studied at Ypsilanti and then gone to the University of Chicago for graduate work in geography and geology. After a year's teaching at Ypsilanti at the time when Mark Jefferson commenced his career there, he moved to Mount Pleasant, where he remained until his retirement.

Calkins was a huge man with beetling brows, who had brought from Chicago the "Socratic" method of teaching practiced by R. D. Salisbury. His overwhelming appearance when ensconced behind his lecture table, his abrupt manner of speaking, and especially his rapid fire of question after question at often timid and bewildered students disguised a thoroughly benevolent nature. Carl Sauer once described him as "a bear with the disposition of a Newfoundland dog." Finding much in his teaching that interested me, I took more courses with him during my year at Mount Pleasant than with any other instructor: physical and historical geology, geomorphology (which he called "topography," and in which I was asked for the first of three times in my career as a student to read in W. M. Davis's Geographical Essays), and an excellent field course in the spring quarter. On our Saturday excursions we found much of the morphologic work of streams exemplified along the course of the Chippewa River, which flows through Mount Pleasant. To the west of the town we became acquainted with morainic deposits, and to the east with the deltaic and littoral deposits of the Pleistocene predecessor of Saginaw Bay. The land forms shaped by the Wisconsin ice sheet and its associated streams and lakes, of which I was later to learn more from Frank Leverett at the University of Michigan, interested me more than anything else offered to me as an undergraduate.

Calkins had a definition of geography, the one that W. M. Davis had persistently proclaimed: it is the study of the relations that subsist between the physical environment and organisms, essentially the human species.1 But in my greenness I was repelled by that definition. Surely, I told myself, no respectable intellectual discipline can be merely the symbol of a ratio (:) between two bodies of fact for which it takes no responsibility. If that was geography, it had no appeal to me; I was looking for substantial information. Fortunately, Davis's stultifying definition did not affect the solid instruction Calkins gave his students.

Toward the end of the academic year 1919–20 Calkins urged me to go to the University of Michigan the following year. "You have got all you can get here," he told me. Since I was wholly dependent on my own earnings for my support, he obtained for me an assistantship in the department of geology and geography at Ann Arbor, through Carl Sauer and the head of the department, W. H. Hobbs. Sauer evidently gained from his correspondence with Calkins the impression that I intended to work in geography at the university. I had no suspicion of his misconception, but immediately attacked the formidable list of courses offered...

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in geology and mineralogy, then taught in separate departments at the University of Michigan. I thus had little to do with Sauer for two years; on the few occasions when I had any contact with him he treated me with perceptible coolness.

APPRENTICESHIP

My association with Sauer began in the summer of 1922, when with the inauguration of the Michigan Land Economic Survey, which he and others had been promoting, he offered me a summer job as field assistant on the Survey. I accepted gladly, for I needed summer earnings to supplement my meager salary as assistant, and had no prospect of better work during the vacation. I found the field season of 1922, in which we mapped Charlevoix County, in the northwestern part of the Lower Peninsula of Michigan, both enjoyable and intellectually profitable. There were two field parties, each with its own camping equipment and cook, supervised by the men responsible for the two phases of mapping: L. R. Schoenemann, of the United States Bureau of Soils, and L. J. Young, of the Department of Forestry, University of Michigan. Sauer, in general charge, looked after logistics and gathered information from county archives and by interviews. Each party was organized by pairs of assistants, who worked together. One member of each pair mapped, on printed sheets representing mile-square sections, the surface forms, essentially soils, and the other the "cover," including natural vegetation and agricultural or other use. The assistants were almost all students of forestry or soils from the University of Michigan and what is now Michigan State University, then called Michigan Agricultural College. My party included, however, one graduate student in geography from the University of Chicago, Stanley Dodge, who later became a member of the faculty at Ann Arbor.

