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*Community Studies and Research for Change:
An Oral History with William Friedland*

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Edited by
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Table of Contents

Introduction	i
Early Life and Family Background	1
Wagner College	15
Joining the Shachtmanite Trotskyists	17
Becoming a Labor Organizer in Detroit	19
Moving Away from Trotskyism	31
A Brief Period in Europe	45
Becoming an Academic	50
Graduate Work at UC Berkeley	52
A Ford Fellowship in Tanganyika	56
Cornell University and the Cornell Migrant Labor Project	67
Coming to the University of California, Santa Cruz	78
Founding Community Studies at UC Santa Cruz	80
Studying the Social Impact of the Mechanized Harvesting of Tomatoes	90
More on Community Studies	107
Other Research Interests: Agricultural Mechanization of Iceberg Lettuce	112
Rural Sociology	114
Integrating Academic Work and Activism	122
Alternative Agrifood Researchers Without Borders	129
Research on the California Wine Industry	139
Center for Agroecology and Sustainable Food Systems	144
More on Academic Activism	150
Appendix I: A Historical Note on the Evolution and Demise of Community Studies	157
Appendix II: An Introduction to William Friedland by Professor Michael Cowan	163

Introduction

After working in the Detroit auto industry and union organizing for twelve years, William Friedland turned to a research and teaching career in sociology, ultimately becoming one of the founders of the sociology of agriculture movement. Friedland came to the University of California, Santa Cruz, in 1969 from Cornell University, where he had created the Cornell Migrant Health Project, a field study program in which he and his research associate, Dorothy Nelkin, sent Cornell undergraduates to work undercover as laborers in the agricultural fields of upstate New York and assist with research on the sociology of migrant labor.

Friedland built on his Cornell experience when he was hired by UC Santa Cruz to establish the Board of Community Studies, an innovative, interdisciplinary academic program that integrated scholarship and community engagement in both research and teaching and that graduated over 2000 majors until it was suspended in 2010, a decision that was greeted with much political controversy.¹

The centerpiece of community studies was its field program, which offered undergraduate students six-month field placements in community

¹ For more on the controversy over the suspension of the community studies major at UC Santa Cruz see "Questions Remain About UCSC's Suspension of Community Studies Department" Santa Cruz Patch <http://santacruz.patch.com/articles/questions-remain-about-ucscs-suspension-of-community-studies-department>; "UCSC Community Studies Program on the Chopping Block," Elizabeth Limbach, April 21, 2009 <http://sixties-1.blogspot.com/2009/04/ucscs-community-studies-program-on.html>; "Community Studies at UC Santa Cruz to be Eliminated," Michael Rotkin, June 3, 2009, Beyond Chron: The Voice of the Rest <http://www.beyondchron.org/news/index.php?itemid=6990>

organizations, training them to be community organizers and preparing them for what often became careers in public service. Michael Rotkin directed the field program for many years; an oral history with him is forthcoming in late 2013. Unlike other college-based field-study programs, the community studies curriculum—emulating the model that Friedland had innovated at Cornell—required preparatory training before the field study as well as synthetic and analytical work following completion of fieldwork. As Friedland’s colleague, Michael Cowan, remarks in the introduction published in the appendix to this volume: “The impact of such educational experiences have often been profound and lasting . . . many community studies graduates currently serve as directors and key staff of social service non-profits and governmental agencies throughout and beyond the borders of California.” In this oral history conducted by Sarah Rabkin, Bill Friedland describes the evolution both of the community studies program and of his sociological research.

William Friedland was born May 27, 1923, in Staten Island, New York, to Russian Jewish immigrant parents whose first language was Yiddish. He attended Wagner College, a small, Lutheran denominational college on Staten Island, where he was inspired by a fellow student to join the Shachtmanites, a branch of the American Trotskyist party, which called themselves the Workers Party and were critical of the Soviet Union. Friedland and his friend started a Shachtmanite group on Staten Island. This trajectory ultimately led to Friedland’s move to Detroit, where he got a job at Hudson Motor and became a union organizer for the next twelve years.

By the mid-1950s, Friedland was becoming increasingly disillusioned with the politics of the left and decided to pursue a career in academic sociology.

Finishing his undergraduate work at Wayne State University, Friedland then went to UC Berkeley and earned his Ph.D. in sociology. For his dissertation research, he spent sixteen months in Tanganyika, studying class relationships in the African trade union movement. While still in an all-but-dissertation status, Friedland was offered a job at Cornell University's School of Industrial and Labor Relations.

The political landscape of universities in the United States began to shift with the advent of the Free Speech Movement at UC Berkeley in 1964, and Cornell University students were soon also swept into the emerging activism of the 1960s. They demanded socially relevant courses from their professors, including Friedland, who responded by starting the Cornell Migrant Labor Project described above.

Much of this oral history is devoted to Friedland's narrations about his research. His groundbreaking research into the social impacts of tomato harvest mechanization served as a basis for a lawsuit that California Rural Legal Assistance brought against the University of California in 1979, claiming that the agricultural mechanization developed by UC researchers at the University of California, Davis and paid for with taxpayer money had a negative social impact on small tomato growers and tomato workers in California.² The suit contributed to a rising tide of consciousness about the relationship between corporate-funded

² This case became known as the Research Priorities (or "Tomato Harvester" or "Mechanization") case. CRLA won the case in 1986, but later lost on appeal to the California Supreme Court in 1989. Nonetheless, in response to the suit, the University of California creates the Small Farm Center, the Fair Political Practices Commission requires that professors reveal personal financial interests that may involve conflict of interest with their research, and some funding is given to UC Santa Cruz for the Agroecology Program. See the oral history with Professor Stephen Gliessman conducted by the Regional History Project for the *Cultivating a Movement: An Oral History Series on Sustainable Agriculture and Organic Farming on California's Central Coast* for more on the impact of this case on UCSC's Center for Agroecology and Sustainable Food Systems. Available in full text online at: <http://library.ucsc.edu/reg-hist/cultiv/gliessman>

agribusiness research at the University of California and deteriorating conditions for workers and small farmers in the state.

Friedland retired from teaching in 1991 but continued his career at UC Santa Cruz as a research professor. He discovered an intellectual home in rural sociology, completing a study of the agricultural mechanization of iceberg lettuce and pioneering what is now known as food systems research. In 2005, Friedland received a lifetime achievement award from the Rural Sociological Society. Doug Constance, professor of sociology at Sam Houston State University said on that occasion: "In a tribal sense, Bill is one of the elders of the sociology of agriculture movement. His contribution was to look at the people in agriculture—the farmers and the farmworkers—at a time when power was being concentrated in global agricultural corporations. Everyone accepted that bigger was better until Bill came along and asked, 'Better for whom?'"³ In 2012, Friedland was again honored as by the Rural Sociological Society, this time as a Distinguished Rural Sociologist. The 13th World Congress of Rural Sociology also designated a plenary session in his honor. His research continues and he is now engaged in a major research project on the California wine industry.

Friedland concludes the oral history with some thoughts on how he has personally wrestled with integrating his life in the academy with activism and the prospects for what he calls researcher-activists. Part of his response has been to found (in his eighties) the organization Alternative Agrifood Researchers Without Borders, which matches graduate students with faculty engaged in food systems research at institutions around the world.

³ See "UCSC Professor William Friedland Honored for Lifetime Achievement," Jennifer McNulty, August 8, 2005 <http://news.ucsc.edu/2005/08/721.html>

This oral history was conducted by Sarah Rabkin in three sessions in November 2012 at William Friedland's office at College Eight on the University of California, Santa Cruz campus. The interviews were transcribed verbatim. Friedland carefully reviewed the transcript for accuracy and also wrote a number of extensive footnotes which appear throughout this volume. We thank him for the generosity he brought to this endeavor in the midst of his research. The volume was transcribed by Sarah Rabkin, and edited by Sarah Rabkin and Irene Reti, director of the Regional History Project.

Copies of this volume are on deposit in Special Collections and in the circulating stacks at the UCSC Library, as well as on the library's website. The Regional History Project is supported administratively by Elisabeth Remak-Honnef, Head of Special Collections and Archives, and University Librarian, Virginia Steel.

—*Irene Reti*

Director, Regional History Project, University Library

University of California, Santa Cruz, June 2013

Early Life and Family Background

Sarah Rabkin: This is Sarah Rabkin, and I'm with Bill Friedland in his office at College Eight [at UC Santa Cruz]. It is November 12th, 2012 and we are here for our first interview session. So, Bill, I will start with the question that opens every one of these oral history interviews: When and where were you born?

Friedland: I was born on Staten Island, New York—that's the rural borough of New York City—May 27, 1923.

Rabkin: And where did you grow up?

Friedland: I grew up mainly in Staten Island, because I was there until I left on my own to go to Detroit in 1941.

Rabkin: Can you tell me a little bit about your parents and your family background?

Friedland: Yeah. My parents were both Russian Jewish by background. My father came individually, but in association with two brothers. But I think he came by himself. My mother came as a member of a family. She was then a young woman, about, I think, sixteen or seventeen, when, with her parents, she came. She already had an older brother who had moved from—this was, let's see, Belarus, I do believe it was. They lived in the outskirts of Minsk. So she

came, essentially, as part of a family unit; my father came individually. And that would have been before World War I. I tried at one point to see if I could dig out more information, either genealogically or about emigration, but I did not succeed.

Rabkin: Tell me a little about the work your parents did.

Friedland: Well, my father was a small businessman. He was a wholesale confectioner, so he distributed candy to candy stores on Staten Island. And there's a good story there, by the way. He was originally a plasterer when he came to the United States. Those were the days when they put laths on walls, and then they plastered wet plaster to and through them. And that was not the kind of work he enjoyed, so he accumulated some money and he bought a candy business. And the person he bought it from explained the business. This would have been, I'm pretty sure, before World War I, because all of the family were essentially living in the house on Staten Island, on Victory Boulevard, and that's where I was—I was born into it; I don't know if I was born at home or in the hospital. When my father bought the candy business, the way he delivered the candy, and got around, was with a horse and wagon. And he did not remember to, in effect, get the route—how do you get to the various candy stores? But the horse knew. (laughs) So when he took the horse out for the first trip, the horse just went around the whole trip, which would have been the northern half of Staten Island. Which is a fairly sizeable area. The horse was quite knowledgeable, and my father always loved to tell that story.

Anyhow, both of them were what I later would call Roosevelt socialists. My father was attached to the Bund, the Jewish socialist organization, in what was then Russia. And my mother fell in with a band of Bolsheviks, that was to say, the majority of the social democratic organization in Russia—most of which was underground. And as a young girl, she knew her way over the rooftops. So she at various times smuggled propaganda (laughs) for the Bolshevik grouping. That was before she had any kind of sense of politics herself. She was not really a very political person.

But my father tended his business. They were both members of the Workmen's Circle—a Jewish social democratic fraternal organization in the United States. And both of them essentially were Roosevelt socialists. That is to say, they thought of themselves as socialists, but they began to vote for Roosevelt in 1932. In 1932, I would have been nine years old. That was the period in which I was going through the Bar Mitzvah ritual. It was also the period before I began to develop any real, serious consciousness of the world out there, which developed for me around fourteen and fifteen—I started becoming aware that there was a big world out there, and it was a troublesome place, and you had to begin to try to understand what was going on in that world.

Rabkin: Just thinking about your mother dashing over the rooftops delivering propaganda: What was her motivation, if at that point she wasn't a political person?

Friedland: Well, if you were Jewish in Russia at that time, you had a long history of political oppression. One or two tsars earlier, before his time, had established a policy encouraging pogroms. In other words, she grew up in that kind of environment. In that kind of environment, many people became politicized in one way or another. And while my maternal grandparents never had a political orientation, they were practicing Jews on the fringes of orthodoxy and reform, but more on the orthodox end of things. So, for example, I never remember having a political discussion with my grandparents—never. Whereas I did have somewhat political discussions with my parents, because my parents were active in the Workmen’s Circle. And I was beginning to come to consciousness when Roosevelt was elected as president.

There was a very strong feeling, coming through the Depression, about Roosevelt. So my parents always thought of themselves as both socialists, but Roosevelt supporters, which meant they voted Democratic. I don’t believe they ever belonged to the Democratic Party, but I don’t really know. I never queried them about that. The Democratic Party was not something that we talked about. There was talk about Roosevelt, but that was more fixed on his personality rather than the party.

Rabkin: And what events or experiences do you associate with your own political awakening?

Friedland: [pause] I became part of a small group of young kids, mostly boys, who formed a kind of a grouping within the Jewish Community Center, which

was about two or three blocks away. And I don't know why I did this—I don't remember why—but I thought we needed some kind of medium to express our views to other young people, and to the Jewish Community Center. So I came up with the idea of having a newspaper. This would be mimeographed. And this was where I began to develop my skills with the technical aspects of media, because I had to learn how to run the mimeograph machine; I had to learn how to cut stencils. And these were the days when stencils were these smelly blue things and if you made a mistake, you had to get this fluid and clean it up. I had to learn how to make visual things, which I did by taking coins, and making everything round. (laughs)

Rabkin: Tracing coins to create circular graphics?

Friedland: Right. To make letters, etcetera. So with coins and a ruler, I learned how to do the stencils, and I became the editor. And I think it was with that that I began to develop a sense of a world *out there*.

The most salient memory that I have of that period was that Thomas Mann came to Staten Island as part of a speakers' program, and I had no idea who he was, except that he was a very famous author. And I guess because I was the editor of this young Jewish "newspaper," I was invited to interview him. And so I interviewed him, and naturally I wasn't very sensitive to his background, because he was already a very established author, but I remember getting *The Magic Mountain* from the library, and starting to read it, and getting very, very confused. (laughs) But I did rise to the occasion. I interviewed him. And that was

really the beginning, because naturally, he mentioned what was going on in Europe. This was before World War II, before the German and Russian invasion of Poland, which triggered World War II. And he talked about Europe, and he was much more Euro-oriented than American-oriented, although I think by that time he was already established in the United States—but I really don't know what his status was.⁴ And of course, I didn't understand half of what he was telling me, but that was probably a key start to my understanding that there was a significantly larger world out there.

