A Sociologist

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Sociologist

1. High School and Into the Navy.

1942. The story has its start in Kingman, Arizona around December 7, 1942—a year after the United States Navy was attacked at Pearl Harbor, near Honolulu, Hawaii. I was still 16 years old. At the time there was not the slightest hint that this had anything to do with sociology. Actually I never heard of the field until WWII was long past. Still, as will be seen, much later lessons learned in the Navy marked my style of sociology.

1938. Concerning radar, the story may also be seen as beginning when I was 12 years old and we were living in Winslow, Arizona, where Dad was managing a grocery store, waiting out the Depression so he could get back to his first love, surveying. I read an article in *Popular Science* that told of a 'ray the Navy had that could see 50 miles out to sea, even through fog': radar, though even the word was secret at the time.

1941. War in the Pacific. In December the Japanese navy bombed Pearl Harbor. Immediately afterward a Japanese submarine shelled some oil tanks near Santa Barbara. We were living in Superior, Arizona. Everyone expected the Japanese to follow up the bombing in Hawaii with an invasion on the shore of California. If they had done it they would have had an easy time of it. Our shores were wide open. In Superior, the kids my age, 15 and too young to volunteer, planned to take our 22 rifles and go into the mountains as guerilla fighters.

President Roosevelt ordered all the Japanese and their descendants into makeshift relocation centers. Today it's popular to criticize his doing this and even to pay reparations. But was he right? Almost all of them were ordinary civilians, many of whom were citizens. Also, it was a serious injustice that many Californians stole the farms the Japanese had cared for. But was there no way to know which ones would sabotage whatever defensive actions or destroy whatever they could? Was the FBI too weak to do the necessary investigations? Or was it perhaps a pandering to the prejudices and avarice of powerful Californians?

1942. By December 1942, we had moved to Kingman, Arizona so Dad could return to surveying. He worked at a US Air Force base. I was a senior in high school, with enough credits to graduate at the end of the first semester. Navy recruiter came to talk to us students. He told us about Navy electronics and might have gone on to say knowledge of this would be useful after the War. Or maybe I jumped to that conclusion by myself. And the high school courses it required were the same as those the University of Arizona demanded for those who wanted to major in engineering, the field I had always before thought I would go into. The Navy recruiter said that if we wanted to try for it we would have to have a high score on the General Classification test and then pass something called the Eddy Test. If we were successful we could be trained to work on electronic equipment. He also told us we could prepare for the test by attending a National Youth Administration program in radio functioning and repair. The school was in Tempe.

1943. By the end of the semester in January 1943 I had enough credits to graduate. So I decided to leave school and earn some money so I could go to Tempe and take the NYA course. Not that I was broke. I had been making money all fall anyway, serving breakfast to the men who were building the Kingman Air Force Base and delivering groceries after school. But I needed more. At first I took a job helping a small trucking company run freight between Kingman and Phoenix. Then a better opportunity arose. Dad was helping to survey the air base and knew a lot about the contractors there. He was working for Del Webb. With his advice I took a job with another contractor on a survey crew. It paid good money.

Around March of that year I took a bus to Tempe. Three months later I had finished the course. I needed to get to Holbrook where Mom, brother Darrell and our sister Ruth were then living. (Dad had gone with Del Webb to a job in California.) But I was broke. So I spent a week or so earning 50 cents a day for cleaning peoples' yards. I survived on two quarts of buttermilk each day, and slept illegally in a dorm of Arizona State College (Arizona State University).

Somehow I managed to get to Holbrook, probably hitch hiking. I was ready to enter the Navy. But at 17 the law required that my parents would have to sign a statement of permission. They did—oddly, they always did what I asked them to. They didn't seem to worry about me, unlike the concern they had for Ruth and Darrell.)

2. Military Service. I hitch hiked to Phoenix to sign up for the Navy and passed the physical examination and whatever else the Navy required. I joined the Navy on July 13, 1943. After the exams they put me and a couple of other kids on a train to San Diego, to spend 12 weeks in boot camp there. The others were a Mexican and an Apache Indian. Soon after arriving we took the General Classification Test. I was horrified at my score of 72. But most of the other kids, practically all from Los Angeles, had scores way down in the 20s and 30s. The Mexican and the Indian scored 70 and 69. It turned out that the three of us were among the top four scorers. Later I learned that my 72 really meant a little above two standard deviations above the mean of 50. So I got to take the Eddy Test and passed it. The Mexican was sent to quartermaster school, the Apache to the signal corps. I was put on a train to Michigan City, Indiana to begin a year of training. The first six weeks were at Michigan City. The next three months were at Oklahoma State College in Stillwater. The rest of the year or so was in radio and radar at the Naval Technical Training Center (NATTC), Ward Island, Corpus Christi, Texas.

This experience, it turned out much later, was a big help in our stratification research. The schematic diagrams of radar sets showed the paths that electrons flowed in the equipment. Path analysis, invented by Sewell Wright (1922), was introduced into sociology be Otis Dudley Duncan. Sewell and I picked up from him—he and Sewell were lifelong friends. The first paper on the status attainment process (with Alex Portes and Bill Sewell) showed the path analysis of the process with a diagram that looked just like a small scale version of a radar schematic diagram. Alex presented it at meetings of the American Sociological Association in1967. I still have a bound copy of this version.

Returning to Corpus Christi, at the end of the NATTC course I had delayed orders of 10 days or so to make my way to San Francisco to be shipped out of the States. I was sent to Barbers Point Naval Air Station, Oahu, Territory of Hawaii, where I spent the rest of the War working in the Station's electronics laboratory; almost everything we did was classified (i.e., secret). My job, as an Aviation Technicians Mate 2/c, was to repair secret electronic equipment. The most secret of all was IFF—automatic interrogation—'Interrogation Friend or Foe'. 'Secret' meant that I was to be shot by our officers if threatened with capture. When injected, sodium pentothal, a 'truth serum', would make it impossible for us to lie. Of course, the Japanese knew all about that and would have used it.

Oahu was a safe place long before I arrived. But this didn't mean that the War far out in the Pacific was meaningless to me. I knew a lot about the slow and killing progress from island to island that the sailors and marines were dying for, not only in 1944 – 1945, but also in the early years of the Pacific War. And later, when Iwo Jima and Okinawa were under attack, we watched the hospital planes flying over our island carrying the wounded to the naval hospital—one after another, one coming over the horizon as the one in front of it was descending to land.

For another thing, the culture of the sailors and marines was the same all over the Pacific as were our general awareness of what others on other islands were doing and had done. And our knowledge of it all dated from the first attacks in the Philippine Islands and on down to Guadalcanal, Truk, Tarawa—where so many were slaughtered while storming the beaches, at least one of them a man I knew. And while I was at Barbers Point Naval Base on Oahu a group of my friends from Corpus Christi, including Jean Rowe, my friend from high school days in Winslow, came to our base for night fighter training. I tried to join them but my request was refused by our officers. Was it good luck that they did? Their unit, Night Fighter Air Group 33, went to Okinawa on a small aircraft carrier called the Sangamon Bay. It was kamakazied. Fortunately none of my friends were killed, but their commanding officer was. And one of our friends was blown off the ship and struggled in the ocean for three hours until a passing destroyer saw him and picked him up. I saw all the group after their ship was towed back to Pearl. They were given a few weeks of rest and recreation before being sent back to the western Pacific…They gave me a small piece of the plane that hit them. It was spattered with the blood of the Japanese pilot. I kept it for years before deciding to get rid of it.

If you want to know what it was like, read James A. Mitchner's *Tales of the South Pacific.* It consists of stories about real navy people, what they did and when and how some of them died. I was never in that part of the world. But our lives and times were not all that different. To this day memories from those years bring tears to my eyes.

On April 1, 1946 I was discharged from the Navy at Long Beach, CA and returned to Arizona intending to enroll in engineering at the University of Arizona in Tucson.

Side story: As far back as I can remember it seems to have been assumed that the three of us, brother Darrell, sister Ruth, and I would go to college. In reality, however, there was no way to pay for it. The War did it for Darrell and me. As the War went on, Congress passed the GI Bill. It provided tuition and a small stipend for veterans to attend college. The number of months was contingent upon the amount of time of military service. Actually, I had only put in a bit less than three years. For that I received enough to take me through undergraduate, masters, and a year on the doctorate. I don't know how much Darrell got, but it was at least enough to put him through Arizona State. Ruth was too young to have seen military service. But she, who incidentally was much younger than Darrell and I, was an excellent student through high school. She entered a California contest to write a paper, on something related to patriotism as I recall, and won first prize. That gave her the four years she studied at a private college (Redlands University) near to where the family had moved well after the War. So despite the odds, we all went to college.

3. St. Paul, Minnesota.

1946. While still at Barbers Point three of us, Ralph Swonger, myself and another sailor agreed to roam the nation after we got out. In keeping with this, after enrolling at the University for the fall semester of 1946 I bought a 1934 Chevy for $800 I'd saved while in Service and set off for St. Paul, MN to pick up Ralph. I named the car the Gizmobile. It was a crazy trip. I said goodbye to the family, then still living again in Holbrook, and began the trek.

Gizmobile's tires were bad but everything else seemed OK. From Holbrook I went east to Gallup, New Mexico and then turned north toward Durango, Colorado. I was on the road in the midst of the Navajo reservation. A tire blew out. There was no jack in the car. There was almost no traffic on the road. I was stuck in the middle of nowhere. A wagon with a Navajo family came slowly toward me from the north. I guess they must have been suspicious. They gave me a wide berth. Night fell. I slept in the car. In the morning a passing motorist stopped and let me use his jack. So the tire got fixed and I was on my way again. At Durango, I bought some other tires, then headed toward Denver via what was called Wolf Pass, eleven thousand feet as I recall. The radiator boiled over. That too was fixed. I arrived in Denver the next day. There I stayed a few days with someone I'd known in the Navy, then on to St. Paul. That took two or three days, each with its own mishap. But I finally got there. What to do and where to find Ralph's place? I knew nothing at all about St. Paul. By accident I got onto an avenue that took me to the street where Ralph lived. He and his family put me up.

But by this time Ralph had changed his mind. He was no longer interested in leaving the city—possibly because he was in love with the girl he eventually married.

Broke again, I got a job or two and stayed with the Swongers the rest of the 1946 summer. It was there, through Ralph's girl friend, I met Hazel. We decided to get married the following February. I sold or junked the Gizmobile and took a bus back to Tucson for the fall semester at the University of Arizona.

Broke still another time, I took a job as night clerk at a residential hotel—on top of 15 credit hours at the university. The hotel was in easy walking distance of the dorm where I stayed when I wasn't working or studying. So I piled up some money to pay for the forthcoming early 1947 bus trip back to St. Paul.

I did a bad job in my engineering courses but got A's in English. It must be added that the teaching in the engineering curriculum, apart from draftsmanship, was pretty bad--especially chemistry. In the English class I wrote a rather prophetic paper. It will be recalled that the Americans dropped atomic bombs on Japan. With that in mind and from the Navy experience I put together what I had learned about electronics together with an awareness of the growing menace of the Soviet Union. I reasoned that a new horrible weapon would soon be invented—a flying bomb powered by atomic energy that would carry an atomic bomb. Such a weapon, I thought, could be exploded at wherever it was directed, not by internal guidance, but by being 'called' from an electronic device at the target. Later, such devices came into being and were called intercontinental ballistic weapons—and they had their own internal guidance systems.

1947. Hazel Laura Zimmermann and I were married on schedule in February. I took a job as a laboratory assistant at the research unit of Minnesota Mining and Manufacturing Co., later known as 3M. My job was to run experiments on the production of fluorocarbon compounds. That was a great experience. I learned a lot about chemistry, adding a bit more science and technology knowledge to the electronics the Navy gave me.

Hazel worked as a secretary during the day and I worked night shift at the lab. By that time I had developed an interest in philosophy. So during the day I would walk to the St. Paul Library and read what I could find. But this turned out to be fruitless. Clearly, I needed to go back to college. Hazel strongly supported the idea. However, I needed guidance to decide what to study. Philosophy? Science? Something else? To get professional advice I talked with the Veterans Administration counselors. They gave me some tests to help decide and told me that my interests were similar to those of personnel administrators and sociologists.

That was the first time I ever heard of sociology.

1948-1950. I enrolled at Hamline University in St. Paul in February, 1948, to look into both sociology and philosophy. While there I took a course in social psychology along the lines of what later was called 'symbolic interaction theory'. That and the structure of societal hierarchies, called stratification, were the subfields I have followed the rest of my career. At Hamline, I took classes from Arthur Williamson in medieval and ancient Roman history. He was one of the two professors from whom I learned the most.

The other was Leland Cooper, an anthropologist. More is said about them just below. Before this another small Hamline story must be told.

We had an erudite professor of English, named Beyer, with whom I took a course on John Milton. One of our requirements was to write a Miltonic sonnet. He liked mine and had it published in a Hamline magazine. This was my second publication. The first was a bit of doggerel published by the Barber's Point Navy newspaper in 1944. Neither one of them was written, however, with any forethought of publication. Other people decided to publish the two poems.

At Hamline I took a course in astronomy in my last semester. Among other things, we studied a bit about Kepler and his theory of the time it takes for a planet to go around the sun. I wondered if his planetary laws applied to satellites of planets. The professor said no one had ever raised that question before and suggested that I test the idea. I did it on a satellite of Mars. Keppler's law was *almost* right. By my calculations, for Mars it was off by four minutes. Many years later I read that some scientists had just recently performed the same calculations and come up with the same answer as I had. This was one of the three most important insights of my pre-professional life.

Another, mentioned above, was what was written in the University of Arizona paper in 1946, on intercontinental ballistic weapons. It anticipated the appearance of these weapons by many years. I thought they would be powered by atomic energy and would carry atomic bombs. (Much later—1959-1965—I became the point man to rebuild the infrastructure around Detroit and Chicago in the event of an atomic bombing by the USSR. Of course, it never happened.)

The third was on the unexpected victory of Truman over Dewey (around 1959?). The pollsters had announced that Dewey would win. He lost. So a bunch of Harvard professors, embarrassed by Truman's victory and the pollsters' failure to predict it, published a book intended to explain why things went wrong in spite of our excellent science of polling. Basically, what they said was that sampling relies on probability estimates, and once in 20 times the wrong answer would come up. Of course, they didn't take the trouble to look at the evidence. I did, in a course with Hans Gerth.

In his seminar, Gerth let me do an analysis of the state-by-state polling results and their real outcomes. I found that over the 48 states the pollsters overestimated the Dewey results by about two percent each. That was enough to throw Ohio to Truman and defeat Dewey. The Harvard people should have looked at the evidence instead of speculating.

In the last year at Hamline (1950) I learned about correlation coefficients. This was in a sociology course on research methods. Actually, the professor didn't know anything about statistical analysis. But he must have mentioned something about correlation, and, surprisingly, he asked me to teach the class about them. I had gone over the a bookstore near the University of Minnesota and bought a little book on correlation by Thurstone, as well as a well known tome (Yule and Kendall) on statistical analysis. Correlation excited me. This was a method by which you could calculate the relationship between two variables, and state it in a single number. What I realized was that a correlation coefficient summarizing the overall relationship between two (linearly distributed) variables would provide a neat way to think about their relationship.

A word or two should be said about this. Why were correlation coefficients so interesting to me at the time? The answer goes back to around 1947, when I worked at 3M's lab. One day I became curious about the accuracy of a measuring device we were using, and decided to test it repeatedly. The tests yielded a frequency distribution of hundreds of specific observations, that was more or less linear and that varied around an implied regression line—a concept I didn't know about then but learned about much later. The coefficient of correlation offered a way to summarize the distribution I'd seen when testing the measuring instrument.

That last year at Hamline, following the advice of the chair of the sociology department, Robert Martin, I did a senior thesis—for him as much as for me. He taught a regular course on marriage and the family and wanted to find out how such courses were handled elsewhere. So I designed a kind of stratified sampling technique to contact other universities where marriage and the family were taught. Another senior student, William Olsen joined me in this. We mailed a questionnaire to a hundred or so such universities, and got a pretty fair return rate. Then we wrote up the results and shortly afterward published an article on our work. It was called "Courses in Marriage and the Family in 100+ US Universities" It came out in the *Alpha Kappa Delta Quarterly* (1951) (*AKDQ*). This was my first sociological publication.

Besides the academic work, quite a few of Hamline's students were politically active. I was one of them, and pulled together a small group of like-minded people and together we brought out a mimeographed paper we called the Pipeline (Hamline's mascot was the Pied Piper of Hamlin Town). The paper was run off by a couple from Milwaukee who were pretty far left. They produced it with a red cover with black print—their idea, not mine. This was—quite rightly—interpreted by the university's president as Red for Communism and Black for Anarchy**.** I was called into the president's office and read the riot act. My anthropology and sociology professors, Leland Cooper and Robert Ray Martin, however, thought the whole affair was funny. So did I.

Our group was not the only one that was politically active. There was also a right wing group. It was led by a psychology student named Robert Holt. Later he became a political scientist and it was he who ran Radio Free Europe during the Cold War. Still later, he became a professor at the University of Minnesota.

Now back to Arthur Williamson and Leland Cooper—and, jumping ahead a few years, to the two other scholars to whom I owe the greatest debts. Williamson had studied with Michael Rostofzeff at the University of Wisconsin. This Russian émigré of 1922 was one of the greatest historians of the Mediterranean basin societies of the ancient world. Williamson remarked that Rostofzeff had directed a research center at Wisconsin and that his records were still there in the basement of the university library. (Much later I looked them up and used some of them in my courses.) Professor Williamson's comments about him and about the University of Wisconsin were the first to bring my attention to that university.

Leland Cooper was one of the two professors whose thinking had the greatest effect on mine. He too had studied at Wisconsin, and had been a teaching assistant under the great anthropologist, Ralph Linton. Leland was the most learned man I ever met. Fortunately he also became one of the best friends I ever had. We traveled together all over Minnesota and parts of Wisconsin. He talked steadily, and his words were jewels of information.

4. Graduate Studies at The Universities of Minnesota and Wisconsin.

1950-1951. I graduated from Hamline in 1950 and in October of that year enrolled in the Department of Sociology at the University of Minnesota. My major professor was Neal Gross. He was a member of a small group of researchers of the Rural Sociological Society. The group included Bill (William Hamilton) Sewell and Otis Dudley Duncan, both of Wisconsin, all of whom were stratification specialists. Their work and a seminar from Gross convinced me stratification was what I wanted to work on—that, along with so-called ‘status attainment theory’, a social psychological and societal structural phenomenon that’s part of stratification research.

Gross was setting up a research project to see how people of Minneapolis saw ‘social class’. He asked me to coordinate the interviewing of around 1,200 interviewees. They were from four different parts of the city, and each part had its own project overseer. So I didn’t really have much to do. More important, I got to use the data. I settled on about 600 of the interviews, those that were complete enough to use. Studying each of them I saw that each response could be analyzed into a *content* and a *structure*, special cases of what philosophers call ‘substance’ and ‘form’. In my thesis the content consisted of things like education, occupation, money, race, etc. The structure consisted of the number of classes. Thus one who said the classes were races, blacks and whites, would in effect be reporting a *content* of race and a *structure* of two classes. And one who said the classes were of educational attainment might say there were two, the educated and the uneducated, or another might say that there were four, those who only finished grade school, those who started high school but didn’t finish it, those who graduated from high school, and those who went to college—the content would be education, and the structure would be the number of classes.

This was, in my opinion, one of the most important sociological writings I ever did because it yielded the concepts of content and structure. They are central to my thinking regarding the theory of stratification.

But frankly, I was not pleased with my Minnesota professors. Gross was going off to Harvard and except for Arnold Rose in social psychology and Don Martindale (a Wisconsin PhD) in sociological theory I really didn’t want much more to do with them. Besides, I thought I could get maybe one or two more years of learning from them and that would be all. Even more important, William Sewell, Otis Dudley Duncan and Hans Gerth were at Madison.

Then in the middle of the 1950 – 1951 academic year Sewell sent me a telegram offering me a research assistantship to work on his projects. I was overjoyed. In addition, Jay and Ginny Artis were already there. Ginny was Leland Cooper's daughter, and Jay had finished Hamline a year and a half before I did and had gone to Penn State to work with Duncan, who was there that year. Jay followed Duncan to Madison. I too had, of course, been hoping to work with Duncan. But he went to Chicago before I got to Madison. Jay and Ginny were poorer than Hazel and I so Leland talked us all into renting an apartment together. We did this for two years, until Jay finished his 'prelims' (comprehensive examinations) and he and Ginny went to Vanderbilt University.

1951-1953. Gerth, also at Wisconsin, was a German émigré who had fled the Nazis. I learned about him and his work in a course on sociological theory from Don Martindale at the University of Minnesota in 1951, while working on my master's degree. At Minnesota, Professor Martindale had assigned us some of Weber's writings. Gerth and Mills had translated and published a book of them called *From Max Weber.* They introduced Weber to American sociological thought. Gerth was another one of those with encyclopedic knowledge. He also talked all the time. (Martindale once wrote a book about Hans and named it *The Mouth.*) I took several courses with him at Wisconsin, including a seminar in which he went on forever about something he was thinking about and wanted the students to do papers on. On the whole, I thought he asked too much of his students, and he wanted them to work on whatever he was thinking about at the moment. I didn't object to doing a lot of work but I wanted to do it on things that interested me. So in the seminar I asked him to do something else and he let me do it. As I said earlier, I wanted to find out why the predictions on the election of Truman and Dewey went wrong. As I said, the Harvard folks who published their 'explanation' never looked at the evidence.

Hans, Professor Gerth, usually required a paper from each of the students in his lecture courses as well as in his seminars. Sadly, for the students, he would read their drafts. Then he would demand an unrealistically huge amount of additional work on their papers. I took a couple of his lecture courses but was determined to outsmart him if I could. For one such course, on a Sunday night in the middle of the semester I read a newspaper article that gave the stage names and real names of a lot of well known entertainers. Their real names revealed the country from which they or their forbears had come from. For example if a name like Otto Schmit appeared this would mean Otto's ethnic origins were Germanic. Here was my chance to do something Hans would like and that I could use to learn a bit more about stratification. So that night I ran a simple little statistical analysis of the data and wrote up a paper with the unspeakably long title of "Changes of Entertainers' Names as Clues to the Ethnic Factor in American Social Stratification." Then I withheld it until the very last moment of the class. This was so he couldn't ask me to do something ridiculous like getting all the names of the movie stars and starlets of the day and doing the study all over again. Happily, he liked the paper very much. I guess it shouldn't be too much of a surprise. After all, he was a German émigré himself and was mistakenly thought by almost everyone to be Jewish. (This would double the stigma because prejudice against Jews was even worse then than it is today). I published this little paper in the *Alpha Kappa Delta Quarterly* in 1954. About the same year Gerth was chairing a session at the annual meetings of the American Sociological Association in Seattle. He actually cited the paper in his presentation.

Though Gerth rambled all over the place, and often off of his lecture topic, I learned a lot from him, and not only in class. My friend Murray Straus detested his historical ramblings but, both Hazel (who sat in on some of his courses as well as those of others) and I liked them. Gerth and I became good friends and I often visited with him at his home. (A few years ago I wrote a paper on his wife's tragic death, and sent copies of it to a couple of professors at Wisconsin.)