My principal profits from this summer's work were acquaintance with and respect for Sauer, and valuable field experience, especially with glacial and fluvioglacial forms I had not seen in central and southeastern Michigan. The relation of our work to geography as I had heard it defined was not clear to me, but the work in itself was satisfying. It certainly helped to lay the foundation for my later conviction that any definition of geography narrower than the etymology of the word blinds the eye to a part of the abundance of interesting objects the earth displays. During the following year I took two courses from Sauer, one on the geography of Michigan, which I found too one-sided in the inordinate attention he paid, in accordance with his interests at the time, to the use of the land; and one called "Land Utilization," in which we examined other parts of the United States from the same viewpoint. Sauer asked his students to draw a good many statistical maps. I had some skill in drawing and lettering, and probably turned in the best maps submitted in these courses.

In the spring of 1923 Sauer accepted his appointment as professor at Berkeley, beginning with the next academic year. To my surprise (and relief, since Hobbs had informed me that I could not expect a renewal of my assistantship) Sauer offered me a position at Berkeley. I suspect that the maps I had produced in his courses led him to consider me for the prospective position, since one of the subjects he wished me to introduce at Berkeley was map drawing. He said that he had been hampered by lack of skill in drawing maps, and wished to spare his students that handicap. He also invited me to work during the summer of 1923 as field assistant in Kentucky; he had undertaken to write a regional monograph on a part of that state, which he called the Pennyroyal, for the Kentucky Geological Survey. My job would be to make detailed maps of small representative areas within the general region, in much the same way as we had mapped Charlevoix County, Michigan, the preceding summer. I would work in the eastern part of the Pennyroyal, Clarence Newman in the western part.

My field work in Kentucky posed more difficulties than the well-organized Michigan survey had. I was completely on my own resources, seeing neither Sauer nor anyone else connected with the work while I was in the field. I selected the small areas to be mapped, and used such methods as I was capable of applying. Except for one area near Mammoth Cave, I had no topographic maps to serve as bases. There were no marked section lines of the federal land survey, such as we had had in Michigan, that might be used as the skeletons of maps. But I knew something about mapping in the field. I had taken a course in surveying at Ann Arbor, had experience with the plane table and telescopic alidade, and had learned the
previous summer how mapping was done in the federal soil survey. Before leaving Ann Arbor I provided myself with an improvised plane table in the form of a small drawing board fitted with a brass plate tapped to receive the screw at the head of my camera tripod, and with a sight alidade of the sort used in the federal soil survey, made by fastening folding sights to a six-inch engineer's scale. These, with a military sighting compass and a protractor, constituted my one-man surveying equipment. I soon found, however, that setting up and orienting my improvised plane table wasted much time, and that I accumulated too much error when traversing the crooked roads I used as the skeletons of my maps. My solution was to plot on my drawing board from compass sightings and paced distances, using compass intersections of features distinguishable at a distance as a weak control.

That summer's work in the field did not complete my part in Sauer's Pennroyal book. After we had moved to Berkeley he employed me to draw the maps and diagrams for it. I learned a great deal about drawing for reproduction on that job; looking at my maps after more than fifty years I find most of them quite creditable for a self-taught beginner, and two or three of them as good as any I have drawn since. One, however, did not satisfy me. It is a relief map of the whole area, executed in the manner later made familiar by Erwin Raisz, A. K. Lobeck, and Guy-Harold Smith: by drawing relief features in pseudo-perspective on a map base not drawn in perspective. I found it impossible to make entrenched meanders such as those of the Cumberland River "lie down" among meander spurs without distorting the map base, and therefore retained a distaste for the procedure, even after seeing much of Erwin Raisz's superlative draftsmanship.

AT BERKELEY

The appointment at Berkeley that Sauer obtained for me in 1923 was as "associate," an anomalous rank in which one might pursue graduate study while teaching certain courses independently. Besides teaching assistants, Sauer and two associates—the other one was Richard Joel Russell, who was working toward his doctorate in geology—made up the teaching staff of the department during the first two years of Sauer's tenure. I doubt that anyone of my generation or later was ever given responsibility for upper-division courses with so little formal preparation as I had. The instruction in geography I had received did not remotely approach what is normally required for an undergraduate "major." Such indifference toward the undergraduate backgrounds of graduate students was habitual with Sauer: few who earned the doctorate under him had taken their bachelor's degrees in geography. He trusted the students' native intelligence and ingenuity to overcome such obstacles as they encountered in their reading and in their original investigations, and in my case in my teaching.