And then, of course, I could talk about that, and other events. I remember, at that time, the key word for what would become the Allies was *collective security*. And so I became a partisan of collective security. We knew about Hitler, because Hitler had already come to power. We had already gone through the experience of Kristallnacht. And while I didn't understand most of what was going on, it was clear that if you were a Jew in Germany, you were in dangerous circumstances. So that, too, served to "open me up."

Rabkin: And collective security meant what?

Friedland: The words *collective security* meant the coherence of Great Britain, France, the United States, and most of the other allies. I did not really know much about the Soviet Union at that time, and to this day if you ask me—because

⁴ According to *Wikipedia*, Thomas Mann did not emigrate to the U.S. until 1939. He did not return to Germany after traveling in the south of France in 1933 when the Nazis came to power in Germany. He settled in Switzerland and received Czech citizenship in 1936. I am certain that I interviewed Mann before U.S. entry into World War II but I cannot recall whether I did the interview before the war had begun in 1939 in Europe—William Friedland.

I dropped the idea later on and went on to other things, you understand—I have no idea whether or not Russia was, at that stage, included within the idea of collective security. But it was the Western European [Allies], mostly, and the United States. The attempt was to try to bring together what were very disparate nations, because France had a distinct orientation towards Eastern Europe. It was really France that got us into the war specifically. France had an alliance with Poland. I think the Brits had an alliance with Czechoslovakia. I can't remember those details. But the Brits were more world-oriented, and the French were, at that stage—they were also world-oriented, but they were oriented towards Eastern Europe.

Collective security formed around that idea, trying to pull the United States in. But the United States was—at that time, the population was significantly isolationist. There was a large German population that was associated with Germany, which then led to the creation of the German-American Bund. And there were Nazis as part of that. So the United States was very split with respect to what was going on in Europe. And there was the historical tendency: Europe is “over there”; don't get involved in “their” wars. Whereas there was also the issue of the Soviet Union, whose birth had led to involvement, and the invasion of the Soviet Union, by Britain, France, Poland, the United States, and a number of other countries. But that had faded into the historical past. But the rise of Hitler: that was a very immediate thing.

Rabkin: Did you have other experiences, as you moved through your high school years, that were significant in forming your intellectual or political outlook?

Friedland: It is very strange, but I have almost no memory of my high school years. Absolutely no memory. I know I went to class, because I graduated. (laughs) I had skipped two grades in elementary school, so I was ahead of my age cohort. I had no friends that I can recall in high school. I cannot recall a single teacher; I cannot recall a single class.

I remember reading voraciously, however. And this was when I discovered the library. Now part of that was the accident of the New Deal program that sought to provide part-time employment to young people. And I don't know how I did this, but I managed to get employed, and I became a library page downtown, down near the ferry building.

Rabkin: On Staten Island.

Friedland: On Staten Island. So I was introduced to the library as a page, and I began to read voraciously. And I mean *voraciously*. I was consuming, probably, three to four books a week. I didn't have any guide to my reading, and over a period of time I began to specialize. So I would go through a Russian phase. And during the Russian phase I would read Tolstoy: *Anna Karenina*, and *War and Peace*, etcetera. Then I would go to a French phase, and read Romain Rolland. And then I would go through a British phase, an English phase— (laughs)

Rabkin: Were you reading mostly fiction, or a combination of fiction and nonfiction—?

Friedland: I was reading mostly fiction, but it was significantly *historical* fiction. A lot of the European writers bring in history in their stories, so I was learning the history at the same time. It wasn't that I was *conscious* of history, or reading nonfiction. I wasn't. But, for example, I went through a Civil War phase, in which I read the complete works of this writer whose name was Joseph A. Altsheler, who wrote, I don't know, a dozen books or more about different—you know, he'd take a battle and write a book about it, and then he would take another period. So, in effect, he covered the whole of the Civil War. And I believe I read all of his books, so I learned a lot about the Civil War. (laughs)

That continued throughout my high school period. I continued my reading throughout the high school years. The result is that I have absolutely no memory of high school. And I have no memory, so to speak, of engaging in politics myself as a result of reading. So I had no engagement with the political parties and what was going on in the United States, aside from that phase of collective security, which was mostly associated with the Jewish Community Center, which I grew out of by the time I was, I don't know, fifteen or sixteen.

The Jewish Community Center became a modern expression of Judaism for me. I had to go through the Bar Mitzvah ritual, and my parents gave me a choice, I think when I was six or seven. They said, "You can either go to *cheder*"— a place

where you learn the prayers, etcetera; you learn to read Hebrew—"or you can go to the Yiddish school." I didn't see the point in going to the Yiddish school because I already spoke Yiddish. Yiddish was my mother tongue.

Rabkin: So both of your parents spoke Yiddish in the home?

Friedland: In the home, yeah.

Rabkin: Did they learn English as well?

Friedland: Yeah, they both spoke, English, both with a reasonably heavy accent—my mother's accent much heavier than my father's, because she was mainly in the house. They both spoke English, though. But the family language was Yiddish. There were four of us boys; I was the youngest. We were all living at home. My oldest brother was really out of the house, because he was already going to college. When the family gathered, we spoke Yiddish. Or, more accurately, our parents spoke to us in Yiddish, and we responded in English.

Rabkin: Mmm! Interesting.

Friedland: All us boys, we spoke English amongst ourselves. And that was the beginning of my transition to English. And then, as I went to public school, naturally I had to deal with English, and so I became fluent in English. But the mother tongue was Yiddish. So this was all part of, so to speak, the growing up of Bill Friedland. (laughs)

Rabkin: So you didn't go to Yiddish school, because you saw no reason—

Friedland: No, I chose the Hebrew school. And that was a dreadful mistake, because the Hebrew school, which was a local one, a five-, ten-minute walk away, was in the back room of this store whose owner sold *gorsets*—corsets.

Rabkin: (laughs)

Friedland: There was a back room, and it was furnished just like you would find in Europe: heavy wooden furniture; a very large armoire in which the books were kept, and miscellaneous paraphernalia. We kids sat at this rectangular, heavy wooden table. Everything was dark. The armoire was dark; the table was dark; the benches were dark. And we would learn the prayers. You have to learn the Hebrew letters, the alphabet—you have to learn how to read. And I did all that, and I became fairly fast, like most of us kids did, at reading Hebrew. And we had no idea what we were reading.

Rabkin: *Oh.*

Friedland: It was like traditional Catholicism, you know, when the Mass was in Latin. Nobody understood what the language was.

Rabkin: You were learning what the sounds of the letters were, and how to speak them—

Friedland: Right, and the proper prayer at the proper time, and the appropriate prayer for this activity or that activity. For eating bread, for drinking wine.

Rabkin: Uh-huh. But you didn't know what the words or phrases meant, necessarily?

Friedland: No. We never really learned enough Hebrew. I must have understood maybe two-dozen, three-dozen Hebrew words that you pick up through the prayers.

Rabkin: Did you have to learn a Torah portion for your Bar Mitzvah?

Friedland: Oh, yes. Oh, yes, you had to memorize it.

Rabkin: Did they have you learn the English translation of that?

Friedland: There was no English translation either in our *cheder* or in our synagogue. This was the synagogue my parents went to, because my maternal grandparents went to it, and it was a traditional Jewish synagogue: women upstairs, not down on the floor. I was a male, so I went down on the floor and I would sit with my grandfather. And I could read along with him. I never could memorize where you had to shift pages. So, for example, I would always get lost when they jumped around, (laughs) and my grandfather would point, you know [Mimes jabbing finger repeatedly at table]— he'd look at my prayer book, and he

would turn the pages and find the place and point, you know, so then I could read and catch up.

This was an experience that left me with a great deal of anger. Because I came out of that experience—I mean, after all, think about it. I must have started Hebrew school when I was seven or eight. And I went through that whole process until I was thirteen. After Bar Mitzvah, I stopped, and nobody really encouraged me, except my grandparents, to go to synagogue. As long as my grandparents were alive, my mother pushed me in that direction. But my parents had already transcended that. My mother maintained a kosher house. She would buy beef, take it home, put the kosher salt on it so all the blood would run out of it, so the damned food was tasteless. I developed—historically, I still have this anger, this resentment, that I was stupid enough to get involved in that kind of time-consuming experience without really *learning* very much.

I knew, for example, in the afternoon prayers, at a certain stage, you have to take three steps backward, and then you continue the prayers for a little bit, and then you take three steps forward. I have no idea—not a *clue*—I have never learned why we took three steps backward and then three steps forward. So I have this subdued anger that all that time got wasted. I learned how to read Yiddish, and I could read Yiddish and understand most of it, reading it, but Hebrew—*pfffft*. (laughs) Nothing.

Rabkin: So, can I infer that religion, then, did not become a central part of your spiritual or intellectual life?

Friedland: No, if anything, I think that my anger about this began to drive me away from the religion, because I asked myself, “Why are we doing this? Why do you take three steps backward?” Or, “Why do you rock?” Or, “When you open the curtain in front of the ark at the front of the synagogue, why should you avert your eyes and not look *into* the ark, where the Torahs are kept?” Why should you do those things? So, in other words, there were a whole series of *behaviors* that you had to learn, besides the reading of the prayers. I mean, *avert your eyes*?! (laughs) Somehow or other, if I looked into it— I don’t know, what would I see—God? (laughs)

Rabkin: Could you not ask your teachers or your parents those kinds of questions?

Friedland: I think the basic understanding was that—at least with this conservative synagogue that my grandfather attended—you didn’t ask those kinds of questions. You did it because it was *tradition* and the object was less to understand and more to express your commitment to God.

I mean, why should you say a prayer for different things? I don’t remember how many prayers there were, but there were prayers for almost everything. There were three prayers that, no matter what you did, you learned, all right? There was bread—and I can repeat it now; there was wine, and I can repeat it now. Then there was the general-purpose prayer, *She’hecheyanu*, which you used if you couldn’t remember the specific prayer which would be appropriate for the

activity. I learned later on that there was a distinct prayer before you screwed your wife. All of that struck me as ridiculous. That is to say, it's the *ritual* which is more important than *understanding* the ritual. So you do it because it is traditional. I think this was one of the reasons why, by the age of fifteen, I was already an atheist. I think my grandparents died when I was around fifteen, and at that stage, then, my parents would not push me in a religious direction.

Rabkin: Tell me about your decision to go to Detroit.

Wagner College

Friedland: Oh. That was associated with my becoming political. I graduated from high school in January 1940—so I graduated in the middle of an academic year. My parents talked me into attending Wagner College, which was a small, Lutheran denominational college on Staten Island. This was before the offshoots of the higher-education system spread to Staten Island. So going to Wagner College was one way of staying at home, going to college, rather than going into the city.

The Wagner experience was also infuriating. Since I was a first-year student, first-year students were expected to go to chapel. (laughs) So here, everything was in English, but by this time, (laughs) I was rejecting all of it. Now I could hear and understand; there wasn't the sheer amount of ritual that you find in the Jewish tradition I grew up in. But there was ritual. By this time I was rejecting ritual quite vigorously.

The only classes I remember taking that first semester were biology and German. I quickly learned I wasn't going to be a biologist, because I didn't like dissecting dead worms. (laughs) The German experience was no better, (laughs) because on the first day of class, I sat down in my chair in the classroom—a lot of young people in there, so I wasn't alone. This was Beginning German. And finally, this small almost-dwarf of a man, with a kind of a rod up his backbone, came in. He's carrying a bunch of books and he comes in the door, and he stops just inside the door and he looks at us. It was clear that something was wrong. He goes and he puts his books and stuff on the desk, which was on a podium a bit higher than the regular floor. Then, with his German accent, he expressed his anger at the fact that we had not stood up when he walked in the door. He said, "From now on, when I come into the room, you will stand up on the right of your chair until I give you permission to sit down!"

Welcome to European academia. (laughs)

Rabkin: (laughs)

Friedland: Or at least German academia.

Well! (laughs) This was then followed by the ritual of calling the roll. He began to call the roll, and we're all responding [Adopts heavy German accent, draws out the names in stentorian tones]: "*Friedland!*" he says. And I say, "Here," in a meek voice. He goes down, continues on, and then he's getting near the end of the alphabet, and he says [with German "f" pronunciation of the letter "v"]: "*Vogel!*"

[Pauses] Nobody says anything.

Rabkin: (laughs)

Friedland: So again, he says, “*Vogel!*” He’s looking through our classroom, and one of the kids, I don’t know, somewhere in the classroom, says, “Do you mean, [American pronunciation] ‘Vogel?’” Then we got a quick lecture about, “In this class, you are [Yells, in German accent] *VOGEL!*” (laughs)

Rabkin: (laughs)

Friedland: So I was off to a bad start. (laughs)

Rabkin: (laughs)

Joining the Shachtmanite Trotskyists

Friedland: But then what happened was that one of the other students who had also arrived in the winter semester, so he was also a starting student—somehow or other he and I began to see some commonality, because the rest of the students were now in their second semester at Wagner. We began to talk, and it turned out that he had already become a Trotskyist. He had joined a group that was usually called the Shachtmanites. There had just been split which had taken place within the American Trotskyist party, which was, at that time, the Socialist Workers Party. The split-off group, the Shachtman group—they called

themselves the Workers Party. This split had just taken place shortly after the Trotskyists had either split or been kicked out of the Socialist Party, which they had joined a couple of years before that. The latest split took place about the nature of the Soviet Union.