Bill Sewell was one of the most important professors of the University of Wisconsin. He was my friend, mentor, coauthor, and co-teacher. As said above, I learned about his work from comments by Minnesota professors, especially Neal Gross and Don Martindale. Bill had become famous among sociologists from his Minnesota PhD thesis and his publications on the measurement of socioeconomic status (SES) of families. I learned about his work on this and on social psychology in the fall of 1950 when I was at Minnesota, and at least as important, I not only learned about this work but also his more recent research on social structure and personality. From Hamline days, I had had a penchant for a social psychology that focused on the interaction of one's personality and one's immediate and broader social environment. So Bill's interest in this field also attracted me. I did my doctoral thesis with him on the variations of children's personalities with variations in the SES levels of their parents. Bill and I published the results together, in an article that changed the course of research on this topic (more about that later). Long afterward we published a lot together of which the most notable was also with Alejandro Portes. This last article, from 1969, was still being republished by others in 2007. Another one we did in 1970 repeated the 1969 analysis on farm boys and then went on to show how the same theoretic structure applies to Wisconsin boys from small towns, small cities, and big cities. But those pieces were not the only ones. Still during my three doctoral (1951 – 1954) and two post-doctoral years (1954 to 1959) Bill and I also published two on the relationship between the social status of a youth's parents, the youth's IQ, and the latter's educational and occupational aspiration levels. It was still being cited long afterward.

In general, Sewell had two special strengths. One was the measurement of sociological variables. The other was in testing the theories of other researchers. He seems to have had a sense of when a theory was probably wrong. If it was both wrong and influential he found ways to put it to a critical test. Sometimes this infuriated a theory's supporters. But he demolished such things so effectively that they disappeared from the literature of sociology. Sometimes, as in the case of his wiping out Freudianism from sociology, a theory might still live on in other fields. (Finally and fortunately, after all these years, Freud's nonsensical thinking is being taken down by research of neurobiologists.) On the other hand, theory *building* was not one of Sewell's strengths.

The theoretic innovations in our jointly published pieces were mostly mine alone or in one important instance, Alex Portes' and mine.

Back to 1950-1951. In October of 1950 Hazel and I enrolled at the University of Minnesota, I as a grad student, she as a freshman. We had quite a struggle to get from our one room home in a third story attic several miles south of the university a few blocks from the Mississippi River. We walked to the road that paralleled the River and hitchhiked up the two miles to the University. (Why all that trouble? It was illegal for us to park our ancient car on the campus, and a ride on the street cars would take hours from where we lived.) I remember on one of those days the temperature was 25 below zero. Hazel got frost bite on her cheeks.

At the time Minnesota was on the quarter system of terms rather than the more usual semester system. Our courses were on the main campus and our part time jobs were three or four miles away on the University's Farm Campus, hers in the Department of Biochemistry and mine in the Department of Rural Sociology—hers in the morning and mine in the afternoon. We took the university's street car to get back and forth between campuses and waved as we passed each other.

I took two courses in anthropology. One was on Middle American societies, taught by a professor from Tombstone, Arizona. I think his name was William Kelley. He was a great teacher, with a heavy emphasis on sociological theory. From him I learned about Robert Merton's work on theories of the middle range—an obvious criticism of Talcott Parson's foolish attempts at a theory to cover everything. I don't remember the name of the other anthropologist. His course was, to say the least, dull. One of Hazel's teachers was taking a course with me. That louse rejected Hazel's papers because he thought I was writing them for her—which was pernicious nonsense. That was another reason we decided to leave the Twin Cities and go to Madison.

The thesis was one of four special hurdles for the MA at Minnesota in those days. A second was a long written exam on sociological theory—which meant Talcott Parsons, whose work I despised. The third was an oral exam, supposedly over the thesis. Another was competency in a foreign language. Actually, at the time I only knew Parsons' theory of stratification.—which I thought was nonsense. My answers seemed to satisfy the professors. For the oral one I was examined pretty thoroughly. Among other things they wanted to know what I thought about the British class system. I told them I was skeptical about the popular beliefs about it, and said this would have to be determined by empirical research. I guess they didn't object to this because they passed me.

Spanish was my foreign language and I passed the exam without any trouble.

I finished the master's thesis, the required credit hours, and the other exams by the end of the spring quarter, in May or June of 1951, and presented a copy of the thesis to the graduate school.

As the reader already knows, around January of 1951 I received a telegram from Sewell offering a research assistantship to work with him at Wisconsin. So in the summer of 1951 we went to Madison. I still had some GI Bill time available. It and the assistantship gave us most of what we needed. Hazel took a half-time job for which she could set her own schedule. Between my courses and the assistantship, I was on a time-and-a-half schedule. We didn't have money enough for her to enroll at the university. But she got the permission of the professors of sociology and philosophy in whom she was interested and unofficially audited their courses. She did that all the three years that I was studying there. A University of Wisconsin education for free! Without the diploma, of course, but she never needed that anyway.

We shared an apartment with Jay and Ginny Artis and their two babies on Breese Terrace, just across the street from the football stadium. Besides the Artises, our closest friends were Fritz Fliegel, who later married my sister Ruth; Jim Copp, who later married Fritz's sister; Orlando Sepulveda and Danilo Salcedo from Santiago, Chile; and Don and Jean Burkholder. Don later left sociology for a fellowship in statistics, and then became a famous mathematical statistician. Incidentally, the Burkholders and the Fliegels both spent most of their careers at the University of Illinois. They and my sister, Ruth, still live there. Sadly, Fritz died years ago in Peshawar, Pakistan, where he and Ruth went to head up a University of Illinois project there. Danilo Sacido will come up again later in this document and much later in time.

1952 to 1954. In 1952, at Wisconsin, Bill and I published a couple of articles in *Rural Sociology* on attitudes toward education among Wisconsin farmers and factors associated with high school attendance by Wisconsin farm youth. Douglas Marshall was a co-author on both and William de Hart was too, in the other one. These pieces were done to clear up the Department of Rural Sociology's responsibility for having used Ag College funds to collect the data for them. Actually, Bill was not a party to the expenditure of the money, but as chair of the department that year, I guess he thought that if he didn't take it over it wouldn't be done, and this would give the department a black eye with the administration.

I did my PhD thesis with one of Sewell's data sets on personality traits and other information taken on Richland County, Wisconsin elementary school children. At his suggestion it was given the longish title of *A Correlation Analysis of the Relationship between Status and Personality*. The degree was granted in 1954. In 1955 we published the results in the journal *Sociometry*. Our findings totally rejected the then current scholarly belief—published by a couple of prestigious sociologists, one at Chicago and the other at Penn State—that held the middle class children were deprived because they were supposedly too tightly controlled by their parents. Today, anyone knows how foolish this idea would be. But then it was revealed wisdom. We demolished it with hard data. The truth is that it's the other way around. The lower the status of the family, the harder it is on the kids, and this is not healthy for them.

But exactly what *are* the effects on the child, other than being generally sort of bad for them? We decided to do something about that question. We used the same data set to determine the factor structure of the personality test items most highly correlated with the status of the parents. We found four factors in them, each of which was clearly identifiable. One was *concern over status*. Another was *concern over achievement*. A third was the *child's rejection of his family.* And the fourth was *nervous symptoms*. The article on it that we published in the *American Sociological Review* in 1959 was republished in three different books by other writers. So maybe it had some effect in policy circles.

A word must be said about my graduate student colleagues at Madison. To overlap a bit with what has been said before, for a couple of years Jay Artis was one of them. Others were Fritz Fliegel, who later married my sister Ruth. Fritz later became one of the foremost rural sociologists in the world, with considerable foreign experience in Brazil, Africa, and India. Murray Straus, who was a co-author on one of the papers with Sewell and me, was another. Murray too became famous, as the nation's top family sociologist. There also were two Chileanos, Orlando Sepulveda and Danilo Salcedo. After leaving Madison, both of them went on to do further work at other universities in this country. They also did fine work when they returned to Chile. I lost contact with Orlando, but had a number of contacts with Danilo both in the US and in Chile.

So why was it that such people gravitated to Wisconsin? It's cold and insufferable in the winter. And the sociology graduate program is perhaps the field's toughest training program of all American universities. I think the reason that such great sociology graduate students go there is precisely because the training is so rigorous. And it provides a rare combination of theoretic sophistication and excellence in empirical analysis. This combination is unique. There are universities that are strong on theoretic speculation but weak on testing it and others that are good at empirical analysis but weak on why the analysis is worth doing. One can see the effects of Wisconsin's way of doing research all over the world. Wisconsin-trained sociologists are noted for their unusually carefully tested insights regarding important matters that puzzle everyone. I have seen this over and over again.

1954-1956. I finished my doctorate in 1954 and stayed on with Bill Sewell for the next two years. It was during those years that we published the child personality pieces. We also published (with Murray Straus) an analysis of the separate and combined effects of parental socioeconomic status and the individual's IQ on the educational and occupational aspiration levels of Wisconsin youths. This work, too, was widely cited by others. Also during those years I traced the 1955 locations and occupational and educational attainments of the 1006 former students who had been in Sewell's Jefferson County, Wisconsin sample of the county's high school juniors and seniors of 1948. At the time we didn't publish anything from this work, but with Sewell's OK, Bill Miller and I published the research in our 1963 [also 1971] book, called *The Occupational Aspiration Scale: Theory, Structure and Correlates* (OAS).In the late 1950s I was teaching at Michigan State University (MSU).The book itself was submitted for publication in 1959. But it was held up by the MSU's prospective Ag College publishers for unknown reasons—maybe because they didn't like it. However, from letters we received, the ideas in it must have percolated into common use. At least as important the book shows the elements of what later came to be called Status Attainment Processes (SAP) along with the data supporting each element. The evidence consisted of correlation coefficients. With the wisdom and knowledge of hindsight, the variables should have been linked in causal sequence by means of path coefficients. But such techniques were not known to sociologists back then. The better-known SAP research was published in 1969 and 1970. The OAS book remains, I believe, as an important step in the development of status attainment theory.

Around 1955 João Gonçalves de Souza, then the head of the Organization of American States technical assistance program and a former student of Wisconsin's Department of Rural Sociology's (DRS), John Harrison Kolb, came to Madison to try to talk me into going to Brazil and working on research under the administration of a Dr. Cottam (another DRS ex-student) who headed the immense USAID program in Brazil. I couldn't do it then because I was already committed to Sewell. But several years later (1962) it all came to pass.

The two status and childhood personality studies by Sewell and I greatly influenced the thinking of sociologists. For example, seven years later another sociologist began his article on the subject by saying that anything new on it "requires special justification". After that, sociological researchers moved on to other things. For the most part, so did we, as indicated in the previous paragraph.

More needs to be said about the roots of my eventual work in Brazil. The professor who had founded the Department of Rural Sociology (where I worked under Sewell) was a man named John Kolb. During the late 1940s Kolb had several outstanding graduate students working with him. Cottam was one of them. Another was the Brazilian, Joao G. de Souza. With the support of Souza, Kolb had spent a year teaching and conducting research in rural Rio de Janeiro. Cottam became an official with the US Department of State. By the mid-1950s he was posted to Rio de Janeiro as the head of the USAID program. As I was finishing my doctorate, Cottam, Souza and Kolb all wanted me to go to Brazil and do some research and teaching. In 1954, Souza even flew to Madison to try to talk me into doing so. However, I had already promised Bill Sewell that I would work with him if his application for funds was approved. It was. And I had to turn the Brazilianists down, at least for the moment.

In mid 1956 Bill went to India for a year and I started my professorial career as an Associate Professor of Sociology and Anthropology, then Professor of Sociology, at Michigan State University (MSU).

1955-1965. Elizabeth came to us in 1955, followed by Stephanie in 1958. Bill was born in 1964.

5. Michigan State University and the Beginnings of Work on Brazil.

I served on the MSU faculty of sociology during from 1956 to 1965. I was also a member of the Department of Communication. In 1959 MSU's president, John Hannah, asked me to serve in the National Defense Executive Reserve, Office of the President of the United States. (Also see page 6.) Why? I don't know. Was it because I had had security clearance in the Navy? Hannah was an insider in military affairs and might have looked into this. Or maybe it was something else. Anyway, I told him there were better people for this. But he insisted.

The Year 1959 was at the height of the Cold War. As a member of the Executive Reserve I learned a few things about defense planning from meetings with generals in Washington and from trips to the Battle Creek, Michigan headquarters of the 5th Military District of the United States. If war broke out it was expected that bombers would come from over the Arctic directly over Michigan, headed for Detroit's powerful manufacturing system and Gary, Indiana's steel mills as well as Chicago. The Battle Creek officers talked of the 'Michigan Bowling Alley'. The point was that the pilots of Soviet planes flying southward from across Ontario at 30,000 feet could see the whole of Michigan's lower peninsula and a bit of Ohio and Wisconsin. The Battle Creek officers told me that in event of an attack my job would be to rebuild the infrastructure of Detroit, Toledo and Chicago. To do this, I was to be made a colonel (temporary rank) in the Army and kept awake for a full week following the bombing. Of course, I wouldn't have survived as a rational human being, if at all. Fortunately for all of us none of it ever happened.

For my research at MSU, in 1957 I had the 17-year-old boys of the schools in Lenawee County, a farming area near Detroit and Toledo, fill out a long questionnaire with items on educational and occupational status aspirations and related matters. The plan was to obtain even better data on the youths' status aspiration variables than Sewell's group had collected in the first (1948) wave of the Jefferson County, Wisconsin project. The Lenawee County data were to serve as Wave 1 of a longitudinal analysis in which the same boys would be re- interviewed a decade later. Girls were excluded. In those days, women were still expected to spend their lives as housewives, a job whose occupational status could not be measured. So there seemed to be no point in including girls. The questionnaires included instruments especially prepared to measure all the variables dictated by our understanding of what later came to be called 'Status Attainment Process Research'. Special care was taken to determine the reliability and validity of the instruments. Some fifteen years later, a team I formed at Wisconsin contacted 82 percent of the original sample members. The information gathered in this project led to a monograph that laid out what came to be the theory underlying the status attainment process, which itself was first published by Sewell, Alex Portes and I in 1969.

The Lenawee data were better than those of the Wisconsin studies of Status Attainment, except that the Wisconsin data covered the whole state, but the Lenawee data were just for a county. The latter research and questionnaires were designed specifically to measure the status attainment variables. The validity and reliability of the Lenawee data were carefully tested. The Wisconsin data were not measured directly. They were quite good, but there was no way of knowing their exact validity and reliability.

So how did the results of the two compare as tests of Status Attainment theory? It appears that Lenawee results not only confirmed those of Wisconsin but actually showed that the theory was better than it appeared to be in the Wisconsin study.

At MSU I was deeply involved in the new academic field of communication research, working with a small but fine set of researchers trained in several different fields—rhetoric, journalism, and social psychology. Though strongly committed to sociology, I am convinced that there is much to gain by absorbing the ways those of other fields think. The chance to do this was one reason why I teamed up with this group. The other is that they were determined to bring social science know-how to the media, including journalism, radio, TV and rhetoric. They thought social psychology should provide the theory for communication analyses. So did I.

One of my departmental colleagues, an outstanding sociologist, was displeased that in my work with professors of other fields I was giving away secrets of sociology by showing how their work might be improved by using sociological methods. Clearly, he was thinking like an auto union worker from nearby Detroit: ’Never tell your enemy what you’re really thinking’. ‘Keep your trade secrets to yourself.’ I thought this was short sighted in our field. So we gave away our field’s special knowledge and other disciplines started to use it. But knowledge is a flowing stream. By letting others have what we once monopolized we could go on to discover new knowledge. I firmly believe that basic science should be available to anyone. Of course, not everyone is prepared to understand it. So how does it flow to such people? If it’s important enough to catch the eye of media people, they’ll restate it in simpler terms and it will diffuse to a much wider audience.

Also at MSU there was an outstanding rural sociologist who became the University’s first dean of international programs, Glenn Taggart (PhD, Wisconsin, c1948). He obtained a large grant to infuse the university with greater international awareness. To facilitate this he set up a few small groups, each of which was to determine what scholars elsewhere were doing in this regard. One was on international communication. I was asked to serve as secretary to this group. We held weekly faculty-only seminars on this for half a year. So we designed the first of several Ford Foundation grants offered to each of several universities, including Wisconsin, that were to add strength to international activities.

One consequence of this was that I was invited to go to Costa Rica and do some work in cooperation with the faculty members of the International Institute of Agricultural Communication (IICA), a unit of the Organization of American States. I told the group about what Michigan State was doing in communication. This was of special interest to them, and later on they established a deep tie with the MSU people.

While there I met an impressive young man who had studied agricultural journalism at Wisconsin named Juan Diaz Bordenave, and encouraged him to go to MSU to study with the communication people. So he did. I became his Major Professor and we became lifelong friends. He was ran the Organization of American States’ Institute for

Agricultural Communication and was based in Brazil. From this post he made important contributions to the economic development of Brazil.

One of the participants in the meetings we held in Costa Rica was a young priest who had been a revolutionary warrior when forces for democracy won control of the nation. Among other things, he had been a graduate student in sociology at Columbia University in New York. In his off hours he was the president of the UN General Assembly. As he and I were both interested in social stratification, he led me on a one-day trip from the bottom of Costa Rica's status hierarchy to its very top.

Another consequence of the Costa Rica spell was that I was asked to join Larry Witt, an MSU agricultural economist, in a trip to Egypt, Tunisia, and Brazil for the purpose of designing a plan to evaluate the Food For Peace Program. Before we left the US, we had a meeting in Washington with Hubert Humphrey's sister, who was important politically quite apart from her distinguished brother. We spent about a week in Egypt, mostly meeting with the US ambassador, Lucian Battle. Still, we had time for some other things. For one, I got to visit the American University in Cairo and talk with the sociologists there. For another, an Egyptian former MSU student came up from a mid-country city to show me around Cairo. One of our visits was to a mosque, the only one I have ever entered. Still another was a trip Larry and I made to Luxor and the caverns on the west side of the Nile. The caves, carved as tombs of ancient Egypt's great, were fascinating. You could go deep into them, following their twists and turns, until there was no natural light from outside. Back in their depths there were wall paintings of the most vivid colors. And there was no evidence of soot on the ceilings or anywhere else. So how did the artists do their work? I guessed that they must have used a set of gold mirrors to send sunlight into where the painters were working. It seems that this is what Egyptologists of today believe.

After Egypt Larry and I spent a couple of days in Tunis, where the American ambassador showed us around.

From there we set out for Brazil, stopping for 12 or so hours in Madrid. That gave enough time to visit the Prado and to see many of its works of art, including work of El Greco.

Then we flew to Dakar, where we changed planes to Aerolineas Argentinas for the trip across the Atlantic. Two hundred miles from the coast of Brazil I saw a fleet of tiny fishing boats called *jangadas*. Pretty dangerous work. Anyway, Larry and I landed at Recife, where I was able to visit with people I had met earlier, in 1962, during our first stay in Brazil.

Heraldo Pessoa Soutomaior was another young sociologist I met had when he was a graduate student at MSU. We have remained friends ever since. Heraldo and Juan Diaz Bordenave in 1962-1963, had carried out the field work for Juan's PhD thesis in a *municipio* called Timbauba, in Pernambuco. As Juan's doctoral advisor, it seemed a good idea for me to go there and look it over. So the consulate lent me a jeep. It turned out to be another of many impoverished rural Northeastern towns. But of course it was a good idea to see it firsthand so as to better understand his doctoral research.

From Recife I went to Rio de Janeiro. At this point the ostensible purpose of the trip escapes me. Anyway, I was asked to pay a visit to a lady there in the city. I went. Her husband was the editor of *Seleciones*, the Portuguese version of *Readers Digest.* Frankly, he was a lush, and he stayed out of our conversation. She was the sister of several Marine officers, obviously very right wing. She told me a wild tale about the alleged stealing of a shipload of butter that was supposed to be given to needy people. I guess she thought our evaluation of the Food For Peace Program needed to know about it. Or more likely, she may have been afraid I was going to look into something undercover that she was involved in. The *Readers Digest* article on Brazil had something to do with bringing a US aircraft carrier to Rio and stimulating the US to encourage the Brazilian generals to crack down on the population.

I stayed in Rio for a couple more months to do some research planning with a group of Brazilian sociologists, including Heraldo Soutomaior, who were interested in setting up ways to carry out modern sociological quantitative field research. It should be remembered that this was early in the military dictatorship. Apropos of that, one of the participants in our group was mysteriously assassinated.

With all this going on it may be thought that I wasn't doing any teaching. But I was. For the most part, I taught a seminar on social psychology that had a large number of students, in addition to courses in political sociology, social attitudes, and rural sociology. One day three graduate students from different departments asked me to give them a seminar on stratification. I agreed, telling them they would have to work hard. One of them was a Canadian-Pakistani anthropologist, named Seguir Ahmed, as I recall. Another was a brilliant Costa Rican sociologist. (Sadly, he later committed suicide.) The third was an outstanding communication student from Brazil. She was fluent in seven languages. When she finished her work at MSU she and her husband went to Italy, where she taught sociology at the University of Rome (La Sapienza). I visited with her and her husband a couple of times when I happened to be in Rome in the late 1960s. Years later I saw the Pakistani in some sociology meetings in San Francisco. He told me he had spent some time in rural Pakistan for his doctoral research. He was appalled at the brutal domination of the people of the villages by the lawless land owners. He was teaching at Simon Frazer University in British Columbia. Later, we learned that he had 'committed suicide" by jumping off a bridge into a deep ravine. Obviously, I suspect he was murdered for talking 'too much'.

During those years and for several years afterward I found myself running back and forth to Washington.

Also, in 1959 I was offered a job at the University of Wisconsin but turned it down after an unsatisfactory discussion with the Dean of Agriculture. I was dedicated to international research and teaching. He wasn't much interested in this. Vice President Bob Clodius called me to his office to find out why I turned the offer down. I told him about the meeting with the dean. He wasn't pleased with the dean's take on international work.

Things were different by late 1961, when I was at Michigan State. I got in touch with João de Souza and told him that if he was still interested the family and I were ready to go to Brazil. I was due for a sabbatical leave. Besides, I had a plan for research that would be carried out there. I was given a Fulbright Professorship and a grant for research from the Organization of American States. Hazel and the girls and I spent 1962 at the (then) Rural University of Brazil (RUB). I learned Portuguese, taught sociology, and carried out the research. Before we left for Brazil de Sousa flew me into Washington to meet with a bunch of Brazilian generals. Why? Well, my guess is that he wanted to ease my way into the country; to make me credible to Brazilian authorities. João was the first of many "angels' who came to my rescue when things threatened to go bad. We kept in touch with each other until he died. Then I published an obituary about him in a rural sociology journal.

That same year, a Fulbright grant was arranged for Fritz Fliegel, Ruth, Freddie, and Ruthie to go to the University of Rio Grande do Sul, in the city of Porto Alegre. We were lucky that the Commission was amenable to a lot of travel. So we were given support to visit various places. One time the Fliegels came to Rio to see us. So we all took a hotel in Ipanema that faced the beach. Now there is an island—really just a big outcropping of basalt—just off the coast. Freddie, four years old, looked out the window and saw it. He yelled to all of us, "Look! There's Australia!" Not bad for his age. But far distant from the real thing.

Another time, Fritz and I received support to spend two weeks in Rio to sit in on a meeting on stratification in Brazil.

Still another time, our whole families were supported to visit Recife. We paid court to the 'Lord' of Apipuco, the famous Gilberto Freyre, and were shown around the museum he owned, later called the Fundação Joaquim Nabuco, after a famous Nordestino. I was asked to give a talk to some young women students who must have been invited to the Fundação for that purpose. So I tried. But my Portuguese was lousy. The girls walked out. Afterwards Fritz, who's Portuguese was much better than mine (having been tutored in Porto Alegre by a master of Portuguese, Mary Schil), asked me something like, "Do you think 'fisherman' and 'research' are the same word? Or what?"—Fisherman is *pescador*, research is *Pesquisa*. (That might not have been the only Portuguese goof I made there.)

The political situation in Brazil was pretty bad when we were there. The president had resigned. The vice president, Joao Goulart, had been in China. Most of the generals were not much interested in having him take over the presidency. His brother-in-law was governor of Rio Grande do Sul, on the border with Uruguay. So Goulart went from China to Uruguay so he could come into Brazil under the protection of his brother-in-law and the Third Army—the strongest in the nation because of the State's other border, with Argentina, and the fear Brazil had that the latter might attack the south of the country.