As a graduate student I browsed about the campus to satisfy my curiosities, though I neglected to repair many deficiencies in my general education. A. L. Kroeber admitted me to a seminar in anthropology, in which any traces of geographical determinism I might have acquired were wiped out. In geology, fulfilling a part of the requirement of a "minor" as a doctoral candidate, I was asked for the third time, in a seminar in geomorphology under J. P. Buwalda, to read in Davis's Geographical Essays. Thoroughly tired of Davis by this time, I brought to the class as a counterweight Alfred Hettner's anti-Davisian Die Oberflächenformen des Festlandes (1921), which Buwalda did not know, and read passages from it in translation. I took courses in plant geography with W. A. Setchell, of the department of botany, and in forest ecology with A. W. Sampson; and audited a seminar in soils with C. F. Shaw. Such supplementary study in other departments became standard practice among our graduate students; instructors were usually lenient about "prerequisites." Once Sauer encouraged Alvena Suhl (Storm) to take an advanced course in the department of philosophy on Kant's Critique of Pure Reason, though her preparation for it was meager. (She survived the course.) Fred Kniffen and Peveril Meigs even did field work in ethnography, and Kniffen published in the University's serial Publications in American Archaeology and Ethnology.

2 Carl Ortwin Sauer, Geography of the Pennroyal, The Kentucky Geological Survey, Ser. 6, Vol. 25 (1927). The map referred to later in the paragraph is Figure 4.
which I had had any instruction. I was, moreover, ignorant of how—or whether—such courses were taught elsewhere. No textbooks were available; indeed, textbooks were used very little in the department. In climatology I at first leaned heavily on R. DeC. Ward's excellent translation of the first volume of Julius Hann's somewhat antiquated *Handbuch der Klimatologie.* But in my voracious reading I soon discovered, in addition to much other material later than Hann, Köppen's classification of climates of 1918, and adopted it as an appropriate summation of my instruction. When after two or three years Sauer turned over to me the lectures in our beginning course in physical geography I introduced the Köppen classification as a prominent constituent of the course, and redrew the Köppen world map for reproduction in the mimeographed syllabus we prepared for the students. Whether the Köppen system was used elsewhere in elementary courses in the middle twenties I don't know; at any rate our use of it at Berkeley was original.

**THE BERKELEY ATMOSPHERE**

Both our teaching and our original work at Berkeley proceeded independently of what was being done elsewhere in the country. Sauer had turned deliberately from the path pointed out to him when he was a student at Chicago. As for his graduate students, not being guided into any narrow path, we were entirely free to find our own. We were isolated from other American foci of geographical work not only by distance, but also by Sauer's generally low opinion of his American colleagues. We were not isolated, however, from past and contemporary work in Europe, where academic geography had not been confined by strait-jackets of restrictive definitions. Both our wide reading and the men Sauer brought to Berkeley supplemented our small staff contributed to our breadth of perspective. When Sauer had an opportunity to add to the staff he recruited Oskar Schmieder, then at Córdoba, Argentina, who had taken his degree at Heidelberg under Hettner. Schmieder returned to Germany in 1930. He was succeeded by Gottfried Pfeifer, who first came to Berkeley on a traveling fellowship. Albrecht Penck came for a semester in 1925, and Wolfgang Panzer for a year at the end of the twenties.