The issue of the Soviet Union emerged because Trotsky's position was that the Soviet Union was still a workers' state—a *degenerated* workers' state, but, compared to the capitalist states, Trotskyists had to support the Soviet Union whenever the Soviet Union was in danger. Because there was still an opportunity to build socialism, if the correct segment of the ruling party got to rule the party—namely the Trotskyists.

The Shachtman group, when they split out, were no longer satisfied with Trotsky's formulation about the Soviet Union. So they were on the brink of, in effect, saying, "Well, it's not a socialist state any longer." Which then left them with a problem: If it's not socialist, and it's not capitalist, then what *is* it? That became a central issue in the first year or two in which I joined this Shachtman group. Because my first contact with them was when Walter—this was the young guy who had already joined—said to me, "There's going to be a debate about Bolshevism"—and such-and-such a date, time, "in the City. You want to come?" "Why not?"

So I went along, and I listened to this debate. Shachtman was working his way into an unusual position called bureaucratic collectivism—a new social formation. Another group that had also split out, they thought that the Soviet

Union had become capitalist. There was another group that couldn't make up their minds. So this was hanging. But the debate was absolutely brilliant, absolutely brilliant. Shachtman was a marvelous speaker, with a fantastic sense of humor, a very caustic capacity in his polemics, which were very sharp. He knew the whole Marxist canon, and he could produce it. He was debating some young people that had split out from the Socialist Party, but who were no longer prepared to say, "We are Bolsheviks." They wanted to break with Bolshevism. Shachtman was not prepared to break with the Bolshevik history. The debate included two people who would later become very well-known sociologists—although I didn't know it at the time, and it had nothing to do with my becoming a sociologist. They were Philip Selznick and Seymour Martin Lipset. Both turned up on UC Berkeley's sociology faculty when I arrived there in 1956. The two of them were participants in the debate. They were both very, very smart. They knew enough about the Marxist canon to give Shachtman a hard time. But Shachtman really grabbed a hold of me. So I joined, on the spot. And from that point on, Walter and I began building a group on Staten Island. And that was the way in which I became personally politicized.

Becoming a Labor Organizer in Detroit

Rabkin: And how did that end up leading you to Detroit?

Friedland: Oh. In the process of the split from the Socialist Party, the Socialist Workers Party took mainly the proletarians who were in the Party. The Shachtmanites took mainly the youth and the New York Jews.

Rabkin: Hmm.

Friedland: (laughs) The Schactmanites were very, very heavy with young people, and very, very heavy with young Jews in New York City. If you're going to be a revolutionary party, you can't stay in New York City. You gotta be with the proletariat. The policy was to "proletarianize"—that was the term we used—and the policy also was to "colonize"—to ship us out to establish colonies in Detroit; Youngstown; Pittsburgh; San Pedro, California; etcetera.

Rabkin: Heavily industrial cities?

Friedland: Heavily industrial cities. The basic theoretical argument was, "The proletariat is out there. You have to go where the proletariat is, because when the revolution comes, you have to be prepared to present them with the correct line. If you're not there, and if you don't understand the proletariat, how are you going to talk to them?" So the object was—and, as a matter of fact, it was kind of the way in which anthropologists (I learned this later) learned to go out into the field. Anthropologists have to go out into the field. Well, when you get out into the field, people are doing all kinds of things that are strange and weird. So the first thing you do is you *shut up*. (laughs) The last thing you do is start telling 'em not to do what they're doing, all right, because it's a ritual, for example. (laughs)

So we were under instructions that for the first six months to nine months to a year, you just get into the shop, you work. You listen to what goes on. You talk what people are talking, which is mainly sports. Only after you've been there a

year do you begin to talk; only after you've been there a year do you start going to union meetings. Or if you go to union meetings earlier, you sit and you listen; you *don't speak*. You don't shoot off your mouth, because you're an ignoramus, and God knows what you're going to say. If you listen, you learn what the culture is. We were not exposed to the concept of culture, but essentially, we were, in effect, told there was such a thing as workers' culture, and you had to learn what that culture was. You had to learn what the shop language was.

Rabkin: How old were you when you went to Detroit?

Friedland: I would have been—let's see, seventeen or eighteen.

Rabkin: What was that experience like for you?

Friedland: Going to Detroit?

Rabkin: And being in this role of observer, and trying to integrate yourself into this culture.

Friedland: Oh. I was quite enthusiastic. Yeah.

Rabkin: Were you in an auto plant?

Friedland: We had to learn how to handle a machine, since most of us young people had never actually *seen* most of the machines that workers work with. So

before leaving New York, Staten Island— I mean, we'd built a *group*, by the way; we had an enduring group in Staten Island that was very irreverent. Even though we had joined, we were all enthusiastic, we Staten Islanders were irreverent. For example, it was common in the leftist movement—not just the Trotskyists—to name groupings after socialist heroes. We named our group the Jacobin Jerques.

Rabkin: (laughs)

Friedland: Spelled j-e-r-q-u-e-s. Okay? Now, why did we do that? Well, one thing was, we didn't have much history, you see; we didn't know that the Jacobins were a *bourgeois* radical group. So when we did this, our city organizer was pissed at us, because—"you named yourself after a *bourgeois* group?! You should be naming yourself after Karl Marx or Rosa Luxemburg." That's kind of legitimate. So that was part of our irreverence. Another part of our irreverence fitted in better, and that is, Walter, the guy who recruited me, turned out to be a terrific parodist. He could take some event, and he could write a parody that fit it beautifully. So typically, our party had an annual picnic, usually held in the City somewhere, or Brooklyn or Queens, we invited them to come to Staten Island. With that invitation, we prepared a skit that dealt with the current political situation. It naturally had a number of songs, and we entertained them—and we wowed them.

So we were irreverent, but we had established ourselves. But we were also being told, "Get out." (laughs) That is to say, prepare yourselves to get out. So I went

looking for a job. I wanted a real heavy industry job, and I found it in Long Island City. I commuted from Staten Island to Long Island City, and in this job I learned: this is a lathe; this is a milling machine—(laughs)—etcetera. I learned how to operate some of these machines. Then at a certain stage, they said, “Okay, enough. Off to Detroit.” Because in Detroit there were already five or six of our group established, and they were in the process of building that group, and they wanted to get it up into about ten to fifteen.

Rabkin: So did you go seeking a job in an auto plant?

Friedland: Oh, yeah. I got to Detroit. Pearl Harbor had taken place and we had gone into the war. There were jobs, plenty of jobs. I got a job at Hudson Motor. I was running a lathe, and that lathe was making exhaust manifolds for the Invader engine, which was used on landing craft. Big, I mean, these are *big* engines. So the exhaust manifold was about yea long [spreads hands four to five feet apart]. That was a big item that had to be machined, and I had this job.

I then kept quiet on the job. I was there, and I slowly but surely began to go to union meetings, slowly but surely began to speak up about issues in the local union. I had no interest in sports, so I would take political issues which people would know about, and I would start talking to them about those kinds of issues. Slowly but surely I established myself, so that when it was time to elect a shop steward, I ran for shop steward, and got elected.

That gave me a union location. We as a group, as a political group, would meet once a week, normally. We had a group at Hudson of three of us, so we could talk about Hudson union politics, in effect—what's going on in the shop, what's going on in the local union, what's going on in the UAW [United Auto Workers], and so forth. So that then became part of the educational experience.

Rabkin: How did the politics of the union that you joined compare with the politics of your organization?

Friedland: Well. You don't go out and start talking about the political group. You want to find issues that are meaningful for workers. Well, the first meaningful issue had to do with the establishment of the no-strike pledge which was taken by the unions, officially as the U.S. entered the war.

There was a peculiar situation. Just prior to that, there was stuff going on in the rest of the world which partially didn't make sense to what was going on in the union. See, at a certain stage—this was before I became politicized—the communists were going through this period in which they were flipping political lines. As I started becoming aware, the communists were talking seriously about collective security. The Soviet Union wasn't really part of that, but was on the edge of it. But then, suddenly, out of nowhere, Stalin and Hitler signed a pact. That was 1939, I believe.

Now, that was an *amazing* thing, because for the communists to sign a pact with Hitler, with his anti-Semitic history— By that time, he had been in power for at

least six years. We had been through Kristallnacht. We had been through the establishment of the concentration camps. We were beginning to get some sense of the concentration camps, not just for political people, but for Jews. For there to be a Stalin-Hitler pact was really quite amazing. I remember—and I was sixteen years old at the time and this was before I became a Trotskyist—I remember giving another kid, who was about two years older than me, and who came from a communist family—I knew the family; I knew their kids, I knew a lot of Jews in Staten Island; these were kids that I had grown up with, although I wasn't that close to them—I remember giving this seventeen- or eighteen-year-old kid a hard time because the *Daily Worker* had failed to report the Nazi-Soviet Pact the day after the announcement of the pact. Communists didn't know what to say. Actually, I learned later on that the *Daily Worker* did appear, but that there was nothing in it about the pact, the next day. It took them a forty-eight-hour lag time before they could get their ideological (laughs) shit together, to figure out what to say.

After I joined the Trotskyists, one of the things I learned was that the communists had come to the Popular Front period around 1936, and that prior to that period, the communists were operating under the Comintern with a form of analysis that was called the Third Period. According to the analysis of the Stalin group, which controlled the party, the Third Period was going to be a period of general world revolution. Well, the Third Period turned out not to be one. Beginning around 1934—late '34, beginning '35—they began to drift away from the Third Period into the Popular Front period. In the Popular Front period, the communists were trying to get the Socialist Party in France, where it was successful, into a coalition

between the socialists and the communists. Léon Blum became the prime minister. Blum was a Jew in the Socialist Party, and so you had a Popular Front government. This was important because in Spain, you had the Spanish Civil War going on, and the question was, would the socialists in France help the socialists in Spain? It was a very complex situation. George Orwell's *Homage to Catalonia* dealt with this, how difficult things were.⁵

⁵ Friedland provided the following explication in written form during the editing process:

To understand the period between 1928 and 1941 requires some historical analysis. The first thing that must be recognized was the major split that occurred with socialist movements with the Russian revolutions of 1917 during which the Communists ultimately seized and kept political power. From 1917 on, the split between the Social Democrats and Communists were manifested in every nation that had had a socialist movement with the beginning of the First World War. By 1928, Lenin, who had been the unparalleled leader of Russian communism, had died and Stalin had become the dominant figure in the Soviet Union. This led to the exile of Stalin's major opponent, Leon Trotsky, from the Soviet Union and a series of purges that wiped out the remaining Bolshevik leadership and destroyed the top generals of the Red Army.

By 1928, Stalin's domination had led to an analysis within the Communist International that became labeled as the *Third Period*. The essence of the Third Period was that the entire world now existed on the brink of a global revolution that would come to the defense of the Soviet victory. For the national Communist parties this required the formulation of extremely revolutionary demands that made little sense with working classes under major economic stress with the first global depression.

For approximately the next four years, the hyper-revolutionary analysis motivated all of the Communist parties. As economic realities asserted themselves and with the rise of Mussolini in Italy and Hitler in Germany, the continued adherence to Third Period strategies became obsolete which led to the gradual emergence of a new period of anti-fascism in which the Communists sought to create anti-fascist *Popular Fronts* with socialists and bourgeois liberal parties. Super-militancy was now put aside to try to form liberal political coalitions. Popular Front analysis dominated the Communist parties for the next four years until, suddenly in 1939, a Nazi-Soviet Pact was announced. This startling shift led to a resuscitation of Third Period militancy in which the U.S. Communist Party had at its main slogan, "The Yanks are NOT coming" even as the Nazis and Soviets invaded Poland triggering the second world war.

This breathtaking shift, after four years of anti-fascist argumentation, took many by surprise but Communist parties adjusted their lines once again, essentially based on the necessity to defend the Soviet Union. This line, in turn, led to yet another shift when in June 1941 the Germans invaded the Soviet Union. Just as the previous shift had required Communist parties to shift within 24 hours to justify the Nazi-Soviet Pact, the invasion of the Soviet Union now produced another 24-hour devastating shift returning to much of the Popular Front strategies. In the U.S., for example, the Popular Front had made President Franklin Roosevelt, essentially a fascist during the Third Period, a popular Communist hero. Roosevelt returned to the fascist-like analysis of the Communists during the Nazi-Soviet Pact period but then got flipped again into a working class hero once the Soviet Union was invaded.

Throughout this period with its many shifts in the party line, in the U.S., socialists mocked the Communists for the frequency of their analytic inconsistencies, the only consistency had been the defense of the Soviet Union.

We young Trotskyists in Staten Island couldn't do much about any of this but we could learn to sing the songs that Communists garment worker unionists had been singing to mock the social-democratic garment worker unionists, and which the social-democratic workers would now sing back to mock their Communist opponents. We in Staten Island could encourage our comrade, Walter Cliff, to write new songs during the Nazi-Soviet Pact mocking the continual shifts in the Communist line where "Volga boatmen sailed the Rhine." Walter became adept at writing parodies that spread quickly among our Trotskyist comrades in New York City. If I can jump ahead for a minute, for example, at a much later stage in the early 1950s, Joe Glazer (then a trade union educator), and I put out an album of songs called "Ballads for Sectarians,"⁶ in which we collected many of the anti-communist songs from the Left, and recorded them for an album. And one of the great songs was a song that came out of the Third Period, which was sung by the Jewish fur workers, who were communists. And during this Third Period, they would sing this song: [Sings]

The Cloakmakers Union

(Sung with a heavy Jewish accent)

The Cloakmaker's Union is a no good union,
It's a company union by the bosses.
The right-wing cloakmakers and the Socialist fakers
Are making by the workers double crosses.

Oh, the Hillquits, Dubinskys, and the Thomases
Are making by the workers false promises.
They preach socialism but they practice fascism
To preserve capitalism by the bosses.