The generals of Brazil's five armies worked out a deal to let Goulart into the country. He was to become the president but all the power was to be in the hands of a 'premier', Brochada da Rocha. But Goulart wasn't through. He agitated to gain power as president. He and his brother-in-law provoked a food crisis in the capital, Rio de Janeiro, and Goulart announced that if the power of the president was given to him the crisis would be over. At this time a caravan of trucks loaded with rice from Rio Grande do Sul set out enroute to Rio. It disappeared. About the same time the newly appointed premier mysteriously died of a brain lesion. Was he assassinated? Who knows?

So Goulart became president. So what did he do? He set about making himself the biggest land owner in the country, and badly mismanaged the government. We didn't feel the effects of this because we returned to the States in early 1963. Another thing he did was to try to set the enlisted men, especially the sailors as I recall, against their officers. One can imagine what the reaction of the generals and admirals was. It must be remembered that this was the time when Castroism was spreading all over Latin America. Goulart pretended he was a communist, though in fact he was nothing more than a crook. In just a few years the generals and the American government decided that they'd had enough. So on April 1, 1964 the army threw Goulart out, with the blessing and help of the United States, who sent a Navy vessel to support the coup.

Actually, I put all this together by myself, just reading between the lines in the newspapers. Much later I was talking with a Brazilian ambassador and laid the story as I understood it to him; as a high official with Itamarati (Brazil's Department of State), he had access to parts of the nation's history that almost no one else knew anything about. He agreed with the interpretation.

This, however, says nothing about how our family lived during those tumultuous days. We were living at the Rural University, and were thus insulated from the worst of the events. Still, the food crisis hit us too. With access to a jeep, grudgingly provided by the university, we were able to go to the surrounding farms and buy what we needed.

We also got to visit with my old friend from MSU, Heraldo Soutomaior. He showed us around the area, with a special trip to Olinda.

Now back to the research in rural Rio de Janeiro. It harkened back to our earliest work on the process of status attainment in Jefferson County and from what I learned from Joao Goncalves, Department of Rural Sociology (DRS, University of Wisconsin), Professor John Kolb, and one of Kolb's Brazilian students, Edgard Vasconcelos de Barros (of the Federal University of Vicosa). Discussions with them made it clear that the Brazilian rural stratification system was changing quite rapidly. At the time sociologists were not even considering the possibility that such structures might change unless a revolution provoked it—even though Pitirim A. Sorokin had written in 1927 that such systems change all the time. If that was happening in Brazil, I reasoned that it must also be happening in the US. This suggested that the status attainments of young people who were beginning their careers would find themselves moving with a changing stratification structure. Obviously this implied that status attainment theory would have to be modified to take such changes into account. But no one knew how to think systematically about changes in the structure of a stratification system. So I decided to use the Brazil sojourn as a time to do stratification research in rural Rio and to think about what I was seeing. Kolb and Vasconcelos had done the field work in some counties of the State of Rio.

With the help of my RUB students and some others from the University of Vicosa, I followed up the earlier research Kolb and his group had done nine years earlier. Joao Bosco Gueddes Pinto, then a researcher at the Vicosa, Minas Gerais agricultural university, helped me both with my teaching—his English was pretty good—and was also especially helpful in the field work. With measures on the same variables, taken on respondents in the same four municipios in rural Rio de Janeiro, it was possible to measure the change that had taken place in the local rural stratification system.

During our stay in Brazil, the Fulbright Commission helped me make two trips to the Vicosa ag college. It was there that I first met Fernando Rocha. Fernando and Joao Bosco both went to Madison to study with Gene Wilkening in the Department of Rural Sociology and with Bill Sewell, by then in the Department of Sociology.

It took years for me to come to understand what I had seen and measured in Brazil. And I might never have understood if, in the late 1960s I hadn't run into a little book on stratification by Kaare Svalastoga. With the help of this book, it turned out that my key publication on it was my Presidential Address to the Rural Sociological Society in 1970. It was called "Changes in the Structure of Status Systems." Up to the year 2000 I had still been treating the theory pretty much as it was presented in 1970. It took a re-reading (after 50 years!) of Ibn Khaldun to jog me into rethinking the whole of stratification theory. (The resulting paper was published in *Population Review* in 2009.)

In 1964 the University of Wisconsin invited me to teach summer school. Bill Sewell turned some of his data over to me, and we began collaborating again. In 1965, I returned to Madison as Professor of Rural Sociology and of Sociology, two separate departments, one in the College of Agricultural and Life Sciences (CALS), the other in the College of Letters and Science. My main position was in the department of Rural Sociology (DRS).

It must have been 1963 when some of us from three or four universities were asked to write statements about what the field of rural sociology should be working on. The records I once had of this affair have long since disappeared, so I'm writing from memory. The group was headed by the famous economist, Theodore (Ted) Schultz, of the University of Chicago. He was known for introducing the concept of human capital into the vocabulary of economics. We were to present our thoughts at a meeting in Washington, sponsored by the National Science Foundation, I think. I was completely overloaded at the time and there was some confusion about just when the meeting was to take place. When I found out the location it was too late to sit down and write a paper for the session. So I grabbed a plane for Washington and wrote a short report. In a few words, it said that our field's job should be to reduce inequalities among rural people. It was hand written, very short, legible but not elegant. The other papers were long, well referenced, and very professional. Ted Schultz didn't like mine at all. It was too short and was in my handwriting. And I suspect that he disliked the emphasis on inequality and its reduction.

There was, however, an economist present whom I had known slightly when he was teaching at Madison and I was a graduate student in the Department of Rural Sociology. He was on Linden Johnson's economic advisory committee at the time. He seemed to like my paper and made an interesting comparison of it with what the economists were doing. He commented that while economists were trying to raise the income levels of people, sociologists like me were trying to reduce inequalities, an implication being that we might be working at cross-purpose. Maybe this difference between our two disciplines might have had something to do with Schulz's dislike of my paper. In any case, the papers—including mine—were published regardless of his opinion of it. Unfortunately I have lost any record of the publication.

By 1964 Helcio Saraiva, Jose Pastore, Fernando Rocha, and Joao Bosco Pinto were graduate students in the Department of Rural Sociology. All of these became lifelong friends. Fernando, incidentally, is a cousin of my wife, Cristina, and it was at a dinner in his house where the two of us first met.

Skipping ahead a bit, by the time these people were back in Brazil, it became apparent that our former graduate students were not in positions to use the deep understanding of sociological thought and research that they were uniquely trained for at Wisconsin. Pastore, for example, was running around from university to university in Sao Paulo teaching courses to undergraduates, with no time or funds to conduct the research he was capable of. I was deeply concerned about this. So I wrote to a vice president of the Ford Foundation I'd chanced to me at Vicosa in 1962, telling him about the quality of training Pastore and the others had received, and what was happening to them in Brazil. Specifically, I said that if the present situation continued for five years they would not only have been unable to keep up with advances in the field but would lose much of the expertise for which they had been trained. Shortly afterward the Ford Foundation office in Rio de Janeiro made three research grants available on a competitive basis. The amount was $20,000 each, perhaps the equivalent of ten times that today. Pastore won one of the grants, Helcio another, and Dave Hansen the third. Pastore was then able to join the University of Sao Paulo's Foundation Institute of Economic Research, and use it and the grant as a base for conducting Brazil's first national study of social mobility—the work that made him famous. (His book was originally published in Portuguese. Then I arranged to have it translated into English, after which it was published by the University of Wisconsin Press.) Dave used his funds to carry out his doctoral field work in Rio Grande do Sul. Helcio used his to develop a network of researchers at the University of Brasilia, where he was teaching.

As stated earlier, back in 1959 I had been offered a job at Wisconsin but turned it down because of the then-Dean of Agriculture's foolish downplaying of international research. He liked to get federal money the Ag College could spend on programs to beef up foreign universities' capacities—and there was a lot of it in those days of the Cold War and Castro's influence in Latin America. But that was all. I turned down the job offer. At this point the Vice President of the University asked me to visit him to discuss the reason for rejecting the offer. He was pretty mad at the Dean.

In 1964 I taught summer school at Madison. By then the University had changed a lot since my first offer from it a few years earlier. Most important, the new Dean of Agricultural and Life Science (CALS), Glen Pound, was a man I admired. Unlike the former dean, he knew what was going on in the world and how to engage it. Also, I ran a comparison of what Michigan State University had that would support my research with what Madison had. These were the results:

PORTUGUESE: MSU had a struggle to teach even a freshman course. UW had freshman courses on up to a PhD program. Besides, the UW had a real faculty in Portuguese, some of them quite distinguished.

COMPUTER FACILTIES: MSU had the most advanced computer of the day, and faculty members could use it with permission of the College of Engineering. UW had the same top line computer plus a whole series of supporting equipment, all open to faculty and students.

RESEARCH COLLEAGUES: At MSU no one understood what I was working on and some influential faculty members, especially my friend, Bill Form, doubted its worth. UW had Bill Sewell and others who understood very well what we were working on.

ADMINISTRATION: The Dean of Agriculture at MSU wanted to sidetrack my work to take over an operation he was interested in. UW: The new dean of CALS, Glen Pound, and I saw eye to eye about my work. He and the rest of the administration of the University were kind enough to let me do what I thought I ought to do, provided that my teaching responsibilities were fulfilled.

FOREIGN RESEARCH: MSU had a great dean of International Studies named Glenn Taggart, with lots of money. So did the UW—maybe even more than MSU. Also, I had strong support for my research on Brazil in both universities, and had the good luck to be well regarded by the top administrators of both.

LATIN AMERICAN STUDIES: MSU didn't have much. Wisconsin had one of the nation's strongest programs.

GRADUATE STUDENTS: MSU's were quite uneven. There were a few good ones, but most of them were second rate. Wisconsin had a national reputation for its especially rigorous graduate training program. So it attracted students who were seeking to be trained under such conditions. As a result, its graduate students were much better than those of Michigan State.

Most of the time I was with MSU it may have been the best place in the world for me. But that changed as time passed. UW had come to be better for what I wanted to do. Actually, Harvard gave me a feeler. But I squelched it. UW was—and is today—a much better place for the kind of work I do. So we moved back to Madison in 1965. Pastore, Fernando Rocha, Joao Bosco Pinto, and Helcio Saraiva were already there. Then, almost immediately Alex Portes, then 20 years old, came to my office and we began working together. Alex worked on one of my projects. Helcio was developing his project, too.

And I had a grant to work on the identification of youths' significant others SOs). Joe Woelfel and Ed Fink took over that project. Dave Hansen also asked to work with our people. But by then I had allocated all the money I had. So I couldn't put him on a budget. He said that would be OK anyway. He went to work with Helcio. It should be added that both the Woelfel/Fink and Helcio projects had funds for undergraduates. So we set up a system in which the more experienced people trained the beginners. This was one of the largest and best groups I ever put together. It was highly productive. All three projects produced solid results; some of them have been republished or are on the way to being republished, perhaps this year.

6. Collaboration with the Ministry of Labor.

In 1975 Joao Goncalves de Souza told me about the new Brazilian president Geisel's plan to slowly return the government back to a democracy. In addition, it was a good time to return to Brazil to work with Pastore. He was teaching at the University of Sao Paulo (USP). One day Geisel called him to Brasilia and asked him to be the Minister of Labor. Jose demurred because he would have had to give up his job at USP but said he would do the work on a consulting basis. I was already at USP. That began a period of several years of work by both of us and others chosen by Jose with the Ministry. The others, each at the government level just below the minister (Murillo Macedo) and Jose, were Fernando Rocha and Renato Simplicio Lopes, both former graduate students at the Wisconsin Department of Rural Sociology, and Sandra Valle, a lawyer who had worked in London for several years.

The Brazilian group had a lot of work to do. But I don't recall having much to do. Probably I was writing reports in English for various kinds of international collaboration. Naturally, I also spent a lot of each year in Madison. Once, Jose and Murillo asked me to look up some information on the National Labor Relations Board. I did, but added that in my opinion the best thing they could do for labor relations would be to get the Army out of disputes between the workers and management, and let the two work things out for themselves. This they did, and it probably saved a great many lives. So why was the Army involved anyway? I don't know, but the practice must have been at least several decades old—one of those things that takes on a life of its own.

7. Back to Madison.

Also in 1965, I was Vice President of the Rural Sociological Society (RSS) and chair of its Development Committee (RSS DC), the group that was supposed to recommend ways to improve the field. I set up several so-called 'task groups', each of which was chaired by a member of the RSS DC, plus other members I appointed to each. Each task force had its own responsibility. Luckily, there was still a lot of money for travel. So the committee members could fly into O'Hare Field for the meetings. I set the tasks of each committee. Two are worth remembering. Of these, one was charged with setting in motion efforts to get research funds for the 13 Black Ag Colleges. The other was to shift the center of gravity of rural sociological research from Chicago to Washington. Both of these were accomplished. It took five years for the Black College issue to come to fruition because it had to be approved by other groups in the RSS and especially by its Executive Committee. By coincidence I had become President of the RSS in 1970. Our Black College committee met with the Secretary of Agriculture in 1970. We presented our case to the Secretary. He allocated $2 million to the 13 Black Colleges for the first year, with substantially increasing amounts set for each year thereafter. Much later, the President of a Black College in Alabama gave me a paper weight in honor of our work. It now rests in the awards case at home.

The shift to Washington was easier. Largely because there was a lot more research money available there than in the Midwest.

So, as said, I arrived back at UW in the summer of 1965, where Pastore, Helcio, Fernando and Joao Bosco were already studying. And almost immediately, I had the incredible good fortune to meet a 20 year old Cuban refugee, a beginning graduate student of sociology: Alejandro (Alex) Portes, a genius (today Alex is perhaps the best known sociologist in the United States, if not the world).

At our first meeting Alex laid out his plan for his doctoral thesis, several years away. His idea was to go to Chile and study the people who lived in the shanty towns (cayampas) of Santiago. In those days, days of Fidel Castro’s widespread influence, it was widely believed that these people were tinder for a revolutionary firestorm. Alex believed that because Chile had a wide open political system at the time, it would be an excellent site for such a study—no interviewee would be challenged because of his political beliefs. In 1969, with funds from a Midwestern Universities program, he went to Chile and carried out his research plan. It turned out to be much more difficult than expected. At Madison, Spanish American sociology graduate students who envied Alex, led by a Chileano, mounted a hate campaign against him in the University of Chile, to which he was informally attached. He had taken care to line up all the leadership of all the nation’s political parties, right to left, including the Maoist and Stalinist ones.

The attack from within the university was unexpected. It was led by Hugo Zimmelman, a left wing sociologist who headed the department of sociology. Alex was paying students as interviewers for his research. Zimmelman arranged for the interviewers to accept their pay and then burn the data. This plan came to the attention of the dean of the college, Danilo Salcido, who was a friend of mine from our graduate student days when we had overlapped at Wisconsin’s Department of Rural Sociology. Danilo was a leftist himself, but unlike so many leftists he was also a serious researcher who maintained his objectivity—despite the opinion of so many Marxists of the day that objectivity was a fraud and so was sociological research. Danilo wrote to me about Alex and his situation. It was claimed by his detractors that he was a spy for the CIA. The absurd reasoning behind this, a copy of which was sent to me, was that Alex had money from the Midwestern Universities group (MWG), the MWG was funded by USAID, USAID was an agency of the Department of State, and the Department was controlled by the CIA. Danilo said he would follow my advice about Alex. I wrote him that I had known Alex and worked with him for five years, beginning when he was just 20 years old, and that I was sure the charges against him were mistaken at best and frauds at worst.

Danilo gave his support and the research was carried out to a successful end but not without attempts at interference. We found out about the plan to burn the interview questionnaires. So unbeknownst to the Zimmelman group, Alex microphotographed the documents and in that way was able to get all the data back to Madison. His doctoral thesis was based on them. In addition he published some journal articles from the project. These were so important that years afterward, in the 1990s, they were still being used by at least one Chilean sociologist.

At this point allow me to tell a little story about the continent's slums of the time. Every city in South America had them, and there was an anthropologist who wrote about the one in Lima, Peru. This researcher was thought to be the discoverer of these poor urban areas. But in fact she was scooped by the Brazilian novelist, Jorge Amado, who years earlier wrote a novel about those who dwelt in the slums of Salvador, and described the process by which the poor organized overnight takeovers of the land on which their houses—slums, of course—were erected. It's pretty clear that Amado had in fact seen this happen, so his novel described the reality itself. And clearly he had scooped the anthropologist's 'discovery'.

Before Alex left for Chile, in collaboration with Bill Sewell, Alex and I worked out what became perhaps the best known single publication in the literature of sociology, or so I have been told. Entitled "The Educational and Early Occupational Attainment Process", (*American Sociological Review* 34: 82-92), it has been reprinted quite a few times, the latest in 2007. The article presented an empirically tested theory of the process of status attainment—fulfilling the Jefferson County beginning that Sewell and I had run in the late 1950s and that Bill Miller and I published in 1963 and again in 1972: in our little book *The Occupational Aspiration Scale: Theory, Structure and Correlates* (1971: Cambridge, MA: Schenkman.). As late as 2007 it was still being republished by other scholars. It was published in 1969, and republished nearly 40 years after it first came out.

Reviewing the situation on our return to Madison in 1965, it will be recalled that I also had a grant for research on the measurement of the influence of significant others on youths' educational and occupational aspirations. Joe Woelfel and Ed Fink, both graduate students of sociology, joined that project. Joe headed it. The publications on status attainment and on significant others popularized the term Significant Other—which is now part of the standard American vocabulary. (It became popular in part because some leaders of the public education establishment picked it up and used it.)

And there was a third project: research on the stratification system of Brazil. Helcio Saraiva headed that one, and David Hansen was his right hand man.

To repeat, this 1965 group was one of the best I ever had—Alex, Joe, Ed, Dave, and Helcio, as well as the teams, Helcio and Dave put together. But to praise that group is not to imply that later graduate students and senior collaborators were weak. On the contrary, they were excellent. But never afterward did I have such a large and fine set of sociologists working with me all at the same time.

As far as UW graduate students were concerned, 1965 was a golden year, with repercussions that still ring down to today, over these many years. That was the year I returned to Madison from MSU and the three teams of graduate students previously mentioned were all immediately set to work, each team with its own project, and each project related to each other one. Alejandro (Alex) Portes headed the ‘Status Attainment’ Project, using Bill Sewell’s 1965—1970 longitudinal data. Helcio Saraiva headed what became known as the Acucena (Brazil) project, aimed at describing the stratification structure of a small and isolated area in Minas Gerais, and on which he eventually did his doctoral research. Joe Woelfel came over from the Sociology Department to work on the Significant Other (S0) project, in which we were trying to learn exactly who the individuals were who influenced a youth’s educational and occupational aspirations and how they did it. Joe and I ransacked the Sociology Department’s records on recently admitted grad students. We found the best of the lot—Ed Fink, and Ed joined Joe on the SO project. Dave Hansen came and asked me if he could work with us. But by the time he asked, I had no money left to pay him. So he said he would work with us anyway. I asked him to work with Helcio. Among all of these great people only Helcio is no longer with us. Each of them has had his own brilliant career.

Some may wonder why there were no women leading these groups. The answer is that the UW sociology female grad students preferred to work with women faculty members, as mentors I suppose…Yet there were a few exceptions. Suzanne Dvorak did her doctoral research with me, as did some on their MS work.

8. Brazil Again—Then Madison.

The Brazilian military took over the government on April 1st, 1964.

In 1965, partly at Helcio’s suggestion, I made a trip to Belo Horizonte to try to establish research relations between Wisconsin and the University of Minas Gerais (UMG). Everything seemed to be working out very well. The Department of Political Science was strong and empirically sophisticated. It was headed by a professor named Julio Barbosa, who was strongly supportive of the idea. The Dean of Social Sciences, the gentle Professor Pedro Bessa, was also, as was the Rector, Gilson Barson (??). I returned to Madison to firm up our university’s side of an agreement with UMG. Gilson started out for Madison to sign the proposed agreement. When he got to the Brazilian Embassy in Washington he was told to turn around and go back to Brazil. When he got there he was fired by the army. So were Pedro Bessa and Julio Barbosa.

This was the start of a long dark night for Brazil’s sociologists. Both of the most famous ones were stripped of their University of Sao Paulo professorships. One of them Fernando Henrique Cardoso went into exile. (Much later, he was elected president of the nation.) I’m not sure what happened to the other, though decades later he became a senator. Quite a few of the lesser, more ordinary, sociologists were allowed to keep their jobs, though at least two or three that I know about were assassinated.

A great many sociologists, perhaps most of them, hated the military government and were, or became, Marxists. In those years, Marxists the world over believed that objectivity was impossible and that objective quantitative research was a fraud. They also detested anything coming from the United States. Unfortunately for Brazilian sociology, they missed the rapid improvements that were coming on line in America—statistical methods like sophisticated regression analysis and path analysis. These advances were not merely methodological. They also changed sociological theory--and not only in the United States. These methods were picked up by many in Western Europe and even some in the USSR. Thus, in Brazil, the entire generation of sociologists who were then in training was left to fall far behind much of the rest of the world. The affects of this may be seen in that country down to today, though over the past decade quite a number of younger sociologists are now studying quantitative styles of analysis.

1966 to 1972. In late 1966 Helcio and I returned to Belo Horizonte to set up his field research. We had grant from the Agricultural Research Council for this purpose. Helcio's research design to study stratification was one of the best ever. Even today it is a model of what a good stratification project should be. We designed part of it in Madison and finished the design in the basement of the Faculty of Philosophy at UMG.
We picked the site, an isolated area of the county of Acucena 80 miles from Belo Horizonte, with great care, and visited it both to gain the good will of the locals and to assess it personally.

But things were not really peaceful in Belo Horizonte at the time. The general who was president of Brazil moved the government from city to city. (What a waste!) In Belo Horizonte, he ordered the soldiers to 'clean the streets', to arrest any and all. Helcio and I and the students worked day and night in the faculty (college) of philosophy. I made arrangement with the chair of the University's excellent department of political, science, Julio Barroso, to cooperate with the UW/DRS in research. This was approved by the dean of the college, Pedro Bessa, and the rector of the university, Gerson Barzon. After I returned to Madison Barzon got on route to Madison to confirm everything. He stopped in Washington to check with the Brazilian embassy. There he was ordered to go home. When he got there Julio, Pedro and Barzon were all fired from the university. I guess the dictators were afraid of sociology and political science.

Now a tale of arranging transportation must be told. A friend of mine at the Ministry of Agriculture, then in Rio, told me the Ministry owned a jeep that had been lent to the Secretary of Agriculture in Belo Horizonte and that he would transfer it to us. The secretary said that the jeep was out of order and if we wanted it we would have to fix it and give it back to him in good order when we were through with it. The repairs cost $300—or about $3,000 today. We did, and the project used it during all its work. Its tires were not good. Once one of them blew out in Acucena. We could repair it. But we'd have to find a way to fill it with air. But how? The nearest gas station was at least 40 miles away. We did what the locals did. We removed a spark plug from an old bus and ran a tube from its location to the tire, turned on the bus's motor and pumped exhaust from the open cylinder (were the spark plug had been removed) into the tire. I'm pretty sure what we pumped into the tire was pretty bad for it. Still, it worked. I guess Helcio must have redone it after I left.

Then there was the problem of money. Our grant was lodged with the University of Wisconsin's budget office. It required signed receipts for all our expenditures. But very few of the people who serviced us could write, and even those who could were not about to sign any papers for fear they'd get in trouble with the military. So we paid our bills, and kept our own record of the expenses. When I got back to the University's budget office, the officers there wanted evidence that we had spent the money properly. But of course we didn't have any evidence at all. Finally, they agreed to take my word that our reports were accurate. (Later I was told this would never happen again. But fortunately all this didn't get me in trouble.)