Sauer did invite some of his Middle Western colleagues to Berkeley for summer sessions, when few regular students were in attendance. In the twenties and thirties these included Wellington Jones, Kenneth McMurtry, Stanley Dodge, and Preston James. Occasional visitors whose names we knew passed through: I recall R. H. Whitbeck, Charles Colby, and Ellsworth Huntington. Seen at close range they were not impressive when measured against figures we knew from our reading. Huntington, with his little stock of *idées fixes,* seemed particularly narrow-minded. By far the liveliest of these fleeting visitors was Griffith Taylor, who stopped briefly in Berkeley on his way from Australia to Toronto. We had a better opportunity for judging the greatest of them all, W. M. Davis, when in 1925 the department of geology brought him to Berkeley for a semester. I took the opportunity to enroll in his seminar. It turned out to be, however, not a seminar as I understood the term, but a continuous monologue by Davis, in which he expounded ideas long familiar from his *Geographical Essays,* to which he had nothing new to add. The only relief was when he occasionally took chalk in hand and exhibited on the blackboard his admirable skill in drawing. The greatest of all was the greatest disappointment.

At a time when departments of geography in the United States were abandoning the physical earth, physical geography remained prominent at Berkeley. We became acquainted with Walther Penck's *Die morphologische Analyse* (1924) soon after it appeared, promptly recognizing the light that his *Massenbewegung* threw on the detailed forms of the Coast Ranges in our part of California, with their highly disturbed structure of mingled weak and resistant rocks. Penck opened our eyes to the meaning of the gently curved lines of fences originally built straight but now sagging downhill, their posts leaning as they were carried with the soil in which they were set; and of the presence in canyons (in California the general term for all narrow valleys) of trees leaning in the direction of slope, their roots dragging behind them as they moved toward the bottoms of the canyons, eventually to fall into the channels of the ephemeral streams. We learned to

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see the sculpture of the land surface as proceeding more by migration of the cover of weathered rock down slopes than by stream erosion. The work of streams, most of which here flow only in winter, seemed to consist more in sluicing away the waste fed into them by creep from their tributary slopes than in the erosion of their beds. After Dick Russell left Berkeley, responsibility for geomorphology devolved on John Kesseli, whose dissertation, a meticulous study of the deposits of Pleistocene mountain glaciers on the east side of the Sierra Nevada, was one of the best ever submitted in the department.

Economic geography, for which Sauer preferred the older and less pretentious term “commercial geography,” and which seems to have been elsewhere a favorite topic in the twenties, was inconspicuous at Berkeley. For a time Sauer offered a course on “primary production,” mainly for the sake of the College of Commerce, now the School of Business Administration, which expected something of the sort from a department of geography. Pfeifer brought the economic-historical viewpoint he had acquired from Leo Waibel, but economic geography gained a firm foothold in the department only when Jan Broek joined it in 1936. Sauer’s view of economic activity shifted, first to “conservation” and then to “destructive exploitation,” which he came to see as the characteristic mode of action by industrial economies on the earth.

If I were asked to name an academic specimen that best exemplifies the thinking current in the department at Berkeley in the twenties it would not be one of our own dissertations, but Jan Broek’s on the Santa Clara Valley, submitted at Utrecht in 1932. Broek, here on a traveling fellowship, gathered the material for his dissertation from a base in the department in 1930 and 1931, working almost wholly in the spirit of Sauer’s concept of the cultural landscape. With much skill and full illustration, Broek recorded the details of the landscape in a well-defined area, reconstructed past landscapes, and recorded the remains of those earlier landscapes preserved into the present. No other work that I know demonstrates so well Sauer’s “cultural landscape . . . fashioned from a natural landscape by a culture group,” in this instance two successive culture groups. Broek preferred to see, however, only one landscape, in which the natural and the cultural are inextricably associated.

Sauer’s discovery of Mexico, of Baja California to begin with, determined for many years the field in which most of his students found material for their dissertations. It also made obsolete, in his department, the writing of descriptive accounts of small areas, which were popular in the late twenties and the thirties, partly, perhaps, because of Sauer’s earlier advocacy. We accepted a few masters’ theses of this sort, but by about 1930 I, at least, became disillusioned with them. The earliest recorded statement I have found in which Sauer expressed his dissatisfaction with “regional” studies is in a letter to J. E. Spencer dated December 6, 1934: “A thesis must have a thesis, and regional studies are likely to be pointless or diffuse as to formulation of problems. . . . I know that it was not a very good thing for me to do a regional thesis, though one was virtually forced to do so at my school and in my day.” Field study was to be directed toward the solution of problems rather than toward the characterization of the area in which the work was done.