⁶ <http://www.folkarchive.de/friedland.html>

They had sung this song to mock the socialists.⁷ And then, when the line shifted, the socialist unionists began to sing exactly the same song as a polemical song against the communists. He and I recorded this song—a very, very short song. We collected those songs which were reflections of the continual shifting of the Communist Party line. Because, you see, once the war ended, and then the Cold War began, the American Communist Party shifted back to a more militant, aggressive, anti-capitalist orientation.

Rabkin: What was your vision of your role, and what you hoped to accomplish as an organizer in Detroit?

Friedland: Our major activity was to try to recruit workers as Party members. So, when there developed a fight against the no-strike pledge inside the UAW, this created a major split in the UAW leadership. Some of them were Communists or C.P. sympathizers. Part of the leadership were socialist but also anti-Communist; others were tied in to the Association of Catholic Trade Unionists who were anti-Communist. There were also a significant number of militant UAW unionists who were not politically tied to any groups but who didn't like the way in which the no-strike pledge benefitted employers and who wanted, as a result, to dump the no-strike pledge.

⁷ Friedland provided the words to the song during the editing process, as well as the following historical note: "Morris Hillquit was a founder and Socialist Party leader. David Dubinsky was leader of the International Ladies Garment Workers Union, an anti-Communist union. Norman Thomas was the perennial Socialist Party candidate for president during the 1930s."

This issue created a major battle at the 1943 UAW convention in which the anti-no-strike pledge delegates almost won a majority to repeal the no-strike pledge. The Communists were the strongest supporters of the no-strike pledge because they regarded the pledge as important to keep American workers working to produce war materials to defeat the Nazis. This flowed from the continuous line of the necessity to support the Soviet Union irrespective of how this became manifest in the many national Communist parties.

So that was one issue. A second issue had to do with the auto shops during the war. Up until the war, in most auto shops, there were no African Americans working on machines. All the African Americans had jobs cleaning the floors, pulling the chips out of the machines—all the dirty jobs that have to be done. Well, during the war, there were a number of battles about black workers moving up into production jobs.⁸ Well, Hudson had a seniority plan which said, from the time of employment, that's when your seniority begins.

Rabkin: No matter what position you're in?

Friedland: No matter what position. Other shops had it depending upon position. But many shops didn't. At Hudson, we had this situation in which, since from date of employment you had blacks who had very long employment. When a job came open that paid, let's say, a dime more, how do you recruit to replace the operator? I was a shop steward. I looked at the seniority list, and I

⁸ Friedland provided the following written comment here: "Black workers had accumulated a lot of seniority on the dirty jobs, so they could move on to production and into some jobs that paid a bit more money, because of their seniority."

saw that the top man in the department was an African American. Up until that time, shop stewards had ignored the blacks. Well. It was clear to me who had the top seniority. So I went to Ernie Lundy and I said, "If you want this job, it's yours. You're gonna get a lot of shit, but if you're prepared to take that shit, I'm prepared to get into the battle, you know, that will follow as well." He said, "Yes." All right? From that point on, I had the blacks in my constituency.

Because of the split between what later became the Reutherites and the Communists, the Communists were trying to get rid of me, because they knew that I had been through the no-strike pledge fight. So they had recognized that I was some kind of a radical, probably a Trot, so they wanted to get rid of me. So what they did is they combined districts, shop-steward districts.

Rabkin: Like gerrymandering?

Friedland: Yeah. Yeah, just simple gerrymandering. And the first time they did it, I got elected, and the second time they did it, I (laughs) got defeated. This was all part of what was going on—all part of the learning experience. You know, you learn about parliamentary procedure, because if you're going to function in a union, if you don't know parliamentary procedure, you can't function at a union meeting. So you have to learn how to make a motion; you have to learn when to make a motion, you have to learn what the possibilities are. So you start learning the politics, you learn— It really is kind of a university experience, except you get it through *experience*, see, which is one of the things that oriented me toward experiential learning at an early stage, once I became an academic.

By the time I began to raise questions, about Trotskyism and the analysis about revolutions, I had a tremendous amount of knowledge. And I was not just learning on the job. When I would meet with my comrades from Hudson, I'm learning from them, as well. And when we would meet within our Trot branch, and we would have reports from the various different people about what's going on in their shops, I was learning about that. So I had a very good knowledge of the UAW and its politics.

Rabkin: How long did you continue working in the auto industry?

Friedland: I was in the shops from around August '41, I think it was, until 1949. I got fired at Hudson, as the local union Communists consolidated their power.

Rabkin: So you not only lost your position as shop steward; you got fired from your job?

Friedland: Yeah. So I then began to work in various other auto shops, and I finally ended up at Ford Highland Park, in the trim shop. Then I learned other things, very different things, but nevertheless it was part of my learning experience. In 1947, I started working at Ford Highland Park.

Moving Away from Trotskyism

By '49, I was beginning to raise serious questions about Trotskyism, the revolution— It was clear, in the immediate post-war period, 1945, '46, '47, that

there wasn't going to be a revolution. And in essence, this is what Marxism promised. The analysis was that war sets up all the contradictions within capitalism, and the proletariat gets militant, and eventually they move into a revolutionary phase.

Rabkin: So you had seen yourself as helping work toward that workers' revolution.

Friedland: You work *into* that, and you became part of that process. Well, by '47, it was clear to me that the post-war period of militancy was over, and that we were in a new phase of union development, in which the union leadership— You see, you have to understand one thing about the nature of the UAW. When the UAW got established in the auto industry, it got established because, in the very beginning, it grabbed control of the shop floor. In the Flint sit-down strikes, what the strikers did was to say to their employers, and their foremen and supervisors, "You can't come into the factory. We forbid you." Okay? They welded the doors shut, and they went through that whole experience.

When the companies—[like] General Motors and Chrysler—began to sign contracts with the UAW to recognize the union, the strikes were over. So you went back into the shops, you started working, but now you felt your power on the shop floor. That continued this notion of workers' control of the shop floor, so that I experienced it when I was a steward. This was already well into the war. Well, during the war, one of the things that the employers decided was that

when the war ended, they had to regain control of the shop floor. I lived through that experience.

What was happening was, the national leadership of the UAW was, in effect, saying to the employers, "We will surrender the shop floor, if you will agree to broaden the role of the union within society." So how do you do that? Well, we had Social Security, but Social Security was inadequate. So one of the things that the union tried to get was the augmentation of the contract to Social Security. That was one thing. Pensions: there were no pensions, none at all for auto workers. So there was a big fight about pensions. The UAW won that fight. So, slowly but surely the employers gave up certain concessions as long as they could regain control of the shop floor.

I went through that experience. It was a very puzzling experience for me, because my comrades did not really have a good analysis of that process. As a matter of fact, I built up what I just told you in the years during and subsequent, because one of the things that was happening to me was that I was beginning to move out of Trotskyism. I'm doing it by saying to myself, "How is it that the companies can send in time study men to study our jobs and make us work more? How is it that they can get away with that? Why does the union let them do that?" I was raising this as a question, and in 1949, it was sufficiently big question for me to be alert that the key mechanism was time study. The time study man was the key figure. So I figured I'd better go and study time study. I'm working in the shop floor eight hours a day, and it's heavy work—I mean, it's not working with machinery so much at Ford; we were working with door

panels, making door panels for cars. I said, "I gotta go and get some professional training. I'll become an industrial engineer."

So I worked out arrangements with my mother to cover me financially for a period of time and I left the shop in 1949. I started at Wayne University to become an industrial engineer.

That was the beginning of my movement out of Trotskyism, although it took me from 1949 to 1953 to move completely out. But slowly but surely, I was drawing away more and more because I was finding more and more that our Marxist analysis was wrong. It also became clear to me once I was studying industrial engineering that this was not what I should be studying. (laughs)

Rabkin: Why?

Friedland: Why? Well, one reason was I was a lousy engineer. You want to be an engineer, you have to— Like every academic department, there are requirements, right? So, if you're going to be an engineer, you have to learn industrial drawing. My industrial drawings were a *mess*. And it was the old story: You've got to learn the ritual; you see, you've got to learn it according to the practitioners of the ritual. They tell you whether or not you're doing it right. I was terrible; I recognized that I was terrible. You have to learn physics. Well, when I took Physics 1, I dropped out, the teaching was so bad. I took it again, and again it was so bad that I dropped out. When I took math and got up to calculus, the teaching was so bad that I never learned *why* you do things with

calculus. You just had to go through the rituals. And we would do things, like, when there were exams, we students would gather outside the exam room. Each of us individually would have a piece of paper with all of the formulas that we had covered in the previous period, and we'd be frantically memorizing those formulas. And when we would go into the exam room, we would all throw our pieces of paper on the floor. We would go in and sit down, take out a blank sheet of paper, and begin writing the formulas, you know? (laughs)

Rabkin: Sounds a lot like your Hebrew classes.

Friedland: Yeah. And, as a matter of fact, I never really understood what calculus was about until I was working for the UAW, and the guy who had hired me said, "Well, calculus is just the study of rates of change. The rate of change, that's all that it is. Different things change rates in different ways, so you have these formulas for processes of change. If you take all the combination of formulas, that's the study of calculus." So for the first time, I understood what calculus was about. But by this time I didn't *need* calculus, because the job I was working on didn't require calculus. As a matter of fact, most jobs don't require calculus. So I was learning something that was required for the sake of doing it. Well, it was just like learning which prayer to say when. (laughs)

So I didn't last very long studying industrial engineering. But in the meantime I was looking for a way to support myself so I wouldn't have to depend on my mother. I got a job working part time in the Michigan CIO education department. I fitted in there very well, because by this time the big fight between the

communists and the Reutherites had advanced very substantially. The Reutherites controlled the Michigan CIO Council. Reuther had taken over the UAW, but very tenuously, for several years before he really fully established his power. The Michigan CIO Council paid very well, even working half time. So I was working half time, and I was still taking a few classes at Wayne, but I had decided that I was not going to be an industrial engineer. But I didn't know what I *was* going to be. But this job fell into my lap.

Rabkin: What were your duties in this job?

Friedland: Oh, the main activity of the Michigan CIO education department was to run a series of one-week classes at the FDR CIO Labor Center, in Port Huron, Michigan, during the summer. Now, these were classes not just for the UAW; they were intended for all Michigan CIO unions. But the UAW was so overwhelmingly dominant that we had to work very carefully with the UAW. But there were the rubber workers, steel workers, and some of the other unions, but they were very small by comparison. We had to set up the classes, we had to recruit the teachers. When workers would come—be sent by their local unions—they would choose a class for the week that they would be at the Labor Center. Classes would be on collective bargaining, shop stewardship, radio broadcasting, time study, etcetera. I had gotten involved somewhat with radio broadcasting, so I would teach a class in radio broadcasting. But the main job was to set up the classes, recruit the faculty (who were on the staffs of various unions), recruit speakers to come in—you know, inspirational union speakers—and set up one-week programs. Workers came; unionists came. They arrived on Sunday, and

they finished the classes Friday afternoon. There would be a Friday evening banquet, and Saturday they all leave and go home.

Rabkin: And the unions arranged for them to have a week-long release from their jobs to do that?

Friedland: Yeah. Usually the union paid their salary, or there was something in the contract about taking union leave for periods of time. So we didn't have to worry about their remuneration. And coming to a facility like— I mean, it wasn't that the facilities were posh; they weren't. They were quite simple and very (laughs) down to earth. But nevertheless, people wanted to do it. I had previously gone to a UAW summer school, which was held up at Ann Arbor. That was before the battles between the communists and the Reutherites. The UAW at this time ran their summer school in Ann Arbor at the university, in university facilities. Dorms were empty during the summers, and we moved into the dorms. I had this experience, so to learn how to handle the programming was relatively easy for me. I understood the politics; I could teach a class or two, and slowly but surely, I began to move into time study, teaching a time study class, even though I'd never actually practiced as a practitioner in time study.

Rabkin: Now, you had been questioning the union's involvement in time study. What was your relationship to that now?

Friedland: Oh. Well, when I began to study time study at Wayne, I took a time study class. I was focusing on techniques. Time study presents itself as a fully

objective process—that is to say, a time study man studies the worker doing the job; the time study man breaks the job down into chunks, and then begins to time the worker as the worker goes through each chunk.

Rabkin: With an interest in finding the most efficient possible way to do the job?

Friedland: Right. Because there was also motion study. So you learned something about motion study as well as time study. You can't really separate the two.

But when you see a worker doing a job, you can see that there are certain kinds of inefficiencies. If, for example, the worker is working with parts, and the parts are scattered all over, and the worker has to go like this [reaching in several different directions at once], can you set up the parts so that there's a sequencing [mimes a linear series of actions]— or, better yet, the worker can pick up parts at either end, and hold them in his hand until he gets to the middle. So, in other words, you reduce the amount of motion.

Rabkin: And the idea is that that's beneficial, both for the company and for the worker?

Friedland: Well, from the point of view of workers, it is beneficial to the *company*. Naturally it's not beneficial to the worker. What does the worker get? The worker gets the same wage.

Rabkin: So why is the union interested in teaching time study?

Friedland: Oh. When there was an argument about a work standard on a job, and you can't resolve that argument, what do you do, under a contract?

Rabkin: Take it to an arbitrator?

Friedland: Well, before you take it to an arbitrator—you don't want to go right to an arbitrator because you can't trust arbitrators. (laughs) Anyhow, you're dealing with a technical procedure. Well, the UAW's Research and Engineering Department—the engineering section's job was to go out and challenge the company's time study. Most of the time study process is relatively objective. When you break down a job into parts, you have an interesting question, which you can argue about endlessly. If you just break it down into parts, you're saying that the sum of the parts is always equal to the whole. Well, there are big philosophical issues you can draw upon. But you at least have to be acquainted with them.

Rabkin: I see.