Helcio and the excellent university students who did the interviewing finished their work in early 1967 and he returned to Madison. (Many years later I was able to talk to some of these former students, who by then were around 40 or so.)

This research project on stratification is, from the point of view of both the quality of the theory that went into it and the care taken in the research procedures, one of the best ever carried out. In my opinion, the theory it embodied remains the most sophisticated ever conducted in its field. Indeed, Bill Haller and I have just published (2010, in *Population Review*) a paper based on Helcio's data.

In mid-1967 the University asked me and a couple of other faculty members to prepare a group of students to do a stint in Brazil in the following year. The whole group consisted of three professors and ten graduate students. As time passed the unspoken roles of the three professors diverged. One was a Brazilian professor of Portuguese. He became our spokesman for the language, especially later on, when we were actually in Brazil. Lloyd Bostian, from Agricultural Journalism, became the budget officer. I, who never liked to deal with budgets, became the unofficial leader of all thirteen people in our group.

As it happened, the other professors didn't really care where in Brazil we took the entire group. I, however, wanted to see something of the Northeast *sertao (*back country). In addition, I wanted the students to do empirical research and to have the experience of having the social ground under them and under the research shaken a bit, but not wounded, by actions of the military government. Besides that my old friend, Joao Gocalves de Sousa, had been the head of SUDENE (Superintendency for the Development of the Northeast). But just before we arrived he went to Brasilia as a Minister of State. So we settled on Pernambuco, where to carry out our research in the back country.

Most of the group flew directly to Recife, where my old friend, Heraldo Soutomaior was teaching at the University of Pernambuco. (UPE). A pair, however, decided to get off at Manaus and take an Amazon River boat to Belem before proceeding to Recife. One of the two, Christine, was in love with a man (Dr. Serrao), an official at the national agricultural research station in Belem. She had some competition. A local girl told her to stay clear of him or she would kill her. Christine is still alive as far as I know. Anyway Cristina and the other young UW woman, Jeanne Langley, from Detroit Lakes, Minnesota, arrived in Recife in time to join the rest of us.

We stayed in the city for about three weeks, to prepare ourselves for the field work. Heraldo Soutomaior and another sociologist at the UPE, Silvio Maranhao, helped us a lot. So did the sociology students.

One of the things we tried to do there in the city was to line up the goodwill of local officials. This would have been easy if Joao Goncalves de Sousa had still been there. Unfortunately the new Director was an enemy of de Sousa's, and he tried to take his anger out on us by threatening to expel us from Brazil. We survived that despite his threats. We also went to the head of the State Extension Service to gain his goodwill. This didn't prove any easier than with SUDENE. One of his assistants, who said he had a master's degree from Stanford, told the Extension director in our presence that he believed we were spies. Still, we weathered that too, probably because our local 'angels', Heraldo and Silvio, intervened.

Other important figures we paid court to included Gilberto Freyre and Dom Helder Camera, the courageous local bishop who had stood up for people threatened by the military government. Freyre was the "Lord" of Apipucos and the founder of the Fundacao Joaquim Nabuco, an institution that remains influential even today, in 2011.

This threat was very real. Our 1968 arrival was only four years after the military take over. The period was known locally as the Repressao—the Repression. Things were still pretty chancy when we were there. Only a little earlier, small time farmers, *compesinos*, were driven off the farms in droves. And there were lots of murders. People in this country may not understand that military dictatorships provide excuses for all kinds of otherwise illegal activities to be inflicted on practically anyone by the so-called Authorities. And they do. A given Authority's enemies may be found dead. Those whom the government fears may be tortured or killed.

Back to Recife. It seemed a good idea for our students to get acquainted with the sociology students at the university; at first the latter held them at arm's length. But after while this passed, and the two groups became friends. One of the sociology professors joined with Heraldo to help us get settled in. His name was Silvio Marcello Albuquerque de Cavalcanti Maranhao. (Much later he became one of my graduate advisees at Madison.)

One day the local students told our group that there was to be a demonstration against the government the next day and that they were going to be part of it. At our evening meeting, I reminded our people that the government was harsh and that we were guests, not citizens—as if citizens had any rights under the arbitrary rule of the generals. And certainly our group had no rights of any sort. If one of us got in trouble he or she might just disappear. Then I said that they were adults and I couldn't tell them what they could and could not do, but my advice was to stay clear of it.

They did, luckily. At the affair, the military police were protected with plexiglass shields. The students and others who were with them had no protection. But it should not be thought that they were just lambs and would let the police do whatever they wished. Indeed many of the demonstrators had metal Y-shaped slingshots that they used to try to kill policemen. The sling shots fired ball bearings.

We stayed in Recife for about three weeks, not all of which were devoted to work. Among other things we played on the beach and visited the sights of the city. One day I was walking down the street with my sandals, my boina on my head and a light khaki jacket on. Suddenly a tiny woman dropped to her knees in front of me and did the sign of the cross. I guess she thought I was Che Guevara or someone like him. (Che hadn't yet been killed by the CIA. That happened later, in Bolivia….Che, whose name was known all over the world, was Fidel Castro's charismatic right arm in the early days of the Cuban revolution.)

At the same time that we were there in Recife a UW Milwaukee (UWM) group had a contract in a neighboring state. The newspapers said there was some sort of trouble between them and the government. There were people who thought we, UW Madison people, were connected with them, which is understandable even though it was not true. After all, UWM and the UW Madison are part of the same Wisconsin university system. I guess our local angels disabused them, so we came out of that unscathed. For reasons I don't remember, I became convinced that the UWM people would be expelled. So at our group's evening meeting I told them the newspapers next day would show that indeed they had been expelled. And so it happened. From what our group said, I guess they thought this prediction was magic.

Heraldo told us the traditional home of his family was in a county 50 miles or so west of Recife, called Bezerros, which was maybe 25 miles east of the more distant city of Caruaru, the so-called "Capital of the Agreste" (the agreste is the hilly area between the humid coast and the dry sertao). Eventually we hired a rather large open 'super-jeep' and headed out to Bezerros to conduct the research. We rented the glassed-in front area of a restaurant that faced the highway. This was so we could use the large tables for preparing the interview schedule and mapping the area.

We were soon visited by *jaguncas*, armed henchmen of the local "coronel", the extralegal chefe (chief) of the area. They pumped us pretty hard. I guess they wanted to know if we were going to interfere with whatever rackets they were involved in. We convinced them that we weren't at all interested in what they were up to. So they went their way and we went ours…Or did they keep an eye on us all time we were working there?

We also rented some small rooms behind the restaurant as sleeping quarters. They were pretty bad. That mistake lasted just one night. The girls rebelled. So we moved operations to Caruaru and set up housing and shop in a small hotel.

Right after we arrived we were visited by a young woman from the area who told us her Protestant pastor had been murdered at the altar. She wanted us to do something about it, thinking we could exert some influence. We couldn't, of course. Of course, we could tell someone like Dom Helder Camara or Gilberto Freyre. But they couldn't do anything about it either.

There were no important interferences with the interviewing, probably because of the cover Heraldo's family afforded us. There were, however, a couple of things of special interest. For example, one of our interviewers met with a 'pistol packin' mama', a woman who, like the 'coronel', had a lot of clout, clout she maintained at the barrel of her gun. Another took place where another of our interviewer's had received an invitation to dinner with a *fazendeiro* family. Now these people are anything but sophisticated. To be specific, no matter how rich they may be, such *Norestino fazendeiros* (Northeaster Brazilian big farmers) are nearly illiterate, boorish, and crude. Our UW student interviewer made the mistake of complimenting the wife on her cooking. But instead of being pleased, she was pretty miffed. She let him know that *she* didn't do the cooking. It was one of her servants who did it.

As the days went by it became clear that Don Holsinger was the informal leader of our group of students. He was fluent in Portuguese and had lived in Brazil as a child for many years. He was also tall and had flaming red hair. So he stood out in any group.

As our time in Brazil was coming to a close, we three faculty members were invited to fly to Belo Horizonte to visit with the demographers at the University of Minas Gerais. With the cohesion and responsibility demonstrated by the students, and with Don as their leader, there was no reason not to go. So we did. The meetings went well and lasted only a day or two. From there I went to Rio de Janeiro to board a plane to Holland, there to participate in a world congress of rural sociology. (I don't recall what the other two Wisconsin professors did. Lloyd had friends in Porto Alegre, and the Brazilian had grown up in Goiais. Maybe they went to those places. Or maybe they returned to Recife.)

To my surprise I was detained at the airport. Although I had been the first one in the line at the ticket counter, I was shunted aside and left to stand there for two hours. Finally, an army colonel came out and escorted me back into the recesses of the airport. He wanted to know what I was doing there in Recife and why I was taking a trip to Europe. Naturally, I told him the whole story, that the meetings were of an international rural sociology group. I didn't see any reason to hide anything. Still, maybe the word 'sociology' ticked him off. As said before, the authorities thought sociologists were Marxists.

So finally I was put on the KLM plane, destined for Amsterdam. My ticket was economy class. So imagine my surprise when the Dutch-Brazilian gate agent, the same one who had refused to take my ticket at the gate, told me, grandly, that because I had been treated so badly by KLM my ticket had been changed to first class. Nice. But there was a hitch. My seat mate was another army officer. It doesn't take any special imagination to figure out why I was moved up to first class. I was grilled again for hours.

The meetings were held in a Dutch university in town. The only thing worth remembering about it was a young and fiery rural sociologist from the Warsaw Academy of Science, Boguslaw Galeski. He later became an important figure in the University of Wisconsin's Department of Rural Sociology.

Back in 1965 I was both Vice President of the Rural Sociological Society (RSS) and chair of its Development Committee (DC). As chair, I appointed a series of task groups. One of them was to start a process by which the RSS would become a participant in the National Science Foundation (NSF), weakening the Chicago-based Farm Foundation's hold on the Society and replacing it with the Washington-based NSF and the many connections that that opened up.

Another process I set in motion was an effort to provide research funding from the US Department of Agriculture to the 13 Black land grant universities and Tuskegee (also Black). Another task group was set up to carry out that job. It took five years for the proposal prepared by the task group to wend its way through the RSS' decision making system. But in 1970, when I was President of the RSS, a group of us had a meeting with the Secretary of Agriculture to present the RSS's proposal of research funds for the 13 and Tuskegee. For Year 1, he authorized $2 million, with the amount to grow each year after that, as the Black schools developed the capacity to use the monies. *I see this as one of the most important things I ever did.*

Returning to Galeski, I spent some time with him in meetings in Turin, Poland in 1974, and in 1982 he was one of the leaders in the Polish uprising called Solidarnosc ('solidarnish') (One of the little treasures I've carried around all these years is a lapel pin of that movement.) It must be added that the Polish sociologists were the only intellectually honest ones from East Germany through the Soviet Union. I don't know how they did it, because in those years Polish intellectuals were under heavy pressure from the USSR—as was the country as a whole. Though they were mostly kept out of sight, Russian soldiers were everywhere in Poland in those years. In fact, just before I left the country a friend and I went to a bar in Warsaw. Present were two Russian soldiers, arrogantly lording it over the patrons.

Now back to late 1960s. The return to Recife was uneventful, except that on the return trip we KLM passengers were given gifts for crossing the equator. They were four very nice copper etchings, now long disappeared.

When the interviewing was finished the group collected all the interviews and boxed them to be sent to Madison. However, the customs people refused to let them out of the country on the grounds that they might be full of subversive material. Now one of the peculiarities of Brazil in the times of the military dictatorship was a phenomenon I once labeled as ‘In elites and Out elites’. For example, Fernando Henrique Cardoso (much later president of the country) was so feared by the government that he, who had been one of the nation’s most famous professors, had been put in a kind of internal exile. Still, he was from a nationally important family. He was allowed to run a research group, provided it did work for the government. (When I saw him there in his office in Sao Paulo, his people were helping plan a big development project in the Amazon region.) He told me that the night before I came to see him one of his people had been picked up by government agents and tortured. “But I got him released right away by calling my uncle in Brasilia, who is a general.” His uncle was an “In elite”, he himself was an “Out elite.”

It was that phenomenon that got our data released. Silvio Maranhao called his uncle, who was an army colonel stationed in Recife. The uncle went to the customs people and released the data.

We all went back to Madison. The sociology graduate students of the group wrote their master’s theses using the data we all collected. Later, Helcio Saraiva, Don Holsinger and I published a mildly influential journal article comparing the occupational hierarchy data from our Recife group’s research with similar data from rural Minas Gerais that Helcio had gathered for his doctoral thesis.

In 1970 I attended some meetings of the International Sociological Association’ Research Committee on Stratification (RC28) in Paris. Some of the people there were from Poland. First, we had a large scale meeting in which papers were presented. After this, the Poles called some of us to another room for some more discussion. To the surprise of the rest of us, the Poles let their hair down and had a debate between those who supported the USSR’s rule of Poland and those who were against it and who looked forward to the day when the nation would become free of Soviet control. Most interestingly, the two sides seemed to trust each other (and ourselves?) to keep their mouths shut and not betray anyone. And of course, a few years later Poland did indeed distance itself from the USSR.

As noted earlier, I was elected president of the Rural Sociological Society in 1970. For my presidential address I presented the theory of the social hierarchies sociologists then called ‘status’ and now seen as ‘stratification’. It built on two things—my experience working on stratification in Brazil and the writings of other sociologists, especially Kaare Svalastoga (1965) and Pitirim A. Sorokin (1927). Many years later I learned about Ibn Khaldun’s 1377 theory of stratification and saw how it began the evolution of the theory through various thinkers, on up to 1970. Published in 2009, it filled out the history of stratification theory. Despite learning about the early history of the theory, it remains today, 2010, just as it was in 1970, with one small but important exception: the content dimensions describe power differentials.

Military rule remained harsh during the late 1960s and early 1970s. I made a decision not to go to Brazil unless my former students and friends said it would be OK. So I stayed away most of the time. There were two exceptions—again a long story. The first exception was in 1970 when Jose Pastore asked me to join him in a research project he was running at USP (the University of Sao Paulo). I agreed to join him in this because it looked like it could help stratification researchers understand a bit more about how money and power worked. We were pretty good at occupational status and education, but we knew little about how to think systematically about both money and power.

Regarding the project, Pastore had put together a data set on the 22,000 specialized employees of all of Sao Paulo's manufacturing companies. Altogether they were about six percent of the total workforce of the firms. Job titles were available for each of the specialized workers. This made it possible to score each one of them according to the level of authority he commanded within a given company's hierarchy of authority. Other data made it possible to determine the amount of education and the job experience—total work experience in years, years with the firm and years on the particular job. The data were used to form a causal model of the direct and indirect effects of each of the experience variables on the individual's education and their indirect effects on in-plant authority, as well as the direct effect of the person's education on his authority level. These results were seen to be useful to the influential economists of the University of Sao Paulo, then the most influential academic group in Brazil. Personally, I didn't care about their possible practical value. My interest in them had to do with their relevance for the theory of stratification. Up to then sociologists working on stratification saw what we called 'status' as two content dimensions, education and occupational prestige. But I had begun to think that our views of the content dimensions of stratification were too limited. As I had hoped they might, the results indeed turned out to help stratification theorists think about money and power. Taken together, this project and Helcio Saraiva's research provided the evidence needed to demonstrate the real-world existence of all four of the content dimensions that describe any society's stratification system. Helcio's data provided evidence for the four dimensions from the *compesinos* of a small and isolated rural area. The Pastore data provided similar supporting evidence from sophisticated upper stratum of a huge metropolitan region.

The other exception to the decision to stay away from Brazil had to do with the organization of a new academic organization. This decision was to attend a meeting three of us had worked out to organize a Latin American Rural Sociological Association. I got the National Science Foundation to support it. It was held in 1970, as I recall. Pastore was elected chair of the meeting. Dave Hansen was there, as was one Dutch brazilianist and various sociologists from Chile, Mexico and elsewhere in South America. This meeting was not a haphazard event. Its roots lay in discussions we held in Rome in 1969.

A sociologist named Alvaro Chaparro was working at FA0 (the United Nations' Food and Agricultural Organization). I was in Rome to give a paper and decided to drop in on him. He told me that all fields of agricultural science were represented at FAO except rural sociology. He believed that, world wide, rural sociology suffered because of that. He said that in order to be recognized by FAO a field had to show that there were at least three world organizations of it practitioners. Rural Sociology only had two: The Rural Sociological Society in the United States, with members from all over the world, and the European Association for Rural Sociology. So a third such academic association would have to be formed. Alvaro, from Rome and FAO, Pastore from Latin America, and

I from America, with funds from the National Science Foundation, were able to set up the above meeting and thus to get a third world rural sociological association put together. Called the Latin American Rural Sociological Association—in Spanish, ALASRU: Associacion Latinamericana de Sociologia Rural. So now we had the three associations required for representation within FAO. Indeed rural sociologists benefited from it, but not the Americans. The Europeans came to dominate its presence in FAO—overwhelmingly, with a little input from the Latinos. Nevertheless, all three collaborated to establish a viable and influential worldwide group, the International Rural Sociological Association. This organization is still effective.

From 1970 up until 1998 I taught at Wisconsin while making frequent trips to Brazil to continue using that nation's experience to try to understand what structures of stratification are and how they work. Seven of these trips were supported by Fulbright grants—an unheard of number of them.

So why did the government provide even two, much less seven such fellowships? And why did the University put up with my absences? True, the Dean of the College of Agricultural and Life Sciences, Glen Pound, told me not to spend more than a semester and a summer away during any given year. But I still don't really know, though I have ideas about it. There might have been three reasons.

First, I had good relations with Brazilians, and indeed I had very little contact with Americans and almost none with any other foreigners, including Australians and Canadians. The second may have been an unintended consequence of my research program. Professionally, I was interested in what I could learn from the nation's experience so as to better understand what systems of stratification are and how they work. (By now, 2010, I think I know, and have published the results.)

So I tried my best to get to know Brazil from top to bottom, in both senses—geographically and sociologically. And with the help and encouragement of Brazilian friends, I was able to do this. So I learned a lot about Brazil that no one else in America knew about the nation. Even some educated Brazilians would say I knew more about their country than they knew. And at the end of each Fulbright I had to write a report. In it, I would state everything I had learned about what I had observed. For example, in one early report, I said that Brazil was going to experience a huge shift of population, from rural areas into the burgeoning urban centers like Sao Paulo. This would happen, I held, partly because rural life was so hard and because the rise of industry in the cities, such as the automobile factories in Sao Paulo, would require large numbers of workers, and would generate huge spin-offs. And of course, it happened.

But note: in fact I would learn about some things that were official secrets. I wasn't trying to do this. Sometimes a colleague would tell me, as when one of them told me about the secret agreement with Germany in which Brazil would provide uranium and Germany would provide atomic technology. But I had a personal rule that I wouldn't tell tales from one country to the other. Each nation had its own official personnel whose job it was to do that. I wasn't one of them. Yes, I was occasionally called a CIA agent but that was pure baloney. I was nobody's agent.

Pastore, for example, became the chief of research and planning for the Ministry of Labor, as Minister Murillo Macedo's right hand man. So I came to know Murillo and his family pretty well. One day he and his wife went to Rome to be blessed by the Pope. I felt there was more to the trip than met the eye. Now it happened that the military government was run by generals who genuinely working toward re-democratizing the nation. But there were others, called the *Linha Dura*, hard liners, who wanted to return to the oppressive system of the late 1960s—the years I tended to avoid Brazil. At the same time the Catholic priests in Sao Paulo were promoting labor/management conflict. I figured out that Murillo was sent to tell the Pope to 'call off the dogs' so as to keep the pro-democracy effort alive. The point was that if the priests continued agitating, the *Linha Dura* would have an excuse to take over the government. So the Pope came to Sao Paulo and shut the priests up. And the pro-democracy effort continued. It took several more years for it to the change-over to take place. But finally, it did. And for a few years before it actually occurred, the movement came to be more and more public, and was given the name of the *abertura*, the "opening". And indeed I figured out exactly who was the planner of the opening: General Golberry, from his office in the presidency. This was one of several things that I had to keep for myself and not talk about until years later, when the issue was dead.

A third reason why our people let me do what I wanted to may have been that I came to know a lot of important Brazilians. I must have given a line or so in my reports saying that I had met with so and so. For one example, I came to know a man who was a judge in the Supreme Labor Court. This is just one of many. Another one of substantial importance was Jose Israel Vargas. At one time Vargas gave a couple of lectures at Madison, one of them over the Public Radio Station. Still another was Murillo Macedo, who was then the Minister of Labor. He too lectured at the University of Wisconsin, to a packed audience in the auditorium of the State Historical Society building.

Before leaving this topic, a word should be said about Vargas. Helcio Saraiva had worked for him and told me I should ask him to come to Madison. We became friends in a most curious way. I was driving him around the Madison area and mentioned that I had just read a fascinating book by a Brazilian writer: *Grande Sertao Veredas*, by Joao Gumaraes Rosa. Vargas said, "Oh, he's my cousin." Vargas and I became close friends and we remain so today. He is one of the world's shakers and movers. For many years he was the Chairman of UNESCO's Executive Committee, in Paris. (We saw each other several times in that city, once when Cristina and I were there. And another time he had me meet with the Director General of the organization. That meeting resulted in my spending three months there doing some consulting.) Varga did his undergraduate work in chemistry at the Federal University of Minas Gerais in Belo Horizonte. Then he did a doctorate in nuclear chemistry at Cambridge, Rutherford's research center. At the time the generals in Brazil didn't want him around. So he went to France, where he became the scientific advisor to the French Atomic Energy organization, while running a set of nuclear research projects in another part of the country. Later on, as the generals were slowly removing themselves from the government, he was asked to come back. At first he set up and ran a research station called the Joao Pinheiro Foundation. The he moved through a series of positions eventually becoming the Minister of Science and Technology.

Another possible reason, related to the first, is that I came to know a lot about how the nation worked. I don't mean its day-to-day politics, matters in which I had no professional interest. I mean the deep changes it was going through. So I could make some reasonable guesses about important social and economic changes that seemed likely to occur.

Now on to other matters. Maybe this is the point at which something might be said about teaching. During all my years at Wisconsin (and earlier at Michigan State) I taught courses and doctoral seminars in stratification, social psychology, and rural sociology. At the same time I was directing the research of my doctoral advisees. All told, I directed the research of 28 doctoral students at Madison and ten at MSU, as well as the research of forty or so master's students. I loved teaching—and learning—from these smart people.

Still, we were quite busy in research at Wisconsin in the late 1960s and on up to 1981. Joe Woelfel and I published at least three papers on the concept and measurement of the influences of 'significant others' (SO) on the aspirations of high school students. I presented one of them at some international meetings in Rome and it was published in an Italian journal. Saraiva and I published a couple of papers on stratification in Brazil. Dave Hansen and I published a couple on the aspirations of Costa Rican youth. I published several papers on the theory of aspirations, some with Lu Otto and others.

As said earlier, in 1969 and 1970, Bill Sewell and I published two articles, one with Alex Portes and one with George Ohlendorf, that seem to have defined all subsequent research on status attainment processes. The two were named *Citation Classics*, and the one with Alex has been republished time and time again, most recently in two different books in 2007. It should be added that in 1973 Alex and I also published an article that introduced the term, 'Status Attainment Processes." It was pure theory, no data. Just at the end of 2010, someone asked our permission to reprint it in a book of readings.