The quality of the department at Berkeley in the twenties and thirties owed much to its small size. After Sauer’s first year, graduate students began to arrive from other states, but almost all could be provided with assistantships for their support. They were not confronted by a long list of courses that bound them to the department, but had an opportunity to broaden their horizons by taking courses elsewhere on the campus. In particular, Sauer avoided loading students with “regional” courses: he made no attempt to provide a “regional” coverage of the earth, but limited such courses to areas well known to particular instructors. Schmieder was qualified to speak from first-hand knowledge of Pampean and Andean South America, and Sauer, after a few years, of Mexico. European visitors could speak of their native continent. The great ad-

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vantage of small size was that all instructors could become personally acquainted with all graduate students, and so could judge them without recourse to mechanical devices such as written qualifying examinations. Probably the largest number of graduate students that can be thus known is less than twenty. By this measure the department did not become too large until after the second world war, when enrollment in the universities greatly increased. Sauer kept a critical eye on his graduate students, tactfully eliminating those who did not come up to his standards. The qualities he valued most were eagerness to work in the field and ability to see what the landscape offers. Probably none of his graduate students, however, completely fulfilled his expectations. He did not ask aspirants to the Ph.D. to earn preliminary masters' degrees, but encouraged them to proceed directly from the baccalaureate to the doctorate. He trusted students who were doctoral material, after some substantial undergraduate courses and a few seminars in which they might demonstrate their competence, to go into the field and gather material for their dissertations. Most had an opportunity to learn by accompanying him on his many forays into Mexico. All added to their competence by acquiring needed knowledge in other departments, taking courses that ranged from taxonomic botany to Spanish paleography.

Sauer always disclaimed any intention of founding a "school" or of shaping students over a common last. He expected the individual to find his own objects of interest and his own ways of satisfying his curiosity. What we had in common we gained mostly from Sauer's attention to the past of geography. Of his seminars I remember only those on the history of geography. In these we learned to appreciate the antiquity, the continuity, and the dignity of an intellectual concern with the earth. We were thus immunized against infection by the numerous "new geographies" whose birth has been proclaimed repeatedly throughout the history of academic geography in the United States. We were spared, too, the anxiety that seems to have afflicted many of our colleagues elsewhere, the apparent fear that the position of geography in the universities is precarious, and needs defense by frequent assertions of its special niche, redefined at intervals, in the academic structure. Best of all, we acquired a sense of participating in the worldwide and perennial enterprise of intellectual discovery, of which the university, the department, the course of instruction, and even the single lecture, are parts, however minute. We caught at least a glimpse of Isaac Newton's great unexplored "ocean of truth," on the shore of which we might, with effort and some luck, pick up our own small handfuls of smooth pebbles.

THE LATER SAUER YEARS

James J. Parsons

Why Geography? Why Berkeley? I had taken an undergraduate degree from the University of California in economics and had learned about the existence of geography through chance encounters with Carl Sauer in courses on North America and Latin America. When I felt ready to go back for a year of graduate work in 1938, geography promised a convenient departmental base. There had been no reply to an inquiry concerning admission, but it turned out that the doors were open to anyone with the academic requisites.

At that time geography at Berkeley was still less a department than an individual. Sauer had been on the campus for fifteen years, quietly making his mark, and carrying the subject along with him. Geography, one sensed, was largely synonymous with his name. He had assembled a small supporting cast with striking individual personalities and vigorous scholarly interests in John Leighly, John Kessel, and Jan O. M. Broek, the last two from Switzerland and the Netherlands, respectively. Erhard Roslund, fresh from the Swedish merchant marine, joined the staff as lecturer in 1945, as I did two years later. Clarence Glacken came in 1952. The strong European flavor of the department had earlier been enhanced by Oscar Schmieder and Gottfried Pfeifer, both of whom had returned home to German pro-