Friedland: But that's a weak argument. The main argument had to do with the process in time study called "leveling." Now, leveling is a process in which the time study man—and we never had a time study woman, so I will continue with that designation—the time study man goes through and writes down the times on each part, okay? So that at the end, the time study man has a series of columns

for each part. Now, you get variations in the times. How shall you set the times for each part of the job, for this particular segment? See, the time study man might take the fastest time.

Rabkin: And then that would be imposed as an expectation of the workers?

Friedland: Yeah. Or, more frequently, the time study man would *average*, which was harder to argue about. The first one we could argue about, but the averaging, you can't argue too much about that. But the *leveling* process is a process in which the time study man, at the end, has a concept of "normal" in his head. So if he sees the worker, and the worker has now been doing this job for several years, the worker has worked out various little shortcuts—has actually done some shortening of the process, the time study man can steal the worker's innovations. That's one problem. So what happens with the worker, is over a period of time, the worker figures out ways of arranging the work, and he's developed certain skills as well. Well, the time study man *steals* those skills, and the innovations, for the company. Workers don't get a benefit. But if you try to tell the worker, "Go back to when you first learned the job," the worker can't do that. Because the worker has now internalized the shorter way.

Rabkin: Sure.

Friedland: So, any time the time study man comes in, the time study man is going to steal skill and innovations. Okay?

Rabkin: I see.

Friedland: So what the time study man does is looks at the workers. Some workers are working like crazy, you know, because they're relatively new, and some workers are working very, very smoothly (laughs)—rounded edges and so forth. The time study man has a concept in his head as to what normal performance is. So if the time study man says, "Well, I say that this worker is working at 80 percent of normal," well, you see, that's not an objective determination. That's purely subjective. In other words, you can *always* argue about leveling. Always.

Rabkin: So this is why union members needed to understand about time study.

Friedland: That's right. And our job was to go in and create the conditions in which the union representatives could negotiate the actual standard with the employer. That was our job: to create the conditions permitting that, so that the company couldn't say, "Well, we have scientific data."

Rabkin: Thank you. Lucid.

So, in 1956, you graduated from Wayne University with a degree in sociology. And we have about twenty minutes left until we've gone for two hours. Are you up for doing it?

Friedland: Let's continue, by all means.

Rabkin: Okay. So tell me how you got from—now you're taking classes part time, working part time—how you got from there to graduating in sociology.

Friedland: Oh. Okay. The first thing I should tell you is that the half-time job very quickly became full time. (laughs) I don't remember how fast, but pretty fast, because I was good at it. I liked it. The guy I was working for was a non-politically associated, leftist, genuine worker, who had been one of the UAW pioneers in the organization of the union. He was a Reutherite, but he was a left-wing Reutherite. He understood Trotskyists. (laughs) And he became my mentor, because, essentially, he taught me how to organize educational programs and then to actually carry them out. Working at the FDR CIO Labor Center— This became the place, by the way, that the Port Huron Statement was written at by SDS [Students for a Democratic Society].⁹ That was some years later. Anyhow, he, Bill Kemsley was his name, had organized a program to recruit students from the Student League for Industrial Democracy. The League for Industrial Democracy was one of the right-wing socialist—I shouldn't call them right-wing. It's one of the socialist remnants, Socialist Party remnants. And they had a youth group, which later became SDS.

Rabkin: Oh!

⁹ Written in 1962 and adopted by Students for a Democratic Society, the Port Huron Statement is a key document of the New Left.

Friedland: Right. Anyhow, my boss at the Michigan CIO Council, Bill Kemsley, had before my time, asked the Student League for Industrial Democracy whether or not students would like to come and spend the summer working in the program. They would be program workers—we would tell them what to do, and they would do it—but if there was time, they could attend classes, so they could meet and talk to workers, interview them. They were always drinking beer in the evening, you know, when people actually relax and talk about their experiences. People would talk to other unionists. So it was a good learning experience for them. So there was a connection to Student League for Industrial Democracy and I met a number of people. One of them was Andrew Hacker, [who] became a professor of politics at, first Cornell, and then Queens [College, New York]. And Aaron Wildausky who was well known in politics—all of whom came through when I was there.

I still didn't know what I wanted to do with myself. I was beginning to have difficulties with the Reutherites. We Trots were split over the Reutherites. One group of us said, "We've got to back the Reutherites," and another group said, "Well, we fought the communists with the Reutherites; now we've got to be critical of the Reutherites, because they're becoming undemocratic, and they're doing this, and they're doing that." I was in the anti-Reuther grouping within the Trotskyists. So, [Sighs] in the meantime, I'm working with Kemsley, who has learned to adapt himself—he's really a kind of a left Reutherite, you understand—and he's quite happy with his job, and I'm getting increasingly antsy with my job.

But then I get recruited to the UAW itself, to the time study group, the engineering group. Its leader was a guy named Robert Kanter, Bob Kanter. Bob Kanter was a pioneer in the UAW. If you've seen the photographs of the organization of Ford, where some of the union leaders have blood on their shirts—Bob Kanter was one of those. He was thrown off the overpass onto Miller Road¹⁰ and survived the experience. Kanter was not about to leave the UAW, so he generated his technical interests by becoming an engineer and became the leader of the engineering group—cluster—within Research and Engineering. And he also was a left Reutherite. But increasingly, I'm having my problems with the Reutherites, and especially in 1952, the Stevenson campaign—all of us working for the UAW were supposed to contribute to the "flower fund." The flower fund was before the Political Action Committees, the PACs, were created. The flower fund was the way in which the union channeled money to work for political purposes. I didn't want to contribute to Adlai Stevenson, because I was still a socialist. (laughs) So I didn't. And Nat Weinberg, the head of the whole department, called me in, and gave me a hard time. He didn't say "You have to," but he gave me a hard time.

I knew that I was running into problems and that eventually I would have to leave the UAW, or I would have to knuckle under. There were plenty of political people, including some of my comrades, who knuckled under. Sam Fishman, for example, became the president of the Wixom local of the UAW, and

¹⁰ Miller Road fronted on the main entrance to Ford's River Rouge plant, at the time the biggest single factory in the world.

subsequently the president of the Michigan CIO Council that I had been working for. But that was later.¹¹

A Brief Period in Europe

So some people made careers within the unions. Some people didn't make a career, they were critical—but they hid. I got increasingly disenchanted, and eventually I said, "I gotta get out." Kemsley, my previous boss, had in the meantime taken a job in which he was based in Paris, with the trade union section working under the Marshall Plan to rebuild unions in Europe. Kemsley was saying, "Come to France, come to France. We'll find something for you to do." So I quit the UAW and went to France. When I did this—this was in October 1953—I did it with the intention of not returning. The McCarthy period was on; I did not see any future for myself in the United States. Kemsley shipped me off to

¹¹ Bill Friedland included the following footnote during the editing process:

I was, however beginning to think about how I could survive in the U.S. given that modern society was so profoundly organizational.

My "revolutionary" experience had begun to make me suspicious of Marxism as an overreaching theoretical system, especially after my experiences with totalistic systems exemplified by Shachtmanite Trotskyism. The Shachtmanite experience was one that emphasized remarkable intellectual freedom in debating issues such as the "American Question," the "Negro Question," etc. They argued for the Bolshevik principle of democratic centralism which favored open democratic debate about political issues until the organization came together and debated strenuously. Once a decision was made, disciplined adherence to the majority decision was required by all the members.

Marxist organizations not under the control of the Communist International emphasized democratic aspects. They also carried aspects of totalism that were less admirable. For example, if a couple were married and wanted to have a child, this was considered a "political matter" and the couple were expected to discuss the question at the branch level of the organization. The rationale was that they should take advice because their energies might be dissipated in raising a family rather than carrying out party actions.

I would have had serious problems with my comrades arguing for a close relationship with the UAW's Reutherites had they had the majority viewpoint on the Reutherites. I believe this was part of my questioning of myself as a Trotskyist.

More profoundly, I had begun to be suspicious of organizations more generally. Later, when I went to UC Berkeley for my doctoral program, I read Robert Michels' book, *Political Parties: A Sociological Study of Oligarchical Tendencies of Modern Democracies* (originally published in German in 1911) in which he set out the "iron law of oligarchies." These suspicions were reinforced when I went to Europe in 1953-4, again without knowing of Michels' book. Michels confirmed my empirical experiences about organizations and this was to become fundamental to me as I took up academic life.

Vienna almost right away to teach some classes to the trade-union youth. I, incidentally, met my future wife at a Thanksgiving party before I got on the train to go to Vienna.

Rabkin: And what was she doing there?

Friedland: Oh, she's English, and she was working as—I think they were called “locals.” Because, see, there was a pay scale for Americans, and then there was a pay scale for local employees, people recruited in France. She was recruited in France to work as a secretary in the Marshall Plan bureaucracy. So she had been invited to this party. Kemsley and his wife took me to the party, and then later took me to my train. That was the beginning of a courtship that ultimately ended with marriage.

Anyhow, I went to Vienna, and I taught the classes. I liked teaching; I liked the people. But I quickly began to realize that— I had originally thought I might become a political refugee in Austria, because they had passed a law to let political refugees come to Austria and take up citizenship. I thought, well, this is my escape hatch. But I quickly became disenchanted with that orientation, because I discovered that—I should have known it from my reading—if you wanted to collect stamps in Austria, you were either in a socialist stamp-collecting group, or a Catholic stamp-collecting group. And if you were a lawyer, you were either in a socialist lawyer group, or a Catholic lawyer group. So we had, to a certain degree, what sociologists would call a “dual society”—two societies living side by side.

Rabkin: Everybody was pretty much either organized around Catholicism or Socialism?

Friedland: Either in one or the other, yes. In France, it was communist, socialist, Catholic. When I got back to France and found that—I mean it was different, but fundamentally the same. And one of the things that had led me to break out of the UAW intellectually was that I had not psychologically prepared myself to accept the discipline of the union. That is to say, the UAW signs a contract. It may turn out to be a shitty contract, but the economic circumstances are such that you can't do better. Okay? So what do you do? You go and you have to sell it to the workers, because that's part of the process. All right. So, naturally, you don't go to the workers and say, "This is a shitty contract."

Rabkin: (laughs)

Friedland: You may do that, but quietly. But mostly what you say is, "This is a *good* contract," or "This is a *great* contract." Well, I was prepared to accept that discipline, but then one of the things I discovered was that I was also expected to *believe* that. (laughs) You were expected to brainwash yourself into believing that these were good things to do.

Rabkin: Expected by the union leadership?

Friedland: By the union leadership, and by the other people—your union peers. We were full-time functionaries; we were full-time trade unionists. We were called “porkchoppers” inside the UAW, by the way. “Porkchoppers.” Because in the early years, people who had worked for the union, and they were engaged in politics, they would say to a worker—one of the leaders—they would say, “Let’s go out and get a pork chop,” you know, “Let’s go get a meal.” So they were called “porkchoppers.” We were called “porkchoppers.”

Well, accept the discipline, yes. But believe it, no! Then we had the whole McCarthy business. My Trotskyist group was split between the Reutherites and the critical Reutherites. It also had become clear that the FBI knew what was going on in our branch meetings, because two FBI men presented themselves and started talking to me, trying to get into my apartment and talk to me some more, and I wouldn’t let them, but enough came out so I knew that they knew what we were talking about in our branch meetings. So I knew we were infiltrated, or they had us wiretapped.

I couldn’t see any future in the United States. So off I go to Europe (laughs) And I discover that I can’t survive there either. I’m breaking out of my Trotskyism. I go and talk to the leader of the Social Democratic youth in Denmark, and he tells me that we have to adapt to the way in which work is organized. And that is to say—he didn’t use this language—“Work itself is going to be shitty. So what we have to do as socialists is to reduce the amount of time that workers spend at work.”

Rabkin: Hmm.

Friedland: (laughs) So, in effect, they can manifest their creative capacities outside of work. One of the things I read in Marx was that it is work that makes human beings what they are. It is work that does that. So I can't reconcile this. I'm saying, "How am I going to live in a Social Democratic society, or a Social Democratic part of a society?"

I developed a pungent expression: "The USA is a pot of shit, but it's *my* pot of shit." Which meant that I had to go back to the U.S. By this time, the Army-McCarthy hearings had taken place and I began to see the end of the McCarthy period. So I said to myself, I'll go back, but what will I do? I will become an academic. I have a great deal of industrial experience. I know all this stuff about industrial engineering, collective bargaining, workers' history. I'll become an industrial sociologist. And the university system was expanding rapidly in the 1950s.

So back home I go to become an industrial sociologist. That's how I got into academia.

Rabkin: Great. This might be a good stopping point for now.

Rabkin: Today is November 15th, 2012. I'm Sarah Rabkin, and I'm, again, with Bill Friedland in his office at College Eight. This is our second interview. Bill, I think last time, when we left off, we had arrived at your undergraduate career at

Wayne University. We didn't really talk a great deal about your coursework or your experiences there, or your decision to major in sociology. Before we proceed on to your graduate work, and then to Cornell, Stanford, and UC Santa Cruz, I wonder if there's anything else you'd like to say about that time as an undergraduate?

Friedland: Yeah. I mentioned the fact that I'd been thinking about what I'd have to do if I couldn't survive in Europe, and that the only logical thing then was to return to the United States. But, to do what? Well, what was clear to me was that I had a very valuable organizing experience within the Shachtman group—the Workers Party—and that I had a very valuable experience as a factory worker, as a shop steward, as a union activist. What does that add up to?

Becoming an Academic

During the period in which I was working for the Michigan CIO Council and then the UAW, I had become very friendly with Harold Sheppard, who was an industrial sociologist at Wayne. I was a useful person for Harold because I was an insider, and at the same time I was a critical person. So we became quite good friends—and it wasn't just him, it was his wife as well. She was a social worker who was very interested in the dynamics of unionism in Detroit. I had become quite friendly with them. And at various stages, Harold had encouraged me to think about becoming a sociologist, and especially when he saw me moving away from my shop experience and becoming increasingly critical of my trade union officialdom experience. So Harold had kind of put the idea in my head.