1972 was an important year for our former graduate students in Brazil. That was the year that Embrapa, the federal agricultural research agency, was authorized by the military government. It should be understood that the generals were dedicated to the advancement of the nation; they took seriously the slogan on the nation's flag: Order and Progress. The generals would provide Order (at whatever cost) and would promote Progress. Evidently they had figured out that Brazil had great prospects as an agricultural products exporter to the world. But the federal agricultural research and extension system was run state by state and was generally weak. So the generals decided to centralize it in Brasilia and to strengthen it everywhere. A small group of researchers, about a dozen, were brought together at the capital to plan the new federal organization of agriculture. Interestingly, the group consisted of sociologists and economists, not agricultural production people. Even more interesting, the group included several of my former students. Those from Wisconsin were Jose Pastore, Renato Simplicio Lopes and Fernando Rocha. Juan Diaz Bordenave (PhD Michigan State University) was another of my students who was on the commission. Juan was there as a representative of the IICA (*Instituto Internacional de Communicacao Agricola.*, an agency of the Inter-American Organization of States. Others included Erly Brandao and Eliseu Alves (PhD Purdue), both of whom were professors at the University of Vicosa. Together they designed Embrapa, the *Empresa Brasileira de Pesquisas Agropecuarios* (the Brazilian Firm for Agricultural Research.) Embrapa transformed Brazilian agriculture; first, by getting rid of the weakest research stations of the various states. Second, by arranging for a large number of bright young people to get their PhDs from the best agricultural colleges in the United States, especially Cornell, Wisconsin and UC Davis. Third, by setting up research stations in areas which were propitious for a particular product, given the rainfall, temperature and soil.

Going back to 1970. That was the year I was president of the Rural Sociological Society. Its meetings were held in Washington DC at the end of the summer. While the meetings were going on someone bombed the door of the Portuguese Embassy building. While there was no proof, I thought it was quite possible that it was one of the Wisconsin professors of rural sociology, Gene Havens. Sometime earlier Gene had told me that he had advised the so-called “crazies’ among the Wisconsin students to bomb the US Army Mathematics Research Center in Sterling Hall on the campus. And indeed in 1971 Sterling Hall was bombed around three o’clock just a day or two before the Washington meetings. Our house was six miles away, but I was awakened by the blast. I thought it must have been *thunder*. At six, I got a frantic call from a neighbor saying, “They bombed Sterling Hall.” (This incident is the subject of a book by Dave Mariness. It’s called *Rads*, I think.) For years I never told anyone about my suspicions, partly because there was no proof about the Washington bombing and also because Havens was not directly involved in the bombing of Sterling Hall. I should be added that a researcher who was working in the astronomy department in the same building was killed in the blast.

In that year there was a lot of student unrest all over the nation, especially at the University of California at Berkeley and Columbia University. The morning after the bombing at Madison, a radical student spokeswoman went on the radio shouting in almost exactly these words: “Yeah! Someone was killed! TOO……BAD! This is WAR! People get KILLED in WARS!

Students at Madison, and from there all around the nation, were appalled at the death of the innocent researcher and at the callousness of the radical spokeswoman. The nationwide rebellion of students stopped cold. All over the country ordinary students concluded that the student rebels were as bad as the ‘establishment’ they were attacking.

Also from 1970 to 1972 I was pretty tied up with work at the University of Wisconsin. I was Chair of the Department of Rural Sociology during those years, and in 1971, President of the Rural Sociological Society. The first of these turned out to be a nightmare, in two ways. First, within three weeks of my chairing the Department the

State cut the budget of the University, with reflections down to the College and Department levels. I had arranged for three visiting professors to join us for up to a year. I was told to cancel their contracts unless I could prove that they were valid. And the department's head secretary, who hated me, had hid the evidence. But I found it and the three all came to work with us. Second, this was a time when the students were trying to take over the university. Sadly, most of the members of the Department's executive committee, led by Gene Wilkening, the senior member, were on the side of the students. A special meeting of all the faculty and students was called. I had figured out what the strategy was and said so. It was for the Department of Rural Sociology to set the precedent, to be followed by the much larger Department of Sociology. Then when Sociology collapsed, the other huge departments such as English, Mathematics and History would fall in line. And thus the students would control the University. I told the rural sociologists, students and faculty, that I knew what the game was and it was "stopping right here". Both the faculty and the students were furious with me. So they got rid of me as Chair and put the only incompetent member of the faculty in as chair. But the good part was that the student take-over died.

A few years earlier I had led a successful movement to bring democracy to the Department. A couple of years later I was put up as a candidate for the job. But the faculty and most of the students voted me down. Dean Pound asked me if I would agree to remain as chair anyway. I told him I wouldn't. This was because my agreeing to it would undermine the democratic system I had managed to establish in the Department.

That year, too—1970—Clifton Wharton, Jr. became president of Michigan State University (MSU) in East Lansing. He was the first black president of a major university in the history of the nation. And it was at least partly our doing, Bill Sewell's and mine. We had both been invited along with lots of others to make recommendations for the new president of MSU. Bill and I decided it was time for a black person to get such a job. So we pumped for him. We both knew him personally, so I guess we must have provided back up information to help out the case.

As a matter of fact, I once had lunch with Cliff's father in Rio de Janeiro. Cliff, Sr. told me he had been the first black ambassador in the history of the nation to come up through the ranks of the Department of State.

1973 to 1983. Over the summer of 1973, at the invitation of its faculty members, I taught at Brigham Young University in Provo, Utah. It was a bit strange, partly because I knew the secretary in the Department of Sociology was a not too distant cousin. But she was clearly afraid of me because I am what the Mormons call a gentile, and she had never met such a person in her life. While there, I took advantage of the weekends to see what I could of that part of the world. Sometimes I traveled long distances, other times I just looked around the city and its environs. I made some trips through cemeteries to get an idea of the local history. One of these was right in Provo. To my surprise, there in the back of the cemetery I saw an impressive marker to my great great grandmother, the first wife of Orrin *Porter* Rockwell. (This may require a bit of background: My maternal forbears were bound up with the earliest days of the Mormons.

Joseph Smith was the founder of the church. He said the idea was given to him by a heavenly messenger in a dream, and that he found the church's main document, *The Book of Mormon*, in a hill in upstate New York. It was in a foreign language. So he said he was able to translate it by wearing a special garment. Porter was one of five or six boys Joseph brought into the church he was founding; Porter's story is told in a book by Harold Schindler, *Orrin Porter Rockwell: Man of God, Son of Thunder*. Salt Lake City: University of Utah Press, 1966. Porter's father apparently printed the first copies of *The Book of Mormon*. To add to all that, two of my other ancestors by the name of Brizzee were members of the Mormon Battalion, 600 soldiers of Brigham Young's Navoo Legion who were sworn into the US Army to help secure the West of the United States from a possible take-over by Mexico. Now back to the 20th Century. The capital's grounds in Salt Lake City have a bronze plaque listing the names of all the soldiers in the Battalion.)

Sometime around the mid-1970s, I met the future Governor of Wisconsin, a Mr. Schreiber, at a small Democratic Party affair. Late on, I was at another one. Someone tapped me on the shoulder while I was getting a glass of punch. It was Schreiber. By then he was the Governor. He said he recalled that I was with the College of Agriculture, and said he wanted me to sound out the Ag faculty about a possible School of Veterinary Medicine at the University of Wisconsin. This was a big issue at the time, and the Board of Regents had come out flatly against it, saying that the contract Wisconsin had with the Minnesota vet school was enough. I thought, "Boy! This is 'way above my league!" So I decided to tell the Dean of the College and seal my lips. Shortly afterward the TV was full of news from farmers' groups calling for a vet school to be provided at the University of Wisconsin. A few weeks later, Schreiber came on TV, saying that it had 'come to his attention' that there was a demand for a vet school to be built at the University, and he went on to say he should be provided with funding options at three different levels. So he got his three optional plans. The Chair of the already existing and well known Department of Veterinary Science, Bernie Easterday, was saddled with the responsibility of drawing up the plans. And, yes indeed, the School of Veterinary Medicine was built and the Department of Veterinary Science was eliminated. Bernie became dean of the School. Twenty years later I decided I'd been mum long enough. So I told Bernie how it all came about. Still later, Dean Pound of the College of Agriculture and Life Sciences, was retiring. A farewell party was held for him. As I went up to shake his hand, he said, "Hello Haller—my partner in crime." He meant that by forwarding Schreiber's plan to him I was breaking the law, and by his carrying out Schreiber's plan he too was breaking the law. He was exactly right. The whole business was illegal because it went around the Board of Regents, who (it will be recalled) was dead set against it. Still, my role was merely as a conduit, and how can you refuse to do something the Governor asks you to?

My second Fulbright professorship was at the University of Sao Paulo in 1974. That year Helcio Saraiva, Jose Pastore and I paid a visit to IBGE, the Brazilian Institute of Geography and Statistics. At the time it was headed by a young economist and demographer named Izaac Kerstentzky. I had met him in 1962, and even earlier (1953, I believe) he had studied in Brazil, with Professor John Kolb who was then on leave from University of Wisconsin's Department of Rural Sociology. And a few years later Izaac had come to Madison to visit the Department.

At IBGE, the three of us were invited to do an informal seminar with three people in the Social Affairs department of IBGE, one of whom was Nelson do Valle Silva, demographer who had studied at the University of Michigan. (He is still active, and we still have occasional contacts with each other.) I found myself having to give an impromptu discussion of stratification theory as it existed at the time, together with some of its backgrounds in European sociology of the 19th and early 20th centuries. The result was that we were given access to an incredible set of national household sample survey data that were collected in 1973. Our group has used those data over and over again. For the first few years the data set was a secret. We could use it but we couldn't let anyone else have access to it.

Early on, Pastore used these data to do his *livre docente* thesis at the University of Sao Paulo. His thesis was the first empirical analysis of social mobility in Brazil, a project he was able to carry out in part because of the support from the Ford Foundation we mentioned earlier. Until his work was published it was believed that there wasn't any social mobility among the people of the nation. Indeed, Brazil's leading sociologist, Fernando Henrique Cardoso, had declared that the nation's social structure was fixed—that there was no such thing as social mobility in the country. (In the Year 2000, Cardoso, by then President of Brazil, conceded publicly that he had been wrong and Pastore was right.) For what it's worth—not much—I met Cardoso a couple of times. .

It may have been sometime around 1973 or so that I first met Neuma Aguiar. Our views of sociology were essentially the same, and continued to touch base ever afterward. At Wisconsin we had a committee on Latin American affairs. Among other things, it was responsible for recommending Tinker Visiting Professors. In one of the meetings an economist suggested Neuma as a possible Tinker Professor, but he said he didn't know how to contact her. It so happened that I had her phone number, so the committee asked me to call and invite her to be the first Tinker Professor at Wisconsin and to offer her a stipend of $6,000 for one semester. I called that night and she accepted over the phone. That was the first of her many trips to Madison, one of which was to work with the Women's Study Program for six months. The Women's Study people asked her to stay on as a permanent member of the Program. She was willing, provided that she could spend six months with them and six months in Brazil. However, they were unable to do so, so the effort came to nothing. On an earlier occasion, the University of Wisconsin awarded her the Honorary Degree of Doctor of Science.

In 1973, Alex Portes and I published a theory of status attainment processes that seems to have become legendary. When a piece is published its future is out of the writers' control. Most—around 70% it is reported—die immediately. They are never cited. Of those that are cited, most peter out in short order. Others fly off into the stratosphere where they shine down on us mortals. This article is one of the latter. It seems to have been republished in 2010 or maybe 2011.

In 1974 and 1975 Jose Pastore and I, with others sometimes, published a series of articles on the concept and measurement of authority.

Also in 1974, Pastore and I were planning a small research conference, one that led to his studies of social mobility, and we wanted to include someone from the United States. We chose Jonathan Kelley because of the reanalysis he did of a study Dave Featherman had published. Kelley joined the group. This set off the long period of collaborations mentioned in a couple of paragraphs back.

Sometime around then I put in four years on a committee to evaluate applications for Edward LaRoque Tinker Fellowships for study in Brazil. Martha Muse was the President of the foundation. We became friends, largely because both of us liked and admired Ed Young, an economist who had done just about everything one can do in the University of Wisconsin. He was a professor of economics, then director of the Industrial Relations Research Institute, Dean of Letters and Sciences, Chancellor of the University, and finally President of the University of Wisconsin System.

Now let's backtrack a bit, to an incident where a failure of mine and an action by Ed Young led to an improvement in the quality of the University's football program.

Back in the late 1960s, some of the top administrators, who wanted the University to look like the Ivy League, decided to downgrade football. They succeeded by appointing a fourth rate head of sports and an equally nondescript coach. Of course, Wisconsin's football record was dismal, just as these administrators wanted it to be.

During that period I had been appointed chair of a committee to oversee a program for Black students run by Ruth Doyle (whose son was, in 2010, the Governor of Wisconsin). A group of Blacks rebelled at the idea that the program could be led by a white lady. So I set up a meeting of my committee to make a decision regarding Ruth. I was sure that I had enough votes to keep her on. We met in a small area of a larger room. Our area was partly shielded by a movable board wall. I sat on the west end of the table. A black girl, a student, sat on the north side. The board wall was directly in front of her, across the table. What I didn't at first know was that some black students were waiting behind the board wall, ostensibly to see what was going on. I knew that the girl was planning to vote to keep Ruth, and that with her vote we would have just enough votes to do it. But when it came to vote, the black students threatened the girl by shaking their fists at her. Of the committee members, only she and I could see this. The girl was intimidated; she voted as the outsiders ordered her to. Ruth lost the position. I called Ed Young, who was Chancellor at the time and told him what happened, then went on to say, "We need bread and circuses." So Ed rebuilt the football program.

During these years I made many trips to Sao Paulo to collaborate with Jose Pastore. Some on these were funded by USAID, others by Fulbright grants. The University of Sao Paulo's (USP) Institute of Economic Research held the federal contract to advise the Ministry of Labor (ML), in Brasilia. Pastore led this work as right hand man to the Minister, Murillo Macedo. Actually, Jose ran the Ministry. To help him he added Fernando Rocha and Renato Simplicio Lopes as two of his three subchiefs, formally called secretaries. The third was an economist from Recife who, incidentally, married a friend of mine who was a Wisconsin Doctor of Law named Alexandrina Sobreiro. Alexandrina once told me that she met her future husband, Jorge Jatoba, at a luncheon I gave for Brazilians and brazilianistas at the UW Faculty Club. So the Ministry of Labor was run by Wisconsin PhDs. As a matter of fact, I was one of them. Literally, I helped run it, though Brazil was their business and not mine; I didn't hesitate to tell them where they were off base.

One incident may be interesting. For many years, whenever the industrial workers went on strike the Ministry of Labor called out the Army to quell it, and armies are trained to use force—to hurt people. I told Murillo and Pastore they should end that practice and let management and labor fight it out themselves. They accepted that advice. I often joined in the research and policy trips to Brasilia. And as time went on Murillo and his family became a good friends of mine.

There was another instance in which I decided to try to influence Brazilian policy at the highest levels of government. The situation was this. Inflation was running up to terrible heights. The University of Sao Paulo's economists, who were running the economy, would look the previous month's inflation rate and try to hold it down, without any success. Rich people were protecting themselves by buying gold. Ordinary working people, were paid the first day of each month, were spending their checks as fast as they received them, because their value dropped precipitously, to the point where they were worthless by the end of the month. I looked at some old data, from the last half of the 19th Century. At that time the urban population was a small percentage of the total population. Around 90% were farm workers who barely made or grew enough to feed their families. The rural property owners, with vast land holdings, were rich. In Sao Paulo, they produced coffee to sell on the world market. But like many businesses everywhere, they needed capital to keep producing and marketing. So they would go to the government, then in Rio de Janeiro, and ask to be funded. The government would give them what they wanted and print money to cover the costs. At first this load fell lightly on the shoulders of the vast rural population. Inflation had set in but at first it was so low it was hardly noticed, if at all. But as time went on, the rural percentage of the population steadily declined. And the coffee magnates continued to demand get subsidies from the government, and the government paid by devaluing the monetary units. So the inflation rate rose rapidly. But the mid-1970s it was out of control. But the economists, who knew nothing about demography and paid no attention to the demographic trends the nation was experiencing as it grew and shifted its weight from the rural people to the urban ones, didn't understand any of this. So they treated each month's inflation are something unique, and tried all sorts of unsuccessful ways to curb it for the coming month. Interestingly, their reports were bathed in complicated formulas. Why? My guess this was to let people think they were doing their best to control the inflation rate. Maybe they really meant it; I don't know if the formulas were or were not meaningful. But whether or not, they were irrelevant. The solution, I realized, was to refuse to grant the subsidies that had become taken for granted, and with that refusal, to quit printing inflationary money.

I showed Pastore my interpretation of the causes of the awful inflation. He wasn't interested. But there was another means of communication. I regularly sent reports of the

Fulbright officer in Brasilia. One of them was my theory of Brazil's inflation rate and how to stop it. Inflation came to an abrupt halt. The subsidies stopped and the money printing rate came down correspondingly. Did someone act on my recommendation? Fernando Henrique Cardoso had become the minister of finance. He's a sociologist, not an economist. Maybe he figured out what was happening and how to fix it all by himself. Or maybe he read my report to the Fulbright people. Who knows?

Another incident of great importance in Brazil needs to be told: the *abertura*—the 'opening'—and its early fallout. For years the newspapers and the TV had been censored, and the book stores at the University of Sao Paulo (USP) were prohibited from selling anything that looked to the generals as if they might be Marxist. I regularly read the paper called *O Estado de Sao Paulo*, a paper that resisted military rule. Every time the paper ran a column the censors didn't like they cut out the offending paragraphs and substituted lines of poetry. One day, with no warning, the censorship stopped. Nothing was said. It just stopped. At the same time, the USP bookstore found itself stocked with books that had earlier been banned as subversive. Again, nothing was said. These moves were part of the *abertura*, though the *abertura* itself was kept quiet for a number of years. President Geisel—himself a general—was behind this. He seems to have thought that it was fast becoming time for military rule to cease.

This had a most curious set of effects. The Sao Paulo priests noticed that the pressure on labor was decreasing. So they tried to help it decrease further by stirring up the working class. Then one day Murillo Macedo and his wife made a trip to Rome to 'be blessed by the Pope.' (I think Pastore was behind this.) Anyway, right away the Pope came to Sao Paulo and told the priests to shut up on the grounds that the more noise they made the more likely that the so-called *Linea Dura*—the hard lining generals—would gain control of the military government and halt the *abertura*. So the Pope came to Sao Paulo and told the priest to shut up. They did. And the 'opening' continued.

So how could all this go on when I had duties at the University of Wisconsin? In one sense I really don't know. Clearly, the University administration, especially the Dean of the College of Agriculture and Life Sciences liked it. Indeed, one time he asked me to keep the Brazil trips short, no more than a summer and a semester. And how about the Fulbright Commission and other US federal agencies? They too were providing me with extraordinary support. Maybe it was because of a combination of things. One may have been that, as a Fulbrighter, I wrote final reports on how I saw the social structure of Brazil and its changes. There may well be something to this: I often noticed that the US Embassy people had very little idea of the way Brazil worked, except for short term political affairs. These didn't interest me and I didn't say anything about them. Indeed, I had a personal policy of never carrying tales between the two countries. I avoided them as much as possible, and avoided the ingrown American colony completely. The fact is that sitting at my desk at USP I often overheard gossip about important policy matters. But I kept them to myself. There may have been another factor at work: I avoided contact with Americans as much as possible. And still another. I had "angels', like Joao Goncalves, Pastore, and Manoel Tourinho looking after me. I was surely one of the few Americans who had such angels during the years I worked there—maybe the only one. I know of instances in which one or another of the above blocked efforts to undermine my work. But maybe there were other such times that I was never even told about.

Yet there were occasions when I found myself talking to US Embassy people. One such was amusing at first and annoying at the end. The head of the Fulbright program, in Brasilia, held a party for the local Fulbrighters. I was there at the time and attended the affair. That day there was a strike going on amongst the metal workers in Belo Horizonte (BH). I learned about it earlier in the day when I was working on the 8th floor of the Ministry of Labor, along with a bunch of Brazilian economists. Suddenly we heard a BANG BANG just below the building. The economists rushed to see what was going on. It was just a truck backfiring. But that set them off in a free for all discussion. I listened, and learned why they had been so excited. The thought the Bangs were shots. Apparently the strike of the BH workers had been met with fire from the army. While I was at the Fulbright party the political officer from the US Embassy asked me what was going on. He must have had an inkling, but no more than that. I told him to read the headlines tomorrow. At ten o'clock the next morning he telephoned me, asking how I knew about it. I told him something like 'just keeping my ears open.' The next day I was at the airport to take a plane back to Sao Paulo, when a couple of Embassy people came to me and said the Embassy owned two seats on each American Airlines plane going to the States, and Pastore and I could use them any time we wanted to. This was a clear attempt to bribe me. This was shocking. I thought American officials didn't do such things. Needless to say, I never took them up on it.

In any case, I always spent at least a semester at Madison each year and kept up with my teaching. My teaching was almost always done in the fall semester. This included working with my graduate students and senior post-doctoral students. I was able to fund my American doctoral students. But the Brazilians had to come on their own or their government's resources. And they did. (A list of these from MSU and UW days is given at the end of this document.)

In hindsight it is clear that 1974 was a key year regarding my work in Brazil, not only for the reasons given above. That was the year I met Jose Israel Vargas. He, more than anyone else, became my mentor on Brazil. As was already said, Vargas is a nuclear scientist with an incredible range of knowledge. Among other things, he likes sociology. We met in Madison. I had invited him at Helcio Saraiva's suggestion. Vargas, PhD in nuclear chemistry from Rutherford's Cavendish Laboratory in Cambridge University, was then head of the Minas Gerais research institute called the Joao Pinheiro Foundation. Helcio was one of the foundation's directors. I had some funds at my disposal at Wisconsin. So he came to Madison and I showed him around. He gave a talk to the sociologists and another over the air at the local Public Radio Station.

Actually, there was more to Varga's work in Brazil than met the eye. He never said anything about it to me but I put two and two together and figured out much of what he was about. For years he had been in exile in France because the Brazilian generals didn't like him. There, he served as the scientific advisor to the French Atomic Energy Administration while at the same time administering four different atomic programs in the south of France. The Brazilian government asked him to come back when they began to cooperate with Germany over uranium. Perhaps with German help, he was to build a set of nuclear reactors in Minas Gerais. All this was secret, of course. But if you keep your eyes and ears open and your mouth shut you can learn a lot even if you're not actively trying to.

As the years past, he rose higher and higher in the Brazilian government and at the same time carried on a career as the chair of UNESCO's Executive Committee in Paris. He and I kept in touch wherever we happened to be at the same time: Brazil or Paris, or the United States, more recently via the internet. Sadly, he is in poor health these days. My guess is that at least some of his problems are due to exposures to radiation from his work. He has two residences, one in Belo Horizonte, his home city. The other is in Rio de Janeiro. He still works, now at the Rio de Janeiro Institute of Physics.

Around the late 1970s or early 1980s I was been asked to do a little job for USAID that required some meetings in Washington. Just before going to one of them I got a message saying I was 'dis-invited'. Then another message came from the chairman of the meetings who said that I was rejected by the White House. This was at time when we had a Republican president, Richard Nixon. I take the rejection to be an honor.

This wasn't the only time the Republicans made trouble for me, although I beat them (sort of) another time. That time was when Ronald Reagan became President. In January, within a couple of weeks after he was sworn in as President, Reagan issued an executive order prohibiting federal funding for any project that had in it the words 'social' or 'sociology'. This was a serious threat to my work and the promised stipends for my research assistants, not to mention that of many others. I was two years into a project funded by the National Science Foundation (NSF), had been awarded an extension that was to begin in February, and had already promised my assistants a continuation of their positions through the spring semester. So I called the office of Congressman Kastenmeier, from the Madison area, and told his people what was happening. A day went by. Then I got a call from the NSF's sociology officer who happened to have done his doctoral work with one of my own UW department's faculty members. He was crying over the phone—literally—and was wailing, "What have you done to us? You've put us in trouble with the President and Congress." The next day I got a phone call from Kastenmeier's office asking if I had heard from NSF. One more day, and another call from the NSF officer: "We are SO glad you raised the issue. We can fund your project and those of other sociologists whose work we are supporting." So our research went forward.