I became very, very clear: I'm going to go back to the United States; I'm going to become an industrial sociologist, because that's building upon *my* experience. I should be able to do the academic work without too much difficulty, because, in a sense, there was a fair amount of academic learning within the Workers Party. That is, when there was an issue about "the American question" or "the Negro question," or whatever, there was a big debate, and everybody was seriously encouraged to participate in writing and reading. I did not write much in this period, but I read most of the argumentation. I went to most of the annual conferences that were held, so I participated in the debate. So I said, I'll go back to Wayne; I will become an industrial sociologist. And I'll have to do it very rapidly, because I'm not going to just kind of breeze through. I'm going to move through fast; I want to get my Ph.D., and I want to find a university where I can hide.

Rabkin: Hide from—?

Friedland: Hide from politics. The political experiences that I had had, had not been— I mean, the experience itself was useful, a terrific learning experience. But it was essentially a depressing experience: you joined to make the revolution and participate in it. (laughs) And if there's no revolution, what do you do? (laughs) You have to find a life for yourself. And it became clear to me that ordinary, day-to-day politics was not what I was interested in, because mostly you'd have to go and do ordinary political work at the grassroots level, and I didn't want to go through that kind of experience. I was disenchanted even with that. So I said to

myself, I'm going to get my Ph.D. as fast as possible, and I'm going to find me a university where I can hide.

And the first summer into my academic student-hood, I found that university. It was the University of Montana at Missoula. I was driving from Berkeley to Detroit to teach in the Michigan CIO summer school, and I drove across the country. I deliberately went the northern route, and I saw Missoula, and I said, "This is the place. This is so far away from everything that I can hide." And essentially, what I laid out for myself was a non-political orientation, to which I then adhered—until 1964. So, this period, 1954, I went back to school. I'm now in an undergraduate program, because when I went back to school I went back as a sophomore. And in two academic years plus one summer, I finished my bachelor's degree and my master's degree at Wayne. I was in a hurry.

Graduate Work at UC Berkeley

Harold said, "You don't want to do your Ph.D. here. You want to go to Berkeley." At that time, Berkeley was the place that was really growing, and really had a stellar faculty. So Harold gave me good advice, and he insisted, "Go to Berkeley."

So I went to Berkeley. What that meant, essentially, though, was a complete break with the Shachtmanites on the one hand and a complete break with what friendship networks I had developed in Detroit, and a complete break with the world in which I had functioned and learned to understand. So I applied to Berkeley and I got admitted without any problem. I had a first-year graduate

fellowship from the Social Science Research Council, which helped a great deal financially—which meant I didn't have to work, and, as well, still had some UAW savings. And I began to push very, very rapidly in the first parts of the Ph.D. program.

Rabkin: Were you married at this time?

Friedland: No. No. But my future wife and I were exchanging letters on a fairly regular basis.

Rabkin: Was she still in Europe at this point?

Friedland: She had quit the job in France and gone back to the UK, and gotten a job in London. But she was beginning to think about coming to the United States anyway—that is, whatever happened between us.

So I was in a hurry in Berkeley, too. But Berkeley was a very good experience for me intellectually, because Berkeley was the place recruiting a lot of smart Ph.D. students. And essentially, that great experience was an experience of my fellow students. (laughs) Since I did not have a teaching assistantship or research assistantship, I had no real access to faculty other than in the classroom. And I quickly learned that there was a kind of distinction in graduate student ranks between those who had research or teaching assistantships and those of us that didn't.

Rabkin: Did you not have a faculty advisor?

Friedland: Oh yes, a faculty advisor. My faculty advisor was Reinhard Bendix—a very eminent sociologist. And I saw him formally, probably— In that two-year period, I probably saw him half a dozen times. Bendix would work with his graduate research assistant students on a regular basis. Seymour Martin Lipset would work with *his* on a regular basis. Since I was not in that network, that first year I was essentially on my own. I also went through a—almost immediately, in Berkeley, I went through a major psychological crisis that I had never experienced before. I did have two friends—two people that I had recruited to the Shachtmanites, in Michigan, while I was there and they were also in Berkeley—and they kind of got me through the crisis.

Rabkin: They were in Berkeley with you?

Friedland: They were in Berkeley, yeah.

Rabkin: In the Ph.D. program?

Friedland: Yeah. Well, he was; she was not. But they were a couple, and they had their first kid, I think, at that time. They essentially got me through that crisis.

In the meantime, Joan, my future wife, had decided to come to the United States, and she did. She arrived in New York, I think, in December '58. Then she came on to Berkeley. We picked up where we had left off.

Indicative of my trying to get away from politics and at the same time use my skills, I had spent a three-month period during my time in Europe in a *stage*—a French word—an internship, with the International Confederation of Free Trade Unions, in Belgium. And there, too, you see, I was put off by the Social Democratic ambience. I made good friends with the juniors in the organization, and when we'd go drinking beer—this was in Brussels—at the Grand Place at the end of work, they would bitch and complain about their bosses. And I would suggest to them that they should join a union. They told me they already belonged to a union, a Belgian union. So I said, "Well, why don't you do something with your union?" And they would say, "What?" I would suggest, "Well, you're bitching about this and that. Why don't you have a picket line, organized for half an hour before work, with picket signs, and picket the headquarters of the International Confederation of Free Trade Unions? Because they're not doing this, and they're not doing that." They looked at me (laughs): "What?!" They'd never heard anything like this. So we got back to drinking beer. I was already moving into my non-political phase. But essentially it convinced me that I could not survive in a Social Democratic ambience, in a closed society, essentially. So I went home to Detroit. I did my first year in the undergraduate program. By that time I was already moving into graduate work, so I had to finish that following summer with my thesis, which I took out of my experience at the Michigan CIO Port Huron Center. Then it was literally pack up, go to Berkeley.

I very much appreciated my Berkeley experience, because it was an important intellectual experience—not much thanks to the faculty, except for the fact that they'd create this attractive pole, in effect—that drew in really good graduate students. For my dissertation, I decided that I would do a study of the new trade unions developing in the Third World. And I decided on Africa.

A Ford Fellowship in Tanganyika

Rabkin: Why Africa?

Friedland: Why Africa? Because there, the unions were newest. And I could have my choice of either French or English. And there were good possibilities. There was a militant union that had developed in what was then Northern Rhodesia, and I said, "I'll go to Northern Rhodesia, do my dissertation there." I got a Ford fellowship that would permit me to go there, that would cover me.

By this time, Joan and I were together, and I said to her, "If we get married, I can get you covered through the Ford Foundation." So we agreed to get married. We were both suspicious about whether this marriage would last, (laughs) but we decided to get married, and I got the Ford fellowship, and off we went—first to Boston, because the fellowship required that I go to Boston to spend two months with the African Studies Center there. And then, since we were going to Northern Rhodesia, I got, I think it was, four months, in London, studying Chibemba, the African language spoken on the Copperbelt of Northern Rhodesia.

And that was what I was doing: I was going to Africa, away from the United States, and away, so to speak, from the local political situation. That was very, very deliberate.

Both of us spent time at the School of Oriental and African Studies in London, learning Bemba: two students, Joan and I, were taught by a faculty member, a native informant who spoke Bemba as his mother tongue. I bought a car for use in Africa, put it on the docks to ship to South Africa, where I would fly down and pick it up. We were about, I don't know, forty-eight hours from departing when I received a notice from the Northern Rhodesian government—well, it was actually the whites in Northern Rhodesia, Southern Rhodesia, and what is now Malawi—at that time Nyasaland. The whites had created something called the Central African Federation. They made a unilateral declaration of independence from the Brits. They also set up a whole bureaucracy, and I received a notice from them saying I was a prohibited immigrant. I had previously been accepted; now I was declared prohibited, and we couldn't go. So I had to get the car off the docks, which I managed to do.

Then it was a question of, “*Now* what?!” (laughs) So I applied for Tanganyika (as it was known at that time), and Nigeria. Tanganyika came through first, so we made our plans, and we got ready to go, and then I was admitted to Nigeria, but by this point I was committed, and off we went to Tanganyika.

Rabkin: You didn't have to go through a new language training program?

Friedland: Well, yes, I would have to, because the language was Swahili. Swahili is a Bantu language, with a lot of Arabic admixture. So the idea was, I would go, and then I would, through my lessons, I would find somebody to teach me Swahili, and—

Rabkin: In country?

Friedland: In country, right. Essentially, we spent sixteen months in Tanganyika. There was a period in which I had a medical emergency and had to go home for a couple of weeks, but Joan stayed on. Turned out that her parents had been in colonial service in Tanganyika beforehand. (laughs)

Rabkin: Amazing.

Friedland: As a matter of fact, the apartment we lived in, which was in a ten-story building, the highest building in Dar Es Salaam—in East Africa, I think it was—looked down on the house that her parents had lived in. (laughs)

It was a typical field experience. It was a very *useful* field experience; I got my dissertation out of it. When it was time to go home, we went home via the west coast, so we stopped in Brazzaville, then went on to a number of other countries. Had a good visit, particularly, in the Gold Coast, now Ghana. Went back to Berkeley, got settled in, and I started to write my dissertation. And I put myself on the job market at the same time. I could have gone to [UC] Riverside, but Joan did not like Riverside, so we did not go there.

Cornell made me a good offer, so we went off— It was in the New York State School of Industrial and Labor Relations, the ILR School. ILR had recently gone through an internal reorganization. This had to do with two reports by Carnegie and Ford, I think it was, studying professional schools in the U.S., and saying that professional schools had become too professional, and that they had to get faculty who were more broad, and do their training in a more broad fashion, and therefore they should reorganize their curricula to bring in more disciplinary-oriented people.

Rabkin: What did they mean by “too professional”? Not scholarly enough?

Friedland: What they meant was that if you get into any field of knowledge, you can get down to just nitty-gritty, where you’re doing nothing but nitty-gritty.

Rabkin: Practical?

Friedland: Yeah, practical stuff. For example, all undergraduate students at the School of Industrial and Labor Relations had to spend a whole year on Social Security legislation in the United States.

Rabkin: I see.

Friedland: It was a very, very boring two-semester course. The students reacted to it continuously, but the faculty wouldn’t do anything about it. Students had to

learn all the details of the Social Security legislation, all the details—when it was changed, and why it was changed.

Rabkin: I see what you mean by nitty-gritty.

Friedland: Okay. So I was part of a package of recruitments over several years in which they were bringing in people who were *disciplinary* trained. I was particularly attractive to them because they had one guy who was a specialist on international trade unionism, and that was it. So I beefed them up by giving them Africa. I also taught in the regular curriculum, which was in the process of gradual change.

I was now a full-fledged academic. I didn't have my Ph.D. yet because I hadn't finished my dissertation, which took an additional two years. I kept on getting job offers from Syracuse, because of my African experience. I knew that the rules were "publish or perish," so I began publishing very, very fast. African socialism was on the agenda in Africa, so I organized a panel on African socialism at the African Studies Association and with Carl Rosberg, who was an established Africanist, we got an edited book out of it. So right away I'm doing publication, and I'm publishing stuff on Tanganyika, and some stuff on Africa as well.

Rabkin: Can you say a little bit about the nature of the field work you had done in Tanganyika?

Friedland: Essentially, what I wanted to understand is how Africans, having no “natural” experience with trade unions— In Britain and the United States, you grow up with it. It may be misshapen, but the point is, you grow up with it. The leadership of the African unions were very, very unknowledgeable about unionism. They had no background about unionism, no sense of labor history. So they were working their way into building their unions. My purpose was to hang out with them, follow them around, go into collective bargaining sessions, go to their meetings.

It took me approximately two to three months to be accepted. Because all their experience with white folks had been that they were part of the colonial oppressors. I came in with good trade union credentials, but even so, I had to prove myself. During that time, while I was in effect not inside, but trying to work my way in, I had the opportunity to spend some time in the railroad workshops in Dar Es Salaam. The headquarters of the railway was in Dar Es Salaam, and the workshops were in Dar, where they did whatever work was necessary on the equipment.

Rabkin: Construction and repair?

Friedland: Yeah, construction and repair. So I managed to get accepted into the workshops, and when I got into my first workshop, which was the foundry, they said, “What do you want to do?” I said, “I want to do what *they’re* doing.” I said, “I know about foundry work. I have at least some experience.” And the biggest problem I had with doing foundry work was, when they were working, they sat

on little squat stools, and I had to do that too, and my body wasn't accustomed to that kind of positioning. So that was the worst part of the job. The job itself was not all that onerous, because it was very, very slow.

Rabkin: They hadn't been visited by the time specialists yet? (laughs)

Friedland: No, no, no. (laughs) They didn't even— In their wildest dreams, the management hadn't come to that stage yet.

I agreed to spend a couple of weeks in this department and then move to another department. And when I moved to that department, the foreman would not permit me to work with the blacks. So I just kind of stood around.

Rabkin: Was the foreman black or white?

Friedland: All the upper echelons were white and the Africans were doing the dirty work. There were a handful of Indians or Pakistanis who were doing more skilled work, kind of semi-foremen, stuff like that—you know, on the way up, but frozen into the colonial pattern. So for the couple weeks I spent in the carriage department, just hanging around talking. It turned out that one of the foremen, who did not have a bachelor's degree, somehow or other he had gotten out in Tanganyika, and because he didn't have any college education, he was looked down on. He had nobody to talk to. So he started talking to me and I started talking to him, and then of course, once we established rapport, he began to unload. He had plenty to unload. So I began to get "inside" the hierarchical

system—from his point of view, but nevertheless a critical point of view because of his own treatment.

At the same time, I spent some time out with the Department of Labor, and met a Scottish trade unionist who had come out under the colonial system. Because once the Africans began to organize unions, they needed somebody who knew something about British unions. What they were trying to do was to introduce a practice called “joint consultation,” which is a British management approach to trade unions, which the trade unions kind of go along with but don’t pay much attention to in Britain. When the Africans started organizing unions, the colonial people said, “Well, let’s get somebody out who understands joint consultation, and maybe we can get Africans to go with the joint consultation rather than to unions.” Because in joint consultation, what you do is you establish groups of workers, and they meet with groups of managers, and they talk through about problems, and they consult. There’s no resolution of whatever gets raised, there are just discussions.

So this Scot—he and I got along very well, even better than with the guy without the bachelor’s degree. And he *really* unloaded to me. He unloaded to me to the point that I had to cover up my field notes, to make sure that he could not be identified. I was operating on an assumption that my field notes, when I put them in the mail, would get opened and studied. The probability is that I was wrong, but I figured that it’s good to be paranoid. (laughs) Because essentially, Tanganyika was still in the pre-independence period, you see, so we’re still working within a colonial frame of reference—although Tanganyika actually was

never a colony. It was a trust territory of the League of Nations (laughs), which had been continued with the United Nations when the League had expired. So it was a historical remnant.

I began to then shape my dissertation while I was in Tanganyika, because it became clear to me when I finally got into negotiation sessions that the African trade unionists did not then understand the nature of modern class relationships. Surprise! (laughs) Because what they would do is, if they had a grievance; they would go in and recite their grievance, and the manager, whoever they were talking to, would knock the argument down, and they would then repeat the argument. And when it got knocked down again, they would *repeat the argument*. (laughs) And one of the things you learn in collective bargaining is you use a variety of different approaches. And if you can find a weak spot, that's where you want to press in. But you don't just have one argument, because they may have a successful argument that you really can't knock down.

So that was kind of one problem that turned me on to the idea that they don't really understand that they are now in *class relationships*, and that just a recitation of a grievance does not mean that the boss will agree with you. That was then followed up by sessions in the trade union meetings—with the leadership. I also went to meetings with rank and file, but this was in the leadership, where they would use this kind of language: they would talk about the fact that there was a strike; the cops came; the cops shot and killed two people. They would use words like "misunderstanding." Two people killed, and you call that a misunderstanding?! I ran across it in the minutes of previous internal meetings.

In Swahili, in the minutes. By this time I had a local research assistant who was doing translations for me, sitting next to me, because my Swahili was not really great, but I could generally know what was going on. The main problem I had with Bantu languages was you use the negative as a prefix attached to the verb. So in other words, you have to hear that negative symbol first, then you get the verb. So I had problems with that. But my assistant was translating some minutes for me, about where the cops came and there was a “misunderstanding.” I said, “Where do you get this?” We’d start looking at the sentence, and I said, “You’re sure “misunderstanding” is what it means?” “Yes.” So what I did is I took the paragraph in which the sentence was included and I went around to, I don’t know, a dozen of the leaders individually, and I’d say, “Please translate this paragraph for me.” And they all used the word “misunderstanding.”

Well, this gave me an important clue as to their understanding of disagreements. When you have class relationships, you accept the fact that the cops are going to be on the side of the employers, and they’re gonna shoot you. (laughs) You don’t really *accept* it, but the point is that it happens, and you wouldn’t call that a “misunderstanding.” I explored around this issue for a while, and what became clear to me was that in the indigenous society—and all of the trade union leaders grew up in the indigenous society, and as school kids, some of them were very bright, so they got advanced, so they moved through an educational experience. Most of them had what might be called the equivalent of some high school. A few of them had no experience at all in an English-speaking school. So most of

them had come through with, essentially, religious training, where the emphasis is on the homogeneity of society, rather than society as conflictual.

This became the central focus of my dissertation. But the larger approach was over the issue of institutional transfer. You have a colonial period; it goes through a period in which it gets too expensive to bring out people from the home country, so you encourage the religious people to have schools, and they then create a cheaper labor force of educated record-keepers, etcetera. These are the people that actually created the nationalist and union movements. Most of them spoke English or French, so that they could deal with the colonial masters, but they did not have this notion of class relationships. The larger entity encouraged—that is to say, the colonial system encouraged joint consultation. So they're pushing all these ideas on the Africans and they don't quite understand what joint consultation is, and they're having a variety of difficulties with it.

So essentially this became my dissertation. You take the institution—joint consultation—you transfer it from Britain—and what happens to it, in effect? What happens to it is, slowly but surely, the trade unionists have to learn to ignore it. (laughs) They were on the *cusp* of that; they were in an actual transition to that. So that became my dissertation.

Rabkin: Interesting.

Friedland: And I finished my dissertation at Cornell, shipped it off—and I am now a full-fledged academic.

Cornell University and the Cornell Migrant Labor Project

Okay. I mentioned that fact that at Cornell, I was part of a cluster of discipline-trained social scientists, and we were brought in ostensibly to help revise the curriculum, make it more disciplinary oriented. Well, essentially, within two-three years it was beginning to become clear that the old faculty weren't having anything to do with this. (laughs) They were not about to change. And, slowly but surely, one of the new faculty peeled off and went somewhere else. They were still recruiting new people. I'm getting increasingly jaundiced with this (laughs) but I didn't quite know what to do. My wife does not want to go to Syracuse, she made clear to me.

Rabkin: Syracuse has made you an offer?

Friedland: Yeah. Yeah. Syracuse wanted me very badly. But because they wanted me badly, ILR gave me tenure after, I think, two to three years. So I was moving up the ladder fast.

Rabkin: They were offering you a tenured position at Syracuse.

Friedland: Yeah. So, slowly but surely, I began to get disenchanted with Cornell. I am staying with my non-political orientation. Faculty dinner parties consist of discussions such as, "Should we dig an atomic bomb shelter in our backyard? And if we do, and they drop the bomb, and your neighbor wants to get into your shelter, should you shoot your neighbor?" (laughs) This is indelibly inscribed in

my memory. You know, first of all, the ridiculous business of digging an atomic bomb shelter in your back yard. And then, shoot your neighbor (laughs) to protect yourself in these discussions, I stayed out of completely. Never said a word. So in other words, I was home. I was back in my "pot of shit." But I was not going to participate in their discussions.

And then 1964 came along. I went to Cornell in 1961. 1964 came along. Berkeley, Free Speech Movement. And then all hell broke loose. Next thing you know, students who have somehow or other glommed onto my history were coming to me and saying, "You've got all this experience, and all you're doing is you're teaching about African trade unions, which are just so remote from us that you have no idea." I knew how remote it was. The key word was "relevance": "We want you to teach us relevant topics." What could I do at Cornell that would be relevant?

Around this time, roughly sometime I think in '65, one of the Tanganyikan trade unionists came to the United States because the U.S. government was offering trade unionists trips, Labor Department trips. "Come to the United States, see our glorious trade unions, blah, blah." And one came through we spent time talking. He also wanted to see something of the countryside. So we got in my car and started driving around the boondocks. It must have already been the summer period—because we're driving along, and then suddenly, in the boondocks, we see a whole bunch of blacks sitting alongside the road—just off the road. He says to me—because Cornell has managed to get two or three black students, and that's about all—

Rabkin: In the entire university?

Friedland: Yeah. They had lots of Asians, and people here and there, but actual African Americans, they had, maybe two, maybe six. They were few and far between. So my Tanganyikan visitor says, "What are they doing here?" Because he hadn't seen any black people. I park the car on the side of the road. We stood there; we're talking, and a couple of them started walking over towards us. There was a fence, so they came to the fence and stopped. So we walked over to the fence and we started talking. Well, it turned out that they were migrant agricultural workers that had come north to harvest green beans. The harvest hadn't started, so they were just kind of sitting around waiting for the harvest to start.

Rabkin: Come north from where?

Friedland: Florida. We're talking and Alfred Tandau says to me, "What did he say?" I told him what he said. I can understand them, and when Tandau speaks they can't understand, so I'm translating.

Rabkin: Everybody's speaking English?

Friedland: Everybody's speaking English. (laughs) He's speaking Tanganyikan English; they're speaking Florida African American English. These are real laborers.

So, with the radical students saying, you know, "Give us something relevant," I said, "Ah! I got it. I'll start a field study program." I'm now too old—I'm already in my forties. I can't go out and do this, in addition to which my wife had our first kid, and the second, Fiona, was coming. I've got two very young kids, and I'm too old to do farm labor myself. How do I do it? Well, I'll recruit Cornell students in the spring semester. I'll train them to do field study.

Remember, I have sociological experience, and I've always been anthropologically oriented. And the anthropologists have always, in effect, argued that field study is *so* primordially significant that you can only do it after years of preparation. This is a Malinowskian residue—you know, Bronislaw Malinowski. Malinowski in effect invented field study. And the way he did it was that he studied for years in Britain; then he went out to Australia, and he's going to work his way into New Guinea somewhere, and he got stuck by World War I. Because he came from German-occupied Poland. (laughs) He got stuck in Australia. He finally talked the Australians into letting him go to New Guinea, and they were very suspicious of this "German." But he had a good field experience. So he came up with this methodology that: you've got to be well prepared, you've got to learn the literature; you have to learn the language. So, you know, two years, three years of preparation; then you can go out and do field study. And the idea of sending rank undergraduates out into field study—that was unheard of. I said, "I'll train these undergraduates. I'll put them out in the summers."

Rabkin: You'll take spring semester to train them, and then send them out in the summer?

Friedland: Summer. And then the fall semester, we'll do the consolidation experience. So you see, that's where the community studies format came from. The only difference was that all my Cornell students would be working on *my* research. I had a research project: I'll learn about migrant agricultural labor in New York State, and eventually I'll produce something about it. I know one thing: I don't know much about migrant labor but I know that that they're going to be exploited. That much I know. (laughs)

Rabkin: (laughs)

Friedland: The ILR school was good about one thing, and that was providing faculty with research associates. I had gotten, right after I got to Cornell, a research associate working with me on my African material. Dorothy Nelkin was her name. She had a bachelor's degree, had raised her kids to elementary school level, and then she put herself back on the labor market. She was intellectually brilliant. Dorothy began to work with me on my African material. She began to peel off some African stuff on the military in Africa, and I encouraged her; she should have something that's hers, not just working on my stuff. And when I started cooking up what became the Cornell Migrant Labor Project, I talked it through with her. She said, "I'll come with you; I'll work with you on this." I went out and talked to some growers, and asked them for access for my students.

They wouldn't even hear of it. So we talked that through— We decided to go underground.

Rabkin: Wow.

Friedland: Well, if the only way you can go in is underground, you go in. Okay? Either that or you give up. I had no other ideas about what I might work on that would be relevant. This was right in—not in Ithaca itself, but in the surrounding area. There was a regular stream of Florida migrants coming in to the harvest in that general area. And I saw possibilities of doing something that might even be policy oriented.

I recruited two Cornell students. One was a Jewish student from New York City, and the other was an African American from somewhere in New Jersey—one of the few Cornell black undergraduates. And we agreed that they would work going underground. That meant that we would have to, in effect, insert them. So we would have to have them trained as to what they would have to do. We couldn't have them writing field notes while they were in the migrant labor camps. We had already discovered that migrant labor camps have a fair amount of violence, a lot of drinking. The key exploiter was the crew leader. So what we agreed was that they would memorize the telephone numbers of Dorothy and myself, and we would cover them 24/7. Any kind of a problem, all they had to do was get to the nearest telephone booth. They always had to carry dimes with them. So we built that into the curriculum: you know: "Show me your dimes."
(laughs)

Rabkin: (laughs) Did you get any grief or skepticism from the administration about liability?

Friedland: It was before the days of regulation of research involving human subjects. Human Subjects was introduced as I was leaving Cornell.

Rabkin: This was the prohibition against using—?

Friedland: This was a prohibition that if you were doing research with people, you had to go through a Human Subjects review.

Rabkin: Yes. And this would have qualified as a Human Subjects situation.

Friedland: Oh, yeah, oh, yeah. So it was before that.

Rabkin: Okay. So it was before that. But what about, just, insurance liability? Wasn't anybody worried that you were going to send these kids into these migrant labor camps and they were going to get injured or killed in a knife fight?

Friedland: Yes, we were seriously worried about that. That's why one of the things we trained them to do was, in the first day or two in the camp, they had to know every exit area from the camp. Usually there's a road in, but there were all kinds of paths. Where are the paths? You have to know all the paths; you have to know it in the daylight and in the dark.

Rabkin: Okay. But no administrator was telling you you couldn't do this because—

Friedland: No. No. I wanted my students to get field study credit while they were in the field. The administrators said, "Field study credit? What are you talking about?" And the ILR faculty were adamant: No way.

Rabkin: No credit for field study.

Friedland: No credit for field study.

Anyhow, so we said, "All right, no credit for field study." You win some, you lose some. I mean, we didn't go around saying, you know, publicizing, "We're going to send students into dangerous situations." (laughs) We taught them that they're going into dangerous situations. You know you're going into dangerous situations. Therefore, you have to have dimes; you have to know where the telephones are; you have to know our telephone numbers; you have to know Dorothy's telephone. You get into any kind of trouble, get out of there as fast as you can, get to a telephone and call one of us. Our responsibility then is to get to them as fast as we could.

Rabkin: They were supposed to hire on as fieldworkers? Was that the idea, that they would join these crews?

Friedland: Yeah. They would join the crews.

Rabkin: Were there some white workers in these crews?

Friedland: No.

Rabkin: And one of these guys was white.

Friedland: Yeah.

Rabkin: So that sounds like it could have been a challenge.

Friedland: Well, you know, he let his beard grow a little bit, he's "on the road," he's a young kid just exploring the world, that kind of stuff. Yeah, we had no problem inserting them.

Rabkin: Okay.