Still, I was not the only one who raised the issue. The president of NSF sent a letter of protest to Reagan. The only answer he got back was, "This is treachery". Note that the word *treachery* is as close as you can come to the word *treason* without actually calling it that. And treason may be punishable by death.

In any case, sociology was saved.

In 1978 Manoel Tourinho arrived at my office unannounced, to do a PhD with me. He was then the director of research at CEPLAC, Brazil's Cacao Research Institute in Itabuna. After he finished his doctorate and returned to Brazil we worked together quite a lot in Brazil itself. For one thing he sent me off on various trips to the north of the nation to get me familiar with the Amazon Region, as shall be seen below.

In 1981 I had a fellowship to the Australian National University (ANU) to work with Jonathan Kelley, the same sociologist Pastore and I had invited to work with us in Sao Paulo in the 1970s. Actually, Kelley and I didn't do much together at ANU, largely because he wasn't there much of the time. So I worked on one of my Brazil projects: developing maps of the socioeconomic demographic development levels (SEDs) of Brazil's small political units. As always, I did it for my own research purposes: to identify macroregions that could be used as surrogates for nations, thus to compare the stratification structures of each in relation to their level of development to see how such structures varied with levels of development. It would have been impossible to do that for nations because the data are not comparable from rich nation to rich nation and did not exist at all for poor nations. (Later on I used these maps and the SED values of the regions they described to test the outstanding stratification belief of the day: that development induces equality. I found that this comfortable notion is nonsense. The results were published in 1982.)

Also during the six months in Australia, I was asked to give lectures in Melbourne and at New England University up country in New South Wales. At the University of Melbourne I gave a talk on Joe Woelfel's 'galileo' measurement system. The talk resulted in a decades' long collaboration between one of the professors there, John Cary, and Woelfel. And there were one or two other talks there in the city. For another thing, my good friend from the University of Wisconsin, Solomon Levine, was teaching in Melbourne at La Trobe University. So we were able to get together. Then, after I returned to Canberra, he and his wife, Betty, visited with us there, and he gave an improvised lecture at ANU. He was a well known expert on Japanese labor relations. So Aussie scholars—always concerned about their nation's proximity to major Asian nations—flocked to his lecture, as did diplomats from various nations who were stationed in Canberra. The seminar room overflowed, with people out in the halls, trying to hear what Sol was saying.

Toward the end of our stay in Canberra, the Brazilian government asked me to go to Brasilia to get an award. It was the Order of Merit of Labor, and at nearly the highest level possible—Grande Officer. So now I have all the medals that came with it in a small display case.

In 1982 I published two of the most important pieces of my career, at least as I see them.

One was the socioeconomic regionalization of Brazil (1982). As said, I needed this to use regions as proxies for nations, in order to test leading suppositions holding that development induced equality. That was my objective. But the piece didn't stop there.

Brazilians of course knew very well that their huge country varied a lot—deserts in the Northeast, jungles in the Amazon, even occasional snow in Rio Grande do Sul. So they tried several times to come up with a satisfactory way to show the various regions. One that was really absurd was done by a fine anthropologist whom I knew, Manoel Diegas, Jr. He tried to do it with so-called 'typical' occupations. It was nonsense. Also, a team from Canada and Great Britain had its go at this. I studied how they did it. They factor analyzed a matrix of 26 states by 40 variables. This way, they ignored enormous differences within states. Pretty bad. But there's more. They threw in every variable they could think of, supposing that the right ones would show themselves. Also pretty bad.

Obviously, smaller areal units would have to be used. I chose the 360 official micro-regions of the nation.

I looked around to see if there was any theory that could be used to guide the selection of variables, the values of which would be attributed to the micro-regions. There wasn't. So what to do? I reasoned that there were two powerful research traditions that were relevant even though neither was directly on the issue. In one, economists were dividing up the world on variables describing nations. The other was the sociological tradition of measuring the socioeconomic levels of households or families. So I decided to put about five variables from each tradition into a correlation matrix and factor-analyze it. I thought that maybe two factors would emerge, one sociological and the other economic. But what came out was just one single and powerful variable: SED — Socioeconomic Development.

The maps we prepared from this analysis were used for a long time by Brazilian planners. And after we did it, I'm pretty sure the Brazilian geographers repeated our analysis. Of course they never cited our work; government officials rarely do, either there or here in the States. I guess they hope to gain favor with their superiors by giving the impression that they are so brilliant they're worthy of advancement.

The other of the two 1982 pieces was published in a book in honor of Bill Sewell. It is a generalization of the well known Sewell, Portes, and Haller status attainment model. It's called, "Reflections on the Social Psychology of Status Attainment". It has never been tested. This is too bad. The Sewell, Haller and Portes precursors of it have been tested several times. Sewell, Olendorf and I redid it on places of different sizes in Wisconsin. My group redid it with better data on a Michigan sample. And Hauser, Tsai and Sewell tried to beat it down by applying reliability estimates to its variables. It stood firm. (Toward the end of their article, one of the authors—Sewell, probably—said, "We did it right the first time.")

Going back to the regionalization research. Among a lot of other places, I presented this was the research group at CEPLAC at Manoel's suggestion. Afterward he asked me to give the same talk to a visiting journalist. The more I talked the more agitated she got. Finally, she came down hard on me, saying that this is subversion. I then realized that she was a sort of spy for SNI, Brazil's then military government's combination of the CIA and FBI. For about 24 hours I thought I would disappear and never be heard of again. But my friends intervened and I was saved….Whoever got the idea that sociology is a bed of roses? Done well, it's not. Even then I had the last laugh. I had been working with maps of Brazil that were made for purposes that were different from mine. Looking at a set of islands 200 or so km off the Northeast coast of Brazil, one could see the kinds of activities that were going on there. As a former sailor in the US Navy, I knew what a naval base looked like. These islands were a secret naval base of the Brazilians. Still, there is a moral to this story.

Objective sociological research threatens the social myths of both the left and the right, and they don't like it.

9. Amazonia, Japan, and Australia.

Indeed, sometimes I have been accused of being a communist, at others a fascist. In general, under the dictatorship I seem to have been watched pretty closely. But my friends, such as Manoel Tourninho, Jose Pastore, Jose Israel Vargas, Murillo Macedo and others always kept me safe. It pays to have angels in high places.

Manoel went on to serve as the Executive Director of EMBRAPA, the nation's agricultural research agency. Interestingly, EMPRAPA was designed in 1972 by a small committee of social scientists, including several of our people—Jose Pastore, and Renato Simplicio Lopes, former graduate students at Wisconsin, and Juan Diaz Bordenave, my advisee in Michigan State's Department of Communication, who was then the Brazil representative of the Organization of American States' Institute of Agricultural Communication. Pastore led the group. In EMBRAPA's new, centralized form it laid out a plan to send large numbers of outstanding young Brazilians to the best agricultural PhD programs in the United States. As these new PhDs came back to Brazil and brought farm production up to world class levels, export agriculture took off—in a few years making Brazil one of the three or four nations leading the rest of the world in this area. (Recently another who was in that meeting told me that EMBRAPA was essentially designed by Pastore and Manoel led EMBRAPA for a term of about two years. Not incidentally, Tarcizio Rego Quirino, who was another of my PhDs, served EMBRAPA for many years as its chief of strategic planning. So my old doctoral students were all over EMBRAPA.

Later, from its modest parent institution, the Faculty of Agrarian Sciences in Belem, Manoel founded the Federal Rural University of Amazonia, also based in Belem, with branches in other parts of this huge region. The new university was inaugurated by then-President Fernando Henrique Cardoso.

As things stand today Manoel has retired, at least formally, and is living with his wife, Dora, in Belem. They are managing a small program of research and action in an Amazonian riverine community, helping it to become self sustainable for the long pull.

Parts of the next paragraphs are repeated, with nuances that may be of a bit of interest.

On a Fulbright in 1979 Manoel arranged for a CEPLAC economist and me to visit a couple of Amazon colonies. Our job was to evaluate each of them in terms of their long term viability. This was because CEPLAC and EMBRAPA had been asked to help to develop agriculture there. The economist who was my partner, Jaimi (Jimmy) was a Brazilian citizen born American in California. He was an interesting guy. His face looked like nothing more than the nose of a horse. Also he had been in Brazil so long he could pass for a Brazilian. But he was much loved. Indeed, when he married a girl from Recife Dom Helder Camera, Brazil's then most famous bishop, presided over the wedding.

One of the colonies was a place called Alta Floresta, high up in Mato Grasso, near Para, and about 200 miles north of Cuiba. It was a gleba, a huge land grant, totally owned by a Sao Paulo Company that was owned by a Paulista family. The best way to get there was by air, which is how I managed it. Manoel must have made the arrangements because the Company sent a plane to take me. I don't remember how Jaimi got there. There was no one else other than the pilot in the plane I was in. (It was a small aircraft with just one engine.) Evidently the flight was chancy, because Manoel told me later on that he was praying all the time I was supposed to be in the air. Actually, the company itself had three planes wholly devoted to contact between Alta Floresta, Cuiaba and Sao Paulo. There was also a dirt road running north to it from Cuiba, although it was not passable in the rainy season. The two of us, Jaimi and me, stayed there three or four days, looking over the Company's two villages, the small plots allotted to the *compones* ('peasants') who were brought there, and the huge properties—the largest of which belonged to the Company, the lesser ones to each of the members of the wholly family-owned Company. We concluded that indeed the cacao production system at Alta Floresta was viable. And the last I heard or read (from Time Magazine, I think) the town of Alta Floresta had grown and was thriving.

Later on, I learned some more about Alta Floresta, something that the owners kept secret. Gold. Now I think the agricultural colony set up was a cover for their gold operations. Indeed, with the wisdom of hindsight, that would seem to explain the route by which they first arrived there. They said they came down onto the Teles Pires River from the Amazon River. It would have been a lot easier to drive up from Cuiba during the dry season. My guess is that from the Amazon they boated down the Tapajos. This would seem to be a strange way to look for farm land. They knew where they were going and probably had already determined that gold was to be found there.

Later on, in Sao Paulo, I noticed that the company had set up its own bank.

The other visit we made was in the State of Rondonia, some 40 miles or more southeast of Porto Velho, the home town of Manoel Tourinho's family. (He told me that his mother walked to Porto Velho from Cuiaba, a distance on 600 or more miles.) Our job there was the same as for Alta Floresta, to check the viability of a large cacau property that was recently opened. We were filthy from the dust of Alta Floresta when we got there. This area was owned by the government and operated by three competing federal organizations, one of which was CEPLAC. We needed to learn as much as possible about the views of the three Rondonia organizations. Because there was no central authority over the three we decided to hold a group meeting to explain what we were about and to get the views of the leadership of the three groups. Back in Madison, when Manoel had asked me to get to Brazil ASAP and to go up in the Amazon area, I went to a store and bought four short sleeved knit shirts. What I didn't notice was that they all had epaulets on them. One shirt was tan, like the military colors of those days. When we got the three groups together I was wearing the tan shirt. An army lieutenant looked at me, with my tan epaulated shirt and filthy khaki-like pants and said, "WHAT is this American army officer doing in OUR Amazonia?" His exact words. Jaimi then took over and said, "He's no army officer, AND he was sent here by the Brazilian Government!" (So there!)

From there we flew west into the far west state of Acre. While there we got even more filthy and sweaty. But still in the same clothes, we flew out of Acre's capital of Rio Branco. Worse yet, our plane routes and the available schedules were messy. So we took a flight north and then east to Manaus. The flight was interesting. It took us over areas rarely seen by foreigners even today, not to mention most Brazilians. It took us over a water wonderland from the Solomões and Japurú Rivers, as well as many miles of solid forests.

At the airport we had to go through customs. Having been working for so many days we were loaded with baggage. And we were tired. The customs officer was a self important, officious pipsqueak who demanded that I, a foreigner, would have to show everything. He challenged everything. I had a camera and some other things he didn't like. As I remember, he took them away from me. But finally he let us go.

Returning to the SES macro-regionalization of Brazil, that article (1982) took on a life of its own. Vargas and his government associates used it to justify allocating development resources to the now-rich agricultural area of Goias. Later, Embrapa used it to decide to locate an agricultural research unit in the new State of Tocantins.

In 1980 Anna Roosevelt, an anthropologist, and I put on a symposium on Amazonia at the annual meetings of the American Association for the Advancement of Science, in New Orleans. Not that I knew much about the region. Indeed the reason I agreed to co-sponsor the symposium was to force me to learn something about it.

10. Australia, and Ohio State University.

In 1981 I was offered a six month research fellowship at the Australian National University in Canberra. Hazel and our son, Bill, came along. On the way there, I got the word that the Ohio State University was offering me a Distinguished Professorship in

Rural Sociology for 1982-1983. Among the flattering aspects of this was the fact that a recent predecessor was the famous Nobel Prize winner, Gunnar Myrdal.

Some time later he came to Madison to work with Institute on Poverty. His, wife, Alva, came along him. She regularly showed up at the seminar Bill Sewell and I ran on stratification. Why? Were the Myrdal's considering Sewell for a Nobel Prize? Or were they interested in what our group had to say about stratification? This seems more likely. In any case she attended the seminar.

At Canberra I was to work with Jonathan Kelley on research on Brazil. Actually, we didn't really do much together because he was away most of the time. I was asked to give talks here and there. I gave some in Melbourne, in the State of Victoria. One or two were to the Melbourne University faculty of agriculture. I recall telling them about Joe Woelfel's galileo system. A young faculty member named John Cary was present. Later on he came to Madison to work on galileo. Then he started working directly with Joe Woelfel. They collaborated for years. Also I gave some other lectures in the city. So I stayed there for about a week. At nights I worked on the map of the socioeconomic development levels of the peoples of Brazil—a publication which later had an unexpected influence in that country. Also, by coincidence, our close friend from the University of Wisconsin, Solomon Levine, was in Melbourne. We got together there and later he and his wife, Betty, came up to Canberra to spend some time with us. I knew he was an expert on the Japanese system of labor relations—right after WWII he even helped to design Japan's famous Industrial Relations Institute—but I didn't realize then that he was also famous in Australia for his work, not only in Japan, but also in the US. When he gave a lecture at the Australian National University the ambassadors from various nations were there and the large seminar room where he lectured, informally, was packed and people were standing out in the hall to hear him.

Back to Melbourne for a moment. By chance, one of my former students, Pamela Weir, was working there as a human relations officer for a large company. For her doctoral thesis research, conducted there, she found a sample of youths who had no Significant Others. To test the effects of such special people, she provided SOs for the sample members and observed the results. She was then able to get SOs set up for them….At least this is the way I remember it. Her work fed into the research Joe Woelfel, Ed Fink and I had been doing in the previous decade.

Around November we learned that our daughter, Stephanie, was coming to visit with us in Canberra. While we were waiting for her I flew up to the University of New England (in New South Wales) to give a lecture the next day. I called home at around 7:00 a.m. and was told that a telex message had arrived, telling me to make arrangements ASAP to go to Brasilia to be decorated by the Brazilian Government. I thought there must be some mistake. But it was true. Stephanie spent a couple of weeks with us. Then I took off for Brasilia. But how to get there from Canberra? There were several possibilities, none of them direct. One was to go to New Zealand and take a flight over the Antarctic to Buenos Aires, one that flew only once a month, and then fly to Rio and from there to Brasília. Another was to go to Tahiti, wait two days and fly to Santiago, Chile, then fly to Rio and onwards. The longest one turned out to be the best: leave Sidney for Los Angeles, go from LA to Lima, Peru, and from there fly into Brazil. This was a long trip. But it was the one I took. The Government of Brazil paid all but A$100, which I believe was really a scam collected by the Brazilian travel agent in Sydney.

So I made the trip and received the award, the Order of Merit of Labor, rank of Grande Oficial, one of the highest decorations offered by the Brazilian Government. When I got back to Canberra our Aussie friends likened it to a British knighthood. Maybe they were right. I don't know. The American Ambassador and a lot of other people were present for the ceremony. The order for the award was authorized by the President of Brazil and presented by the Minister of Labor, my friend, Murillo Macedo. There was a big party afterward and we still have a picture of me looking a little bit tipsy.

This not the end of the story. Here is the cast of characters. 1. Harry Manning Secretary of Australia's World War II Veterans organization. 2. An unnamed head of the nation's War Memorial Building. Call him WMHead. 3. A Philippine woman whose name I don't remember; call her Gloria. She was the daughter of a general and wife of their country's mission to Brazil. 4. A former University of Wisconsin student of library science; call her UWGirl. 5. Salete Marinho, a friend of UWGirl from Madison days and a former student of ours. 6. Frank Jones, head of the ANU sociology research program. 7. A political scientist. Call him PSc. 8. The Brazilian Ambassador to Brazil, Marcos Cortes. 9. The US Ambassador to Australia. The Brazilian Representative in Sydney. Call him Brazil Secretary.

All of a sudden I began to notice that I was being investigated. By whom? The Americans, the Brazilians, or the Aussies? And why? The Brazil Secretary from Sydney came up to Canberra to interview me. I showed him the Brazilian data I was working on and he pumped me. I realized that he was more than just the Sydney Vice Consul. He was SNI (Servico Nacional de Investigacao, like the FBI + the CIA). One day Harry Manning was leading me around the Latin American collection at the National Library. UWGirl was following us around. Then she announced that she had studied at the UW and asked what I did there. I told her I taught sociology. She said she had a syllabus for that field. Actually there were only two at the UW, mine and Professor Robert Hauser's. I asked her how she got mine. She said her UW roommate, Salete Marinho, gave it to her. Things were getting stranger and stranger. So I wrote a letter to Salete and she confirmed UWGirl's story.……Now Harry liked Brazil and he had a friend from earlier days, Gloria, who was married to a Spanish government official , now stationed in Rio. And Harry was concerned that Australian War Memorial chief, who hated Brazil, was refusing to recognize Brazil's participation in WWII with a proper plaque at the War Memorial Building, saying that there was no evidence that Brazil recognized Australia's participation in it. Of course, this was just an excuse for discriminating against Brazil.

So Harry Manning asked me to go to Brazil's War Memorial building when I got to Rio and take a picture of it if indeed it was there. He also asked me to look up Gloria and ask her to pose by Brazil's plaque honoring Australia and take a picture of it with her in it. He gave me Gloria's address, and I went to it and talked with her about it. But she was afraid to go out on the street in Rio, and had remained in her and her husband's Botafogo apartment the whole time they had been in the city. I talked her into going to the Brazilian War Memorial building with me. It was within walking distance of their place. So we went, and I took a picture of her standing by the Brazilians' plaque honoring Australia's war participation. Then I went through the other things that were planned for my short visit to Brazil—the award of the medals for service to Brazil—and returned to Canberra. Harry showed the picture we had taken and the head of the nation's War Memorial building was thus forced to recognize Brazil.

After the award business in Brasília was over, I flew back to Rio and stayed overnight there. Neuma came down to where I was put up in, and we had a chance to talk a bit. (I don't know how long we have been friends, but probably more than 40 years.) Then I took a night flight from there to Lima over the Andes. It was a memorable night, clear skies and a bright moon. We flew over Lake Titicaca. It was not only visible but actually reflected the light of the moon. Beautiful. Eventually I got to LA, with an eleven hour layover. By then I had developed an infected cyst of sorts on the surface of my chest. So I looked up a doctor to lance it, but he refused. (Can you imagine that?) . So I flew on to Hawaii, Fiji, Sydney, and Canberra. There I picked our faithful 20-year-old Holden car (a kind of an iron clad vehicle made by Chevrolet) and drove home. Arriving at the house, I was immediately sent to the nearby hospital to have the infection removed.

Now a parenthetic comment about the Holden. This ancient auto was one of the best I ever had. It was bought for A$500. It did 20 miles to the gallon, far better than American cars of the day. It was so sturdy it felt like a tank. We used it for lots of weekend trips from Canberra to the Pacific shore.

Though we didn't realize it at the time, Hazel was showing the first signs of the cancer that ended her life. In our ignorance we all had a ball. Bill, age 15, attended high school near the house and rode all over Canberra on his bike. He was also impressed by the forthrightness of the Australians' discussion of the Vietnam War, something the Americans were reluctant to do in those years.

All in all we had a wonderful time in the Antipodes and then we returned to Madison to stay until it was time to go to Ohio State.

We enjoyed our time in Columbus very much. There were lots of old friends there. Dave and Aida Hansen were there, of course. They have been two of our staunchest friends ever since his graduate student days at Wisconsin. Really, it was through Dave's efforts that we were invited. It was then, also, that I met Saad Nagi, Chair of the Department of Sociology at that time. We had several discussions about how to improve the department. My suggestion was to add stratification and social psychology. Then he offered me the startling opportunity to hire six professors of my choice. As explained below, I was unable to accept this incredible possibility. The Nagis and the Hallers became friends for life.

It was Dave Hansen who arranged the 1982-1983 position at OSU. It was called Distinguished Visiting Professor of Rural Sociology. As said earlier, Dave had done important work in Brazil during the seven years he and Aida had lived there. Indeed, I once had the good fortune to stay with them for a week at their sea-facing apartment near Botafogo.

Tseng and Pei Tseng Wu, old friends from MSU days, lived only a block from the house we rented in the Columbus, Ohio suburb of Worthington. Dave and Aida lived a mile or so from us. Bill and Sue Flinn were also there. Bill had taught at Wisconsin before returning to OSU, his alma mater, eventually to serve as the head of the Midwestern Universities Agency for International Activities—the same organization that had funded Alex Portes' research in Chile nearly two decades earlier.

But there were clouds we still didn't understand. I had some meetings in Tokyo and Kyoto with the International Industrial Relations Research Institute. Bill was a freshman at Hamline, in Saint Paul, Minnesota, although he did come to visit with Hazel while I was out of the country. So, for the most part, Hazel and I were living by ourselves. Though she didn't tell me anything about it, she knew she was not well. While I was gone she checked into a hospital and learned that she had cancer.

In my ignorance I had a fine time in Japan. Yoshitaka Okada (a UW sociology PhD) met me in Tokyo, and, via bullet train, we traveled together to a continuation of the meetings in Kyoto. On the way we stopped at Nagoya to visit with Dr. Mishra, head of the United Nations Centre for Regional Development, concerning my work on regionalizing Brazil. In 1983 the Centre published the long version of the work.

Then Yoshi and I returned to the tracks to get on the next train. It was Sunday, Japan's day of weddings. During the 20 minutes or so we were there we noticed several wedding groups, all young men except for the brides. Three groups were also waiting for the next train. Then one group broke out in song, with the soprano of the bride's voice soaring beautifully above the voices of the men. We were entranced. An elderly man came up to us and asked Yoshi what I, the foreigner, was doing there. Yoshi told him I was a professor from Wisconsin and that we were on the way to Kyoto. The man said this was quite a coincidence. Following their honeymoon the newlyweds were going to Madison, where he was to enroll in the School of Engineering.

Sol Levine was also attending the meetings in Tokyo and Kyoto. He, it will be remembered, was an expert in industrial relations and had helped to establish the Japan Institute of Labor just after World War II. He was so well known and respected there that I saw the head of the Institute bow to him.

Two other things took me out of Columbus. One was a call from Vargas to meet him in New York for sessions with Martha Muse, who was the president of the Edward La Roque Tinker Foundation. They wanted me to set up a research center in Minas Gerais, Brazil. This would have required living permanently in Brazil, and we didn't want to do because it would separate us from the rest of our family.

The other thing was that Pastore phoned and asked me to grab the first plane to Madison and to meet him there. We were to do an English draft of a contract already decided upon by the Brazilians. It was to set up a program of industrial relations training at Madison for Brazilian labor people. We worked on it until around 3:00 in the morning. Brazil would pay all the expenses. Later in the morning I took the draft to Bryant Kearl, who was vice-chancellor of the UW. He was skeptical at first, thinking that it was going to cost the university a lot of money. But he was finally convinced that the Brazilians would cover all the costs. They did.

In part all this came about because earlier, on one of Pastore's visits to the University, I showed him around the Industrial Relations Research Institute's operations in the Social Science Building. Then later, Pastore got the minister of labor, Murillo Macedo, to come to Madison and asked me to give Murillo the same tour that I'd given to him. The result was that the Brazilian government gave the University a half a million dollars---lots of money back then—to run the training program for Brazilians in industrial relations. On the Brazilian side this was managed by Sandra Valle, by coincidence a friend of Cristina Haller's cousin, Fernando Rocha (UW PhD, 1968). Sandra and I managed the visits of the Brazilian labor relations people.

About this time, too, I received another extraordinary job offer, to chair the Department of Demography at Johns Hopkins University. Alex Portes was then heading the JHU Department of Sociology. He wanted me to help him merge the two departments and was in a position to get it done.

But by this time Hazel's cancer had progressed to the point where she was undergoing chemotherapy. For awhile we thought the treatment might work. But as the months passed it became clear that it would not. In the meantime I had told Saad Nagi that I couldn't keep him waiting and that he should proceed without me. Then I told Alex I couldn't accept the position at JHU.

Interlude. Having had the luck to have lived in a number of places during this long life, perhaps a word about some of them may be worthwhile. As the reader knows, I was born in San Diego, a city that sits on the coast of the blue Pacific Ocean. And the ocean and its shores have played a significant role in my life through the years. As a youth, I lived on Oahu for about a year and a half, courtesy of the US Navy. Sure, out at the Barbers Point Naval Air Station we 'airdale' sailors worked at our various jobs on the aircraft that came into our shops. But we got around the island, too. It's only the size of a small American county. We often went into Honolulu on the little steam train that was built of carry the nearby sugar cane crops to the city but which had been turned into a passenger train to carry the sailors on liberty to Honolulu. Once I had a few days liberty and took a military aircraft to the island of Hawaii. Flying toward it over the stretch of the Pacific, one could see a good many water falls from the top of the cliffs into the sea. They fell from green heights over the cliffs. Such beauty!

Many years later, Tina and I spent our honeymoon in Honolulu. We drove around the island as much as possible, seeing all the great and well known sights: Waikiki, Diamond Head, the Blow Hole, the Pali where the conquering King Kamehameha threw his rival off the cliff, falling a thousand feet or so, and Kaneohi Bay, where in my Navy time there had been another great naval airbase. We also took in the Mormons' special show on the windward side of the island. And of course we went to see the battleship Arizona that had been sunk in 1941 during the Japanese attack on Pearl Harbor.

At the risk of repeating myself, much earlier Hazel, Bill and I flew across the ocean to Sydney, Australia, stopping in mid-ocean for a few days in Fiji. As said, we lived in Canberra, across the higher lands and down the escarpment a few dozen miles to the Pacific shore at Batemans Bay. Still on the edge of the Pacific, but far south and east of my San Diego birthplace.

Repeating again, in 1981 I left Canberra for Brasília, flying first from Sydney to Aukland, there to Los Angeles, Lima, Rio de Janeiro and finally Brasília. A few days there, then back to Canberra the way I'd come. 18,000 miles: northeast to LA, south to Lima, east to Rio, north to Brasília.

Still later, Tina and I flew to Canberra from Madison, via the most unbelievable route—Madison to Detroit, then across the Arctic Pacific to Osaka and over the Indian Ocean to Sydney.

These were not the only flights over the grand ocean. On another, in 1990 I went to Dhaka in Bangladesh from Madison, San Francisco, Tokyo, and Bangkok. The trip back took nearly two days: Dhaka, Bangkok, Tokyo, Honolulu, San Francisco, Detroit, Madison.

So the Pacific Ocean, the largest geographic structure in the world, and its shores are one of my most cherished homes.

This is not to say that there aren't other lovely places far from the Pacific. For one, there is the Lake Leelanau area of Michigan, northwest of Traverse City. It and the little arts and fishing town of Leland are special. We first learned of it while I was teaching at Michigan State University. And of course there is lovely Madison, dear to my heart, with its grand University of Wisconsin. The Tucson area is the most recent of all, yet as a child and youth I lived in Phoenix, Winslow, Holbrook, Superior, Parker, and Kingman as well as in Tucson, where I attended the University of Arizona in the fall semester of 1946. And our present home in the Tucson metro area is in Oro Valley. Our backyard garden faces the beautiful Santa Catalina Mountains, rising to 9,000 feet, far above our own 2,880.

Well beyond the least of these cities is St. Paul, where Hazel and I were married, where I learned a bit about chemistry from the 3M Company's research division, and where both I and son Bill were undergraduates at Hamline University. Tina and I also lived there for two years just before our move to Oro Valley.

And there is Columbus, Ohio, where I've spent many months at one time and visited several others. As of today, we have more university friends there than in Madison.

Brazil. How many times have I been there? Seventy? In 1962 we lived in the countryside fifty miles from Rio de Janeiro, a city we often visited. Also, I lived in Sao Paulo off and on for many years, in Ilheus for months, and in Belo Horizonte from 1996 to 2000.

Important, too, are the many visits to Rio, especially where Tina's parents lived, in the lovely bairro of Ipanema.

And so the interlude ends.

1982 was the year that Bill Sewell's festschrift was published. It was edited by four of us. I have two essays in it, one on my own and one with Pastore. The first of these must be one of the most important pieces on stratification theory I ever wrote, despite the fact that hardly anyone has ever cited it. (Maybe this has something to do with the cost. The book was too pricy for most academics.)

That same year I published, in Geographic Review, the article that solved an old problem of the Brazilian geographers—what the nation's macro-regions are. Like everything else I did in or on Brazil, it was done to further my own research interests. Specifically, I needed this so such regions could be used as proxies for nations to check conjectures by Lenski and Treiman who, in different ways, held that development tends to destratify a nation. Result: Our data do not support that hypothesis.

But it turned out that the results were useful to Brazil. It had two effects. The first was that the government decided to move development efforts northwest of the State of Sao Paulo into the State of Goias. The second was that Embrapa decided to locate a new agricultural research station in the new State of Tocantins. Later on, I presented the same results in several different venues. One was at the Indian Center for Science and Technology for Development. Another was at a meeting of a UN group in Graz, Austria. The third was in a meeting in the Rockefeller Center in Bellagio, Italy. This last was chaired by Vijai Singh, one of our former students; later, Bill Haller's Major Professor at the University of Pittsburgh. The Belagio meeting had another consequence. One of the participants was an Indian named Mishra who headed the UN Center for Regional Development in Nagoya, Japan. As I mentioned earlier he wanted to publish a longer version of the regionalization of Brazil. It came out in 1983.

This experience—regionalizing Brazil—taught a lesson: you never know what the consequences of your publications may be. In truth, about 70 percent of the articles in the main sociological journals are never cited according to a University of Arizona sociologist who studied this. So cited articles are pretty rare. But of course they can have effects other than being cited by other scholars. Any interested party can use the results for their own purposes. That's what happened from the publication of the regionalization of Brazil article. But most of the time you never know who may be using your work or how it's used. It may or may not be used for the benefit of humanity.

1984-1990. 1984 was not an easy year. Hazel was suffering with the ups and downs of her cancer treatments and it was obviously a losing game. Even so I made discussions of research at Johns Hopkins University and in Chicago.

She died on February 5, 1985. For weeks before she had been wearing a wig because her treatments had cost all her hair. She was so strong in the face of all this. A week before she died she held a dinner for the Sewells.

Later on in that year I gave a talk at the AAAS meetings in Los Angeles. Also in that year several articles from my research program appeared, three of them with David Bills. One was in the Journal of Developing Areas, another in the American Journal of Sociology, and yet another in a book of the Center for Science and Technology for Development in New York. These were leftovers from work written earlier.

11. Amazonia Again.

It must have been around this time when Manoel asked me to go up into the north of Brazil to the city of Boa Vista, in the state of Roraima, to look at the Embrapa operation there. Boa Vista is about 600 miles north of Manaus and 100 or so miles down a dirt road from the border with Venezuela. It's also around 750 miles north of the equator. I was surprised to learn that the seasons change immediately from one side of the equator to the other. I made the trip by plane—paid for by Embrapa, of course. We left Manaus in the rainy season and arrived in Boa Vista in the dry season. My trip schedule called for a stay over the weekend. So the Embrapa guys I was talking with decided to take their pickup truck up to a Venezuelan border town to buy groceries and they asked me to go along. I did.

When we got to the border they wanted me to cross over with them. But I didn't have a visa for that country so I stayed on the Brazil side, right up against the border. They were gone for quite awhile. It happened that there was a little restaurant on a small hill on the Brazil side, overlooking the highway and the ramshackle port of entry. So I took a seat on its veranda and spent my time studying what I could see of the surroundings and observing the traffic coming and going between the two countries. For one thing, I noticed that there was a Brazilian Army post a little west of the highway. That makes sense; the road was the only way crooks from Venezuela could get into Brazil.

Another was a comparison of the Brazil-bound traffic with that from Venezuela. The cars with Brazilian plates were up to date and in good shape. Those with plates of Venezuela were ancient and decrepit. So was the north of Brazil richer than the south of Venezuela? Or was something else going on?

Actually, something was going on. But it had nothing to do with cars, as may be seen below.

By coincidence, while I was on the border with Venezuela there was a *coup de etat* against the President of Venezuela. If I had gone across the border, a certain University of Wisconsin professor would have disappeared and never would have been heard of again.

While at Boa Vista I got to drive around the area to see what life was like there. For one thing, around the town there were the usual Brazilian signs touting the names of politicians. And there was an Indian village away from the town. I noticed a couple of things about it. One was that it had had some modern equipment, in particular a satellite antenna for television. But the antenna was old, rusty and had long been unused. The other was that there were no men around. Where could they be? I didn't ask because it was pretty certain that no one would answer. The most likely was that they were working gold veins somewhere nearby, and of course that would be kept secret from an outsider. But it may also explain why the Brazilian pickups mentioned before were so new. The local economy was probably buoyed up by the gold workings—which, incidentally, were probably illegal.

We also took a drive in another direction, east across Apiau River. There we were able to talk with recent agricultural settlers. What we learned from them was fascinating. There were families that had been moving together in stages from far south of where we were, burning the forest they were settling, farming it for awhile, and then moving on to a new area where they did the same thing again—'slash and burn' agriculture. Interestingly they were blonds, deep in the Amazon region were everybody else was *moreno* (brown) or Indian, or a combination of the two. So where did they originate? Surely, nowhere in the north of Brazil except maybe in the backlands of states like Pernambuco or Ceará.

From 1986 to 1990 I gave papers in various places—a couple in Salt Lake City, a couple in New Dehli, one in Itabuna, Brazil, others in Recife and Rio de Janeiro (2 or 3), University of São Paulo, a school in Campo Grande (MT), the Federal University of Pernambuco (3), Quebec, Brasília (2), New Orleans, Norfolk, Curitiba, Manaus, Belém, and Rio Branco. Those in Brazil were sponsored by the Fulbright Commission.

Camila Rocha and I met in Ilheus in late 1985 and were married in 1986 and divorced the next year. This rebound marriage was a disaster.

Lucky for me, Cristina and I met in Brazil in 1988 and were married in Madison in September 16, 1989.

1991-2002. Early in this period I did some consulting for the Presidency of Brazil, and also with the Faculty of Agrarian Sciences of Pará, where Manoel Tourinho was serving as a professor. Manoel turned the Faculty into a much more important institution: The Federal Rural University of Amazonia. Then he served six years as Rector of the University. Today he and Dora live in Belém, the city where the university is located. I love the parts of Amazonia I've been lucky enough to have visited and/or flown over. And once I had an opportunity to swim in that marvelous river. Another time, Tina and made a trip from Manaus, 1,000 miles up-river from the Atlantic Ocean, to Belem. While in Manaus, we visited the famous opera house. We also crossed the immense Rio Negro near its confluence with the Solomões, where the two combine to form the Amazon River. On the other side of the Rio Negro she encountered an agricultural engineer who had studied at the Federal University of Viçosa and who knew her sister, Maria José Peloso (ZeZé.)

12. Madison, St. Paul, and Tucson.

Now for a trip like no other—standby from Madison the Dhaka and back. In 1997 Majeed Khan, an old friend, asked me to go to Bangladesh to see what the country was like and give a couple of talks. I was there for an enjoyable couple of weeks. The stay itself was fun. But the trip there and back, flying standby, was not. The trip going there: was fine—Madison to Detroit to San Francisco. The last leg of that flight arrived at San Francisco too late to make the trip across the Pacific. So I stayed overnight in a motel and left the next day. The trip to Tokyo and Bangkok was also fine, as was the last leg, to Dhaka.

The trip back was mostly a disaster. For one thing, my deteriorating right knee was extremely painful. For another, I had to stay overnight in Bangkok. This required a visa, to be paid by US dollars. So I officially entered Thailand on a tourist visa. I got three hours sleep. At six a.m. I re-entered the airport to seek out the office of Northwest Airlines and clear up my passage home. They never did open the office. Much later, they began seating passengers. My leg was so bad I could hardly walk. One of the NWA people took pity on me and put me in a wheel chair, then got me through the normal procedures and onto the plane. The flight went from there southwards down the peninsula as far as Singapore and then north to Tokyo. We arrived in the morning. There were lots of flights from there to the US. There was no place to sit down and my leg was hurting a lot. The flights were fully booked. I limped from one to another, all day long. No space available. Finally, one of the Japanese girls working the gates for NWA noticed my situation. She asked if I was willing to go to Hawaii instead of the West Coast. Hawaii was the USA. I accepted. This flight was a beauty, partly because they put me in first class. Arriving in Honolulu all the flights to the Coast were full. So I paid a bit to have the right to sleep for a few hours in a kind of cubby hole there in the airport. It, and others like it, were provided by the airport authority. Finally, a space became available on a flight to San Francisco. I grabbed it. Then, in that city, all the flights to Detroit or Minneapolis and from there to Madison were full. Finally, I was told that one seat was available to Detroit. There was a woman and her child who were also standby. She had priority over me. So I thought she would take the seat. She told me, though, that I could have it and she would remain behind with her daughter. The trip to Detroit and on to Madison was normal. But where in the world, literally, was the bag I had checked in Bangkok? To my surprise, it followed me all the way home. How it got through customs in Hawaii or San Francisco is a mystery.

In 1998, Tina was offered a position in the Twin Cities and I was offered one at the Federal University of Minas Gerais (FUMG) in Belo Horizonte, where Neuma Aguiar, and my wonderful former students, Jorge Neves and Danielle Fernandes, were teaching. We decided to do both. Tina went to the Twin Cities and I went back to Brazil. I taught there with the support of the Brazilian government for four years and Tina moved from store to store in the Cities all through those years. Actually, I visited her many times, using pass rights to fly from Belo Horizonte to Minnesota. I also quite often took a bus to Rio to visit with Tina's parents, and incidentally to get around to other old friends there.

When I returned, she quit her job in the Cities and we began our move to Tucson. I stayed in Eagan, Minnesota to sell our house and pack everything to make the move. She built the house in a suburb called Oro Valley—exactly where she wanted it and the way she wanted it, in a special part of the town called Rancho Vistoso. She chose a lot from which the beautiful Santa Catalina Mountains could be viewed from the back of the house. While that was underway, she lived in an apartment not far from there. I flew down to visit there once in a while. I'm not sure whether we drove to Tucson together or if I drove it later and flew back to the Cities. Then she took a job at a Target store as the Human Resources officer. It was far over on the east side of Madison. The store manager was incompetent. But she and the district managers understood each other. Her store manager was fired. Actually she did such a good job that as time passed, around three of four incompetent store managers were fired. After awhile she got a transfer to her store at Thornydale and Ina, eleven miles from our house.

A friend in Madison made her a present of a little dog. The catch was that I would have to pick it up deep in the center of Wisconsin. I did. Tina named her Lua, Portuguese for Moon. She was so tiny I added to her name: Lua Littlemouse. The builders finished the house in Rancho Vistoso. When the house in Eagan was sold and the truck loaded to take everything to Oro Valley, Lua and I boarded a plane and went to our new home.

Grazi, Tina's eldest, stayed in Minneapolis, where she worked and where at the end of her stay she did an MSW degree at St. Thomas University. Camila remained at the University of Wisconsin until she finished her BA, then came to live with us. Today, Grazi is a social worker in Tucson. At first Camila lived with us, then she married Burak (Brock) Bekat and did an MS in econometrics at the University of Denver. Today she and Burak live in Tucson. He and his partner, Jose, have a small architectural business. In these hard times of 2010 and 2011 it's not easy to find customers. But they're keeping their heads above water. Camila, with her econometrics background, is consulting for a woman in Holland. In January, 2011, Camila and Brock brought forth a beautiful baby girl, Mim Sofia. Tina adores being a grandmother. She calls the little one Tatuka.

My eldest, Elizabeth, did an MSW degree at the University of Wisconsin and eventually she and her man, Kunu, moved to Black River Falls, WI, where today she is the Executive Director of Social Services for the Ho-Chunk Tribe. Her daughter, Ilana, is finishing her BA at Edgewood College in Madison. Her other daughter, Marisa and her man Winston, are in Milwaukee, where he is in medical school. Son Bill finished his PhD years ago at the University of Pittsburgh with Vijai P. Singh and moved on to Princeton where he spent four years as a post doctoral student with our friend, Alejandro Portes. Today he teaches sociology at Clemson University in South Carolina.

2003-2010. For me, all went well for the first six or so years. Then I began to have health problems. A year ago, I was in pretty bad shape, though now it's passable. I stay pretty close to home and have been entertaining myself be doing things like editing *Population Review* and publishing a few pieces in it. All of my present work is on the theory of stratification.

At this time in the lives of our two families, everyone is well and happy. Long may it remain so….

….It didn't. Six months ago I got a sudden heart blockage. They opened me up and put in three bypasses. As things stand, there is reason to think everything is OK. But after all I am in my mid-80s, and people don't last very long after that age. Still, I'm happy and so is everyone else in our family. Indeed, after three months of cardiac rehabilitation and another three months of swimming, my health seems pretty good. My 'kids' and I usually see each other in Madison at the annual so-called "Haller Distinguished Lecture in Rural Sociology." We did so this May, a couple of months ago. About the same time, Bill married Donna. They had known each other when he was at the University of Pittsburgh. Stephanie and Rick, and Liz and I were all at the wedding, which was held in Pittsburgh. Fortunately, my health now seems to be pretty good.

As of today, July 1, 2011, I continue my writing and Tina continues with her job as a Human Relations Executive with Target.

As a matter of fact, I think some of my best—and hopefully most important—work has been done in the last few years.

In 1965 Bam Dev Sharda and I presented and then published a paper on the two dimensions of National Development (domestic development and international authority), which we isolated from a factor analysis of development indicator variables. It looks like this paper may, just *may*, have had a consequence or two for American foreign policy. A couple of Department of State people paid close attention to our presentation.

In 2009 I published a paper on the evolution of stratification theory from Ibn Khaldun in 1377 to today—next to Max Weber in the early 1900's, then to Sorokin in 1927, and on to Kaare Svalastoga in 1965, Otis Dudley Duncan in 1968, and myself in 1970 and 2000. That paper includes discussions of various alternative theories and of implications of the theory as I see it. One important piece is omitted from it: my MA thesis at Minnesota, where I developed the concepts of Content and Structural Dimensions of Stratification. These concepts are crucial to understanding how to think systematically about stratification. 2010 saw the publication of my The Structure of Stratification Theory, as well as one by Bill Haller and me that tested the theoretic validity of a measure of socioeconomic status. It used data from Helcio Saraiva's doctoral research in rural Minas Gerais.

Last October Manoel Tourinho and I put several theory pieces and a set of our previous publications together as a small book. Danielle Fernandes joined us as one of the authors. She is planning to hold an international symposium on stratification sometime early in 2012. She says that's when her university's press is to issue the book, and that she's arranging form some sort of award for me.

This doesn't mean that my writing days are over. For one thing, an English language book on stratification theory is nearly ready to send off to prospective publishers. Alex Portes wrote the introduction. When that's done I want to write a sociologist's interpretation of the strange society of warrior women who were encountered—and much feared—by some fifty soldiers who were accidentally broken off from Pizzaro's army in 1742.

Collaborators and Students

I believe it has already been mentioned that my greatest professional joy has always been working with our wonderful senior colleagues and graduate students. Let's look at them. Looking at these people helps to understand my own work and how it got done.

Senior Colleagues

NEUMA AGUIAR has been a friend since sometime in the early 1970s. We have never written anything together, but I did help her design a research project called the Belo Horizonte Area Survey. Also, I taught in her sociology program for four years, just a decade or so ago. She may be the most versatile research sociologist I know. She works equally well in the styles of ethnography and in those of quantitative analysis. Her specialties are women's studies, time share analysis, and stratification. Her PhD was taken at Washington University in 1969. Neuma also received the Honorary Degree of Doctor of Science from Wisconsin.

SUBBAIAH ANANDAKRISHNA does research in science and technology in India. We met around 1996, when I gave a couple of talks at the Indian Institute for Science and Technology for Development in Delhi. He has since moved to the city of Chennoi, in the southeast of India. I have lost contact with him.

JOHN CARY is an Australian. His research uses Joe Woelfel's galileo system to study the behavior of farmers. His PhD was taken in the field of psychology at Melbourne University.

JOSEFA SALETE BARROSA CAVALCANTI. Salete is also my gateway to lots of friends in the Federal University of Pernambuco—Heraldo and Rosália Soutomaior, Jorge Jatubá, Alexandrina Sobreira, Silky Weber, and others. Salete did her recent field work on the small farmers in the irrigated areas near the São Francisco River. It should be noted that her PhD was from Manchester University in the UK and that she is well known in France. She spent a year with our group in Madison, and also visited us here in Tucson when she was on a Fulbright in Houston, Texas.

LELAND COOPER (MA. University of Minnesota). Leland was an anthropologist, and the most erudite person I have ever known. Together we traveled all over Minnesota, Leland lecturing all the time. I was lucky to have also served as his assistant in the laboratory he ran at Hamline. We became life-long friends. As the years past, he became well known in the Twin Cities and nationally and was able to obtain grants from the National Science Foundation to carry out excavations in Minnesota. He had studied at the University of Wisconsin and while there served as a teaching assistant for the famous Ralph Linton. Wisconsin's anthropology group gave him a medal for his work. He died several years ago.

OTIS DUDLEY DUNCAN (PhD, Chicago, c1949). Dudley and I had some exchanges in the 1950s and 1960s over similar research concerns. Along with Alex Portes, we published an article that used my Lenawee, Michigan data from the 1950s. It turned out to be quite influential, and among other things, was cited in a book of research methods in 1971 and is still being used by quantitative research methods teachers today.

MARIAH EVANS (PhD, University of Chicago). Mariah worked with Hélcio Saraiva and I at Madison. She and Hélcio published an article from our research program in a British journal of sociology. Today she teaches at the University of Nevada in Reno.

PAULINE FOSTER (PhD, Warwick University in England). Pauline spent a year under my supervision at the University of Wisconsin while she was doing her field research in Milwaukee. After taking her degree, she moved to that area, and set up a company to help people who were jobless due to the changes in the city's economy.

RUTH GASSON (PhD, Cambridge University). Ruth spent a year working with our group back in the early 1970s. With William Sewell, she and I published a monograph on attitudes and facilitation in status attainment processes.

WILLIAM JOHN HALLER (PhD, University of Pittsburgh). It may seem strange that my son should be included in this group. But in fact we collaborate on research. He and another of our closest friends and colleagues, Alejandro Portes, do quite a lot of work together on migrants in the Miami area and in Spain. Indeed, Bill spent four years as a post doctoral researcher with Alex at Princeton before going to Clemson where he is now Assistant Professor of Sociology.

LI KANG (PhD, University of Peking, Beijing, PRC). was Fei Xiao Tung's doctoral student. His research is on rural China. Li and Bill Haller first met in Beijing when Bill went to see Fei, carrying a letter of introduction from Bill Sewell (see Sewell, below). Li spent a year with our group at Madison.

MAJEED KHAN (PhD, University of Minnesota). Majeed attended Hamline as well as Cambridge University as an undergraduate. (Bill and I also did undergraduate studies at Hamline.) At Minnesota, Majeed worked with the famous rural sociologist, Lowry Nelson. He held a number of important national and international posts after returning to his native Bangladesh. At one time, he was Ambassador to France and to Unesco. In his home country he has served as Minister of Science and Technology and Minister of Education. He has also founded two universities. One is a school of business, the other a school of science and technology. Majeed spent one year working with us at Madison. While there, he had the opportunity to meet with Bill Clinton when the latter was president. He and Bill Haller presented a paper at meetings in Washington, D. C., during his stay with us. I once visited him in Dhaka.

JONATHAN KELLEY (PhD, University of California at Berkeley). Kelley and I have collaborated off and on since 1974. It all began when Pastore (see *Graduate students)* and I were looking for someone to join us in planning the research that Pastore was to use as his *livre docente* thesis, required for admission to the University of Sao Paulo's faculty of economics. I spent six months in Canberra with him in 1981. As this is being written, I believe an article of ours, with Bill Haller, is under review at the *American Journal of Sociology*.

SOLOMON B. LEVINE (PhD, MIT). Sol was a close friend and collaborator for many years. He was the leading expert on labor relations in Japan, and his advice was sought, not only by American scholars but also by Japanese leaders. I have seen with my own eyes the respect in which he was held by top officials in that nation. Among other things, he helped found the Japan Institute of Labor. (For much more information about him, see Wikipedia, Solomon B. Levine, University of Wisconsin.)

JOHN McNELLY (PhD, Michigan State University c1966). John had been a newspaper reporter in Britain during WWII, and he met his wife, Pamela, while there. Later, he established a couple of local newspapers in California. As a student at the University of Wisconsin in Madison, the city where he grew up, he had been editor of the student newspaper called the *Cardinal*. After his stints in England and in the West, he returned to Madison to do an MA in Journalism. But then his professor moved to Michigan State to help found the first Department of Communication. John went with him, eventually to become the first PhD of the Department. I was on his doctoral committee, and in the small city of East Lansing the McNellys lived a block or two away from our house. Our kids were about the same age and they played together. Later on, John took a position with and MSU project in Latin America. This set him on a career as a specialist in his field in Spanish America. After a couple of years at MSU he was invited to return the Wisconsin as Professor of Journalism. From then on until his retirement we often taught seminars in Latin America together. He attracted quite a number of outstanding journalists from all over South America, at least one of whom is still contacting me with messages to send on to John, who doesn't use email. We still talk to each other over the phone now and then.

SAAD NAGI (PhD, Ohio State University). Saad has been a close friend for many years, beginning in 1982, when I was at OSU as Distinguished Professor of Rural Sociology. He is an Egyptian, and is married to an American named Kay. During our months there, he was the Chair of the Department of Sociology. He and I had many discussions about the future of the department, and it was obvious that he had the authority to improve it. I thought it was focused too much on applied work and needed to add strength in the basics. My suggestions were social psychology and stratification. Then he made me the incredible offer of hiring six new professors of my choosing. But it was then that we learned that Hazel's cancer was progressing. So I turned it down. Today, I sense that Saad's health is not the best and neither is Kay's. I believe he is 86 or so. But he is still working… writing and maintaining contacts with colleagues.

WILLIAM HAMILTON SEWELL, Sr. (PhD, University of Minnesota, 1940). Bill was my Major Professor at the University of Wisconsin, and a life-long friend. For at least 20 years he and I team-taught the stratification seminar at the UW. Also, we published several research articles together between 1952 and 1970. Two of them (1956 and 1959) changed the way researchers and the public understood the effect of the status of a child's parents on the personality of the child. Another (1957, with Murray A. Straus) cleared up understanding of the relations among status, IQ and the educational and occupational aspirations of youths. Still others (one with Alex Portes, the other with George Ohlendorf) launched and set the standard for many projects by others on the same topic. Each has been republished by others, the one with Alex quite a few times, including abroad, and was still being republished as late as 2007. Except for a couple articles in *Rural Sociology* in 1952, each of these was republished several times. One of ours (1966), together with some that Joe Woelfel, Ed Fink and I did later, popularized the term 'significant other'. It had been around for a couple of decades but until we started using it, was just part of the jargon of sociologists. Also, our work on both personality and aspirations was picked up and used by the public education community. Finally, it must be added that Nobel Peace Laureate, Alva Myrdal, sat in on our stratification seminar during a semester that her husband, Gunnar (Nobel Laureate in Economics) spent at the University of Wisconsin.

JOSE ISRAEL VARGAS (PhD, Cambridge University, 1959). Vargas is one of the movers and shakers of the world. For years he was the Chairman of Unesco's executive commission. He has also served as Minister of Science and Technology in Brazil, as well as the nation's Secretary of Industrial Development. Vargas and

I became acquainted at the suggestion of Hélcio Saraiva (see below). At the time, Hélcio was serving as a director of the Joao Pinheiro Foundation, then a physical research agency headed by Vargas. I invited him to give a talk at Wisconsin. I gave him a ride around the city, and we talked. Among other things I mentioned a book by Joao Guimarais Rosa that I thought to be excellent. Vargas told me Rosa was his cousin. We became life-long friends and have kept in contact ever since. Today he works at the Rio de Janeiro Physics Institute.

Major Professor of Graduate Students at the University of Wisconsin.

ILYA ADLER (PhD 1985). Doctoral research on media use in Mexico. Ilya was joint with John McNelly and myself. The last I heard he was doing research in Mexico.

MIRIAM ROSA BENSMAN (PhD c1975). Miriam did her doctoral work on a problem in the psychology of status attainment. When she finished she moved to Baton Rouge, Louisiana, with her husband. The last I heard she was working for the State as a demographer.

JAMES W. CONVERSE (PhD 1969). Jim's doctoral research was on the measurement of anomie and alienation in Brazil, using data collected by Saraiva. On completing his degree, he accepted a USAID/UW position in Rio Grande do Sul, Brazil. After this, he spent a year at another Brazilian university, and then went to Cornell University as an assistant professor. If memory serves, he was fired for advocating the return of the Cornell's campus land to the Oneida Tribe. I saw him only once thereafter, when he and his children were passing through Madison. Actually, his advisor was Havens until Gene's death.

JOSE A. L. DRUMMOND (Phd 1969). Jose did his doctoral research on the relation between an American mine and its treatment of the natural environment in the far northeastern state of Amapá, Brazil. The state is one of the most isolated in the nation. It's bounded on the east by the Atlantic Ocean, on the northeast by Guiana and Surinam, on the southeast by the Amazon River, and on the west By the Jarì River. His work destroyed the myth promulgated by some people in Amazonia that held that everything the Americans did worked to the detriment of Brazil. In fact, he found that the Company and the Brazilian Government had drawn up a contract to do the mining for 30 years and then to leave the site in good condition, which was exactly what was done.

Today, José is probably the leading environmental analyst in Brazil. He teaches at the University of Brasìlia. He writes and publishes a constant stream of books and articles….So how did it happen that I became his major professor? He was a student with the environment group. I guess it was because they needed to have someone who knew something about Brazil.

SUZANNE DVORAK (PhD 1989). Suzanne is a Texan who did her undergraduate work at Texas A & M University in sociology. When she arrived at Wisconsin she asked me if she could be my 'gopher' (go for)—run errands. So she joined our research group. She was fluent in Spanish and was studying Portuguese. Hélcio Saraiva and I wanted her to get acquainted with Brazil, so we had her accompany us on a trip to Brasília. When she was ready she used our data for her doctoral thesis on the situation of single mothers in Brazil. This was an important work but as far as I know it was never used by anyone….I hope I'm wrong. For reasons I don't understand she had trouble getting a professorship. Eventually, she went to New Mexico State University as a lecturer, a job she kept for only a couple of years. Then she took a job with the Department of Education in Dallas, and later moved to Houston. That was the last I heard of her.

DANIELLE C. FERNANDES (PhD 1999). Danielle did her doctoral research on race, socioeconomic status, and educational attainment in Brazil. Then she returned to her birthplace in Recife, along with her husband, Jorge Neves (see below). She couldn't get a decent job there, one that could use her expertise. Neither could Jorge, though his was quite a bit better than hers. Then it happened that a position came open in Minas Gerais, at a time when Neuma Aguiar was there as the chief sociologist. Danielle took the required '*concurso*' in competition with a University of Michigan sociology PhD named Letícia. They both passed, but Danielle was rated the best by each of the five evaluation committee members. (I know because I was teaching there at the time and was one of the five.) So she settled into Belo Horizonte while Jorge remained in Recife in the Department of Economics of the Federal University of Pernambuco. Then Danielle submitted a shorter version of her doctoral thesis in a competition with IPEA, a federal group, and won the first prize, the equivalent of $10,000. Later, she gave a lecture at Stanford on her doctoral thesis research. It spurred some Americans to look into the same phenomena she had discovered for Brazil—that the educational levels attained by blacks were lower at all points along the educational hierarchy. So she became well known among American sociologists. Just recently she was invited to become an associate editor of an important journal of sociology in the US. In the meantime, the Federal Universities of Minas Gerais and Pernambuco came to an agreement by which Jorge was able to join Danielle and Neuma at their university. Today Danielle is one of Brazil's leading sociologists.

EDWARD L. FINK (PhD 1975). Ed came to our projects in 1966, soon after I returned to Madison. Joe Woelfel (see below) and I were looking for someone to be a research assistant on our 'Significant Other' (SO) project, in which we were trying to learn exactly what SOs were and how to identify them and measure their influence. The Sociology department had admitted many more new graduate students than they could support. So Joe and I went through the records and found about Ed. He had an excellent preparation, math through calculus, and top grades from one of the toughest sociology departments (Columbia). He joined our group.

At this point it must be added that in those years I was blessed with the largest research group I ever had, and it was one of the very best. It included Alex Portes and Hélcio Saraiva (see below for both), as well as Joe. I was major professor for Alex, Hélcio and Ed. Joe's major professor was N. Jay Demerath, III. Joe was the senior of our group. Dave Hansen (see below) was also there, working with Hélcio. Anyway, we did the project and published the results. I gave a paper on it in Rome, in meetings organized by Ed Borgatta.

Ed Fink did his doctoral thesis on vicarious embarrassment and took a job at Notre Dame University. After two years he left Notre Dame. Then Ed went to the Department of Communication at Michigan State, in which I had been involved earlier. He was there 1973-1981. The faculty had given up doing serious research and just did simple things to satisfy mass media customers. He and Joe Woelfel, who was there with him, were teaching modern statistics and sociological theory to the graduate students, and the other faculty members didn't like it. It was not a happy place; soon he, Joe, and Cushman left.

Finally, Ed got a job at the University of Maryland, where he eventually became the Chair of the Department of Communication, and for a while, associate dean of the graduate school. He built the department and raised its quality substantially. He continued his research, making important contributions to communication theory by means of carefully designed experiments and developing sophisticated mathematical models of communication behavior.

DAVID O. HANSEN (PhD 1972). Dave was one of the people in that first (1965-1970) group mentioned before. One day he asked me if he could work with me. By then I was out of money and I told him so. He replied that the money was not a problem. So he went to work with Hélcio. This made a lot of good sense. He is married to a lovely Mexican named Aida and he himself had been in the Peace Corps in Bolìvia for a couple of years. So working on Brazil research with Hèlcio fit well into his experience and interests. When he finished his 'prelims'—final written and oral examinations for the PhD—he accepted a job replacing Jim Converse at the University of Rio Grande do Sul (RGS) and stayed there for four years. He made the Wisconsin contribution to RGS hum. Then he went to the Piracicaba branch of the University of São Paulo where he worked on the Ohio State University project. After two years there he moved to Rio de Janeiro, where he teamed up with Dr. Eduardo King Carr, a Brazilian geneticist, to develop a program to improve the second tier agriculture colleges. From there he went to Ohio State and joined the Department of Rural Sociology and the Department of Sociology. After awhile he was asked to serve as the Associate Dean of International Agriculture. Again, he did a superb job building relations with such colleges in developing nations. Then too he was responsible for offering me the position of Distinguished Professor of Rural Sociology, which is how we came to be living there in 1982-1983. OSU had and still has a number of our good friends, besides Dave and Aida the Wus, the Nagis, and the Flinns. Much later, Dave more than anyone else was the driving force behind the honorary degree OSU gave me in 2007. In the last few years Dave had been in Washington D. C. developing a program to help African universities. His doctoral thesis used data he collected in Rio Grande do Sul to show the relationship between status and land tenure.

WAYNE THOMAS HASSL (PhD 1989). Wayne did his doctorate in the UW school of Education. I was his co-supervisor because his research was on the role structure of school principals, teachers, superintendents and board members, a subject sociologists had worked on earlier. I have no additional information about him.

LYN HIRSCHKIND (PhD 1980). I was also co-supervisor of Lyn's doctoral work. Her Field is anthropology. She did her research on development in Cuenca, Ecuador. When she finished her degree she moved to Ecuador and bought a hacienda.

CHANOCK JACOBSEN (PhD 1969). Harry, as Americans called him, is an Israeli born Germany, sent to Britain, and eventually migrated to Israel. His research grew out of a fear that Israel could break up if the pressure on her were to be released. Specifically, he believed that differences in norms and values among Israelis would be the force that would cause the rift he expected. But there were no Israeli data to use for this purpose. So he found some appropriate data on Christian denominations in the US. His thesis was entitled *Secularity and Sacred Norms.*

SÍLVIO MARCELO MANANHÃO (PhD 1976; deceased). Sílvio did his thesis on development and dependency in Brazil's Northeast. We first met when, in 1968, three of us UW professors took a group of 10 graduate students to Brazil's North East. For while he headed Sociology at the Federal University of Pernambuco. Later he was named Vice Rector of the University.

JORGE ALEXANDRE BARROSO NEVES (PhD 1997). Jorge's doctoral research was on the earnings of different classes of farm personnel as a function of education and experience. He found substantial effects of both. His work was ground-breaking in Brazil because the only other publication on the matter, by a UW agricultural economist, claimed that education had no effect at all, a report that fit widespread beliefs of the Brazilian people. A version of his work was published in the Haller *festschrift* edited by Dave Bills (2005. See below for Bills.) Its effects in Brazil are unknown. My guess is that that they simply melted into the common understanding.

YOSHITAKA OKADA (PhD 1981). I was co-supervisor of his research. Yoshi did his field work in Indonesia, on the structure of multinationals in Indonesia. A fellowship to study Indonesian at the East-West Center in Hawaii was arranged for him by Professor Solomon Levine (see Levine, above). His main position in Japan was at Sophia University. He has become a world leader on research on the organization of business firms. Today he also holds a position at a Max Plank Institute in Germany. (For further information, check his work on Google.)

SYAD PASHA (PhD 1988). Research on media use in Trinidad. He had taught in Saudi Arabia and grew up as a Muslim in India but for some reason was unwelcome there. Then he taught for awhile in one of the State Universities of upstate New York.

ALEJANDRO PORTES (PhD 1970). Alex did his doctoral research on the political attitudes of Santiago, Chile's shanty town people. The research was quite difficult, because the students and their professor, Hugo Zimmelman, tried to undermine his work. Fortunately, the dean was an old friend of mine and he helped Alex. Even so, the student interviewers had a plot to take their pay and burn the interview schedules. In this they were egged on and abetted by a Chileano student in Madison who accused him of being a spy for the CIA. But the friend mentioned above, Danilo Salcedo, the dean, said that if I vouched for Alex, he would he would support him. I told Danilo that I had known Alex for five years and had absolutely no reason to think he was anything but what he said he was. In the end, Alex photo micro-copied all his interviews and brought them back to Madison, where he wrote his thesis. As of the 1990s, one of his articles from that study was still being used in Chile.

He and I did an article with William H. Sewell, published in the *American Sociological Review*, that was republished many times, most recently in 2007. He and I published another, in *Sociology of Education*, that also made quite a splash. Just this January (2010) I was asked for permission to republish it. Also, he and I did one with Otis Dudley Duncan, in the *American Journal of Sociology*, which has been cited hundreds of times.

His first job was at the University of Illinois. Then he went to the University of Texas, and from there to Duke University, then Johns Hopkins, and now he has an endowed chair at Princeton.

Today, he and Bill Haller are working together on what happens to the second generation of migrants in Miami and in Catalonia (Spain).

Alex has also served as President of the American Sociological Association, the Latin American Research Association, and various other academic groups.

JOSE PASTORE. Actually, Jose was the advisee of another professor. He is included here because he planned his famous mobility research in my seminar and because the two of us did a lot of work together. Jose is one of the most influential Brazilians of our group. In 1970 the nation's government decided to centralize agricultural production. The mechanism they chose to do it came to be called Embrapa: The Agricultural Research Enterprise. A group of a dozen or so was put together to design it. I have been told by someone who was there that José was the leading designer. But that was a few years after he finished his doctoral thesis and returned to Brazil. That was in 1969, I believe. In 1969 no one in the country had any idea what a well trained sociology PhD could do. So he found himself running from campus to campus in São Paulo, teaching first-year students. A year or so later, the Ford Foundation gave him a huge grant for research. That's when he began his ground breaking studies on social mobility. This enabled him to gain a professorship in the University of São Paulo. This was at a time when the economists and he had a great deal of influence in the government. He was invited by President Geisel to be Minister of Labor but turned it down so he could keep his professorship. But he led the work of the ministry anyway, with the help of Fernando Rocha, Renato Lopes, Jorge Jatubà, and me—all connected with the University of Wisconsin's Department of Rural Sociology.

TARCÍZIO REGO QUIRINO (PhD 1974). Tarcízio did his doctoral thesis on the absorption of university educated personnel in São Paulo's industry. He then went back to his home town of Recife but found that he had no chance of employment there (envy?). So Saraiva (see below) and Vargas (see at Senior Colleagues) arranged a position for him at the João Pinheiro Foundation, in Belo Horizonte, where he published original research on manpower in Minas Gerais. Then he took a job at Embrapa, Brazil's agricultural research agency, where he became chief of strategic planning. (He was the one who decided to use my work on regionalization to decide to develop a research unit in the new state of Tocantins.) Then he wrote a book on agriculture and the environment that won the year's prize as the best book on science. Today, now that he has retired, he lives near the city of Campinas and continues his studies of farming and its environmental consequences.

DAVID RADIN (PhD 1973). Dave did his research on the relation between prejudice and hostile behavior. He used formal experiments to test his hypotheses. On finishing his thesis he took a job at the University of Indiana/Purdue campus in Calumet, Indiana, just south of Chicago. He may have retired by now.

JOSE BOLIVAR VIERA DA ROCHA (PhD 1989). Bolivar's doctoral research was on automization of Brazil's banks and the effects on bank employees. For years he taught in Brazil's Northeast. His whereabouts today are unknown.

MIRIAM BENSMAN (PhD About 1975). Miriam did her thesis as a test of a psychological variable. The last I heard of her she was working for the government of State of Louisiana.

HELCÍO ULHÔA SARAIVA (PhD 1969). Hélcio was one of that first (1965 -1972 or so) group of wonderful graduate students who were working with me: Hélcio on his doctoral project, with Dave Hansen (see above) helping him, Alex Portes on the status attainment processes (above), and Joe Woelfel (below) and Ed Fink (above) on the identification of significant others and the measurement of their influence on the youth. Hélcio's PhD thesis research was done in an isolated region of Minas Gerais. As of today, it is still one of the best pieces of research ever done on the nature and form of stratification systems. He and I published the research in *Rural Sociology* and then, together with Donald Holsinger (one of my masters students), published another in *The American Journal of Sociology.* This last used data from both Hélcio's project and from the program our group carried out in Pernambuco in 1968. In later years Hélcio built the Federal University of Piauí, served as president of the association of federal university presidents, was chief of cabinet of the Ministry of Education, then planned and carried out the first national survey of educational behavior. When this was finished he spent one year at UCLA, then two years as Visiting Professor of Rural Sociology at Madison, where we two taught a seminar together. When he returned to Brazil, he took over the job of CEO of the nation's largest academic funding agency. Then, sadly, he died. He is much missed by his friends, myself, Neuma Aguiar, Jose Israel Vargas, and José Pastore, among others.

VIJAI P. SINGH (PhD 1970). Vijai did his field research on the structure of jatas ('castes') in three Indian villages. He brought the data with him when he came to Madison to do his doctoral studies. On finishing his degree he took a post doctoral position at Cornell University for a year. During this time he prepared his thesis as a book, one that was well considered by American sociologists. Then he took a professorship at the University of Pittsburgh, where he has been ever since. Today he is Professor of Sociology and Director of a research center at the university. He has also serves as Vice Chancellor or maybe Associate Vice Chancellor, of the university. He is an indefatigable researcher. In recent years he has opened up research on bi-national business firms—at least as I understand it. He was Bill Haller's Major Professor at Pitt, and Bill ran the research center for him for quite a few years. The two have published at least one journal article and three book chapters together. (I was Vijai's co-Major Professor, along with Joe Elder.)

KENNETH I. SPENNER (PhD 1977) (Co-Major Professor with David L. Featherman). Ken had studied with Ed Fink at Notre Dame University, who suggested that he should come to Madison and work with our group. We published one or two things together and still keep in contact with each other. He is now the Chair of Duke University's Department of Sociology and Professor of Sociology and Psychology.

MIN CHIEH TSING (PhD 1997). Min Chieh did his thesis on earnings of the Taiwan labor force. I visited with him in Taipei a little later. He was a professor of social work at the University of Taiwan. In addition he had founded a foundation in support of children like his own who had a certain disease.

JOE WOELFEL (PhD 1968). Joe could just as easily be included among the senior scholars. He worked with Ed Borgatta and was N. Jay Demerath, III's doctoral student. He and I have done a lot of work together. Joe is one of the most creative people I have ever met. His 'galileo' system provides a way to measure and influence the cognitive distances between any pair of concepts, such as Clinton and Reagan, for example. In recent years he has been working on neural networks as a metaphor (?) of global communication. I confess I don't understand it. But he says so, so that's the way it is. For reasons that mystify me he is putting everything I ever wrote on the internet.

Other Researchers with Our Wisconsin Group.

DAVID B. BILLS. Dave and I did a lot of work together, and including several joint publications from the end of the 1970s to the middle of the 1980s. Dave is now Professor of Education and Sociology and Associate Dean of Education at the University of Iowa. He is also the editor of *Sociology of Education*.

ROBERT MEIER. Meier worked with our group in the 1970s or so. He was co-author of an article we published in one of the journals. Too bad, that piece has disappeared.

LUTHER B. OTTO. Lu worked on the longitudinal project I had begun 15 years earlier at Michigan State University. He has taught at Washington State University and at North Carolina State University. At NCSU he held an unusually important professorship. As I understand it he is consulting for the Navy these days.

Archibald Orben Haller, Jr. | Tucson, Arizona
July 1, 2011

* all that's been said is from memory and I hope any errors will be forgiven.