Friedland: Sometimes, the field crew employers would say no, and then they had fallback. So when we inserted somebody, for example, what we did was, either Dorothy or I would do the insertion. We would find and map the camps. But the maps always had the road; we didn't go into the camps. Usually what we would do is we would map two, three, or four camps relatively close to each other. And so we would have maps; Dorothy and I would have done that beforehand. We would then train Roger and George—those were the first two

students—about the maps, you know, and scare the bejeezus out of them: “Anything happens, out of there as fast as you can! Don’t argue, don’t fight; just get out of there!”

That then became, so to speak, the regular protocol as we recruited more students in the next two sequences. So we had a one-year sequence bridging a summer, and we had a format, which was clear, and we had them writing papers in the fall semester. And they loved it! They loved it!

So the next year we had a full complement of about eight students, something like that. Anyhow, I got funded by the Labor Department, so we were set. So the ILR administrators loved me. We did one cohort with two, and then we did a second cohort with about eight. Then for the third cohort, I went down to Tuskegee and Florida State University, which is black, and recruited—I think I had four students between those two places, including at least one female student. We had females from Cornell, too, doing this.

Rabkin: Did the students from Florida and Tuskegee transfer to Cornell, or did they retain their affiliation with the Florida and Alabama schools?

Friedland: They did, and they got transferable Cornell credit. All of that had to be worked out, but the bureaucracy was much less then than it is now.

In the second full cohort, I had a sabbatical, and I decided to come to California, to get a sense of migrant labor comparatively speaking. I found myself at

Stanford. I went out and did actual agricultural labor. (laughs) By this time I'm forty-five. And let me tell you, a couple of hours crippled me up so bad— (laughs) I could tell you stories about that, but I won't. Anyhow, I didn't do that very much. (laughs) I think I did three days, with about a week between each one, recovering. My grant permitted me to fly back to Cornell a couple of times to participate in— Dorothy was now running the day-to-day activities. We walked around with a loose-leaf notebook that had all of the stuff that we needed to know, and we never separated from our loose-leaf notebooks, except if Dorothy was on and I was off, or vice versa.

Rabkin: And what was in the notebooks?

Friedland: Oh, in the notebooks were the maps, the telephones, where the telephones were located—there was no telephone in the camp, so if you got out, you had to go find the telephone. We had the telephone numbers of all of the telephones close to the campus. What else? I don't remember. In any case, you see that loose-leaf notebook up there? [Points to a large binder on a bookshelf.] The big fat one.

Rabkin: Big, fat notebook.

Friedland: Each student had one of those.

Rabkin: Wow.

Friedland: The protocol was, when they went in, we would continue to cycle around for two-three-four hours, when we would reasonably expect that they'd been accepted. If they weren't accepted, they would come out, and they would start walking along the road, and we would come by, pick them up, and we'd take 'em away. Then on to the next one. We got them all in. We had one young woman, in the second full cohort, who had some kind of ailment, and so she couldn't go out into the field. So we set up a— (laughs) —we set up a “safe house.” And she ran the “safe house.” We were operating essentially like the CIA, except we didn't have dead drops and stuff like that. But we were operating, essentially, below the surface.

Coming to the University of California, Santa Cruz

During the Stanford period, my wife and I said, “Let's go over and see the new campus at Santa Cruz.” Which was then only two years old. So we drove over. We found the campus without any problem. We drove onto the campus, drove around, and drove right off. Never stopped, never put our foot on the ground. We said Cornell is centrally isolated, (laughs) but by comparison, this place is *really* isolated.

Rabkin: (laughs)

Friedland: So we got some sandwiches; we ate on West Cliff Drive, and went home to Palo Alto.

And then what happened was that a former colleague from the ILR school who had been turned down for tenure—whom I had helped recruit, who had a brilliant theory about why the Algerian Mujahaddin were cleaning out the French successfully, and my ignorant colleagues in the ILR School turned him down for tenure. (laughs) His name was Eqbal Ahmad, and he was one of the Harrisburg Seven charged and tried in 1972.¹² They were the ones that were accused of planning to kidnap Henry Kissinger. They had a hung jury and the prosecution failed to prosecute again.

Eqbal had been invited to do a lecture at Berkeley. At Berkeley they appreciated people like Eqbal. Anyhow, I was invited to come up to a party that they were holding for him. So—

Rabkin: In Berkeley?

Friedland: In Berkeley. Yeah. So Joan and I drove up. We were glad to see him. I knew one or two people besides Eqbal. So it turned out there were a couple of people from UCSC there: Peter Kenez, and Arlie Hochschild, who had just been hired in sociology. They said, “Have you been down to see UCSC?” “What are you doing?” —you know, and I told them. They said, “Oh, you’ll like Santa Cruz!” I said, “Ah, well, we drove on, and we drove off.” And they said, “Oh, no, it’s nothing like that at all. All the students go running off campus, and are

¹²The Harrisburg Seven was a group of religious anti-war activists during the Vietnam War era. Ahmed was a Pakistani journalist.

engaged in all kinds of activism,” and so forth. “All right.” (laughs) We went back home, and thought that was the end of that.

Founding Community Studies at UC Santa Cruz

But that *wasn't* the end of that. Peter Kenez mentioned to Dennis McElrath what I was doing. So, on one of my trips to Watsonville, Dennis and I had established a connection, and Dennis, who was chair of sociology at the time, said, “Stop by, and we’ll talk a bit.” So I did. He said, “You ought to come here.” He said, “Let me invite you down for a colloquium.” And he did. I came over from Stanford, did the colloquium on the Cornell project. That was the beginning of my recruitment to Santa Cruz. Arlie and Peter were right, all the students were running off campus, doing all kinds of crazy things and getting academic credit for it! And not *producing* anything for it. So I think what was happening was, Dennis and a couple of the academic cooler heads knew that there would eventually be trouble, academically. You know—you’re giving credit; what do they *do*? They *say* they do something, but—

So that got built into the curriculum. They wanted me to design a program which would require full-time field study. So I took the Cornell model—prep, field study, consolidation—but I made one big difference, and that is, students wouldn’t be doing my research. The students would choose the area they wanted to do their field study in. And that was agreed upon. And then Dennis started the bureaucratic process of getting me recruited.

Rabkin: Did that involve proposing a new program or department?

Friedland: A new department, called community studies. I had nothing to do with the choice of the name. They had already decided the name choice. There were a number of people that were interested in it. David Kaun, in economics. And Ralph Guzman. And, Bill Brown from geography.¹³

Rabkin: Would you have proposed a different name, other than community studies?

Friedland: The name was an inconsequential issue for me. "Community studies" was sufficiently vague, but it covered what we were planning to do.

Rabkin: (laughs)

Friedland: I set out the basic orientation, the three basic parts. I set out the notion that students will choose their own field study. It's their responsibility. There are two limitations: it couldn't be on any campus, and it couldn't be illegal. And then the other element was, we want to emphasize students going to communities that the UC has pretty much overlooked, which meant poor people, racial minorities. But we do not dictate to students where they go. They choose.

¹³ Friedland clarified that at one time a geography board had been planned at UCSC but these plans did not come to fruition.

Rabkin: Did you provide listings of possible placements, or did students have to dig up the possibilities all on their own?¹⁴

¹⁴ Bill Friedland provided the following historical note during the editing process:

Before I get into the next issue, student representation on the Board of Community Studies, I should tell you about what happened as the academic year 1969-70 opened and how community studies began with a freeway study.

Students had come back to campus after a summer away, classes had not yet started when the *Santa Cruz Sentinel* carried a story about a community meeting being called by the California Department of Transportation to be held to discuss the issue of freeway location. CalTrans had been planning a freeway to connect Highway 1 from the north with the existing Highway 1 freeway heading south. CalTrans was proposing several alternative routes through the town and wanted a community expression as how people felt about the alternatives. At this stage I knew nothing about the Santa Cruz community and local politics so I decided to attend to find out what was going on.

When I arrived at the Civic Auditorium I found the CalTrans commissioners were sitting on the podium facing the audience. A table to their side held the members of the City Council. The auditorium was pretty solidly packed and included a sizeable number of what were obviously senior citizens, many of whom were holding signs with labels such as "No Freeway" and indicating displeasure with route that was favored the overwhelming members of the Council. With one exception, the Council members favored a route that would run close to the Boardwalk and to downtown Pacific Avenue, the main business street in the city. I also learned that this route would require exercising eminent domain that would disrupt the lives of hundreds of senior citizens who lived in the freeway's path. I subsequently learned that the one council member who did not agree with this route was the one woman on the council and she had organized the seniors to come and protest the route.

The hearing itself was fairly predictable. The council members spoke for economic development and the need to support local businesses and the single opponent spoke against the route that the council majority favored, pointing out the damage it would do to the lives of so many senior citizens. At the end of the hearing, the commissioners expressed their uncertainty; it was clear where the majority of the council stood and that most of the citizens in the auditorium did not favor the downtown route. The commissioners told us that the record would be kept open for thirty days and any expression by anyone should be put in writing and forwarded to CalTrans in San Francisco.

I saw an opportunity to put community studies on Santa Cruz's map within thirty days if we could organize a freeway study and file a report on how the citizenry felt about the issue. I returned to campus, where students were getting ready to start classes in the new academic year and grabbed Dennis McElrath, the chair of sociology. Dennis had been the key figure in getting community studies set up on campus and also in recruiting me. I knew little about the campus' organization but Dennis, who had been here at the start in 1965, knew it very well.

We knew that any such study would have to be quantitative and we then turned to Marshall Sylvan, a statistician who was associated with the Mathematics Board of Studies. The three of us were located at Stevenson College and our offices were almost next to each other. The three of us met immediately and before the day was over, we had arranged to paper the campus with an announcement of the possibility of a study and invited interested students to turn up for a meeting in the next day or so.

Some several dozen students turned up to hear what was going on. We explained the issue and told the students that, if anyone was interested, we would organize the study with an impromptu class offering five units of credit; students would have to work full time on the research but it would be over within thirty days and they could then turn to the other classes within which they were registered. We ended up with twenty-eight students.

Essentially, what we did was to explain how survey research was done and as we passed through each stage of the research, we would tell the students: how a study would be designed, how a random sample of the Santa Cruz citizenry could be drawn; how the study would be explained to citizens when students went to do interviews and what to do if people refused to be

Friedland: We didn't even *have* to do that. Students would almost always come in: "I want to do this," you know, "I want to do that, I want to do the other."

Rabkin: So they always had the initiative.

Friedland: There was always a few students who said, "I don't know what I want to do." (laughs) And, at least in the early years, we would spend time with them, trying to get them to articulate what they were interested in. After about several years of that, we finally established a requirement: "You want to come into this major, you have to prepare a proposal. Because we don't want to go through that business with you."

Rabkin: I see.

involved and how substitutes would be interviewed; the interview questions would all be coded; a pre-test would be taken and the questionnaire might be edited and revised; we decided that we wanted to have 1,000 interviews because we wanted a large sample which would make the data more convincing; we explained how an interview would be conducted and how students should mark the questionnaire so that the responses could be keypunched on to Hollerith (IBM) cards; once the cards were punched, we showed the students how to develop tables showing citizen responses which could be cross tabulated with age and other variables; and finally, how to write a report that could be used locally and delivered to CalTrans in San Francisco.

The twenty-eight students who signed up for the project worked phenomenally on all aspects of the study. They also recruited their friends when we came to the interview phase and got them to volunteer to do interviews. These friends were also trained by us three faculty members of the study. The cards got punched, tables run, the data were analyzed, and the report was written and delivered in San Francisco in twenty-eight days.

The results were clear: an overwhelming majority of the respondents opposed the route favored by the majority of the City Council. The main consequence was to kill the proposed route the council majority wanted. What we also learned was the staggering capacity students had for work when they were well motivated and had an imminent deadline. It also led some students to decide to major in community studies. Finally it demonstrated to the campus the capacity of community studies to perform research having a significant impact in community decision-making.

Friedland: If you can't get it together enough, you don't really belong with us.

Rabkin: So ultimately, you did not admit students to the major unless they submitted a satisfactory proposal about field study.

Friedland: Yes, they had to be *clear* what they wanted to do. So we were not going to hold the hands of the occasional student who didn't really know what they wanted to do, but the *idea* of community studies enthralled them. "Okay, it's a terrific idea. Go away and figure out what it is you want to do, and if you can't figure it out, we won't let you in."

In other words, as we moved through the process, we began to layer on more requirements. The first year or two, a student would come back from field study and we'd say, "You have to write a senior thesis." "Oh, I can't write a senior thesis." "Why not?" "Because the experience was so, so *remarkable*; how can I encompass it in words?!" (laughs)

Rabkin: (laughs)

Friedland: And that then became, you see, a serious element. Because I said from the very beginning, we *have* to have a senior thesis requirement—and this was not a hard issue to convince people of, because the senior thesis was, at that stage, required almost everywhere at UCSC. I just glommed right onto that, because the idea was—especially after that first student came in and said, "I can't write about this," we then defined it. The senior thesis has to come out of your

experience, and it has to be tangible and communicable. You can do any kind of thesis you want. You can do it as a social science senior thesis, a work of fiction, a collection of short stories— We even had one woman who did it in art form. But it has to be a manifestation of your learning experience.

Rabkin: Visual arts?

Friedland: Yeah. It had to be something that was tangible. You could *see* that the student learned something. So that then became part of the requirements, from the very beginning.

Students at the very beginning also came in and said, “We want to have representation on the board.” These were boards of study—remember boards of study?¹⁵ “Sure. How many representatives would you like?” “Ah, we don’t know.” (laughs) “Well, go away, and tell us how many representatives you would like.” And they come back: “How many representatives would you like?” “Same number as the faculty.” We said, “Oh, no. You can have any number you like, but you cannot have the same number. You can have more, you can have less, but you can’t have the same number.” See, the same number set up the notion that students have a different interest than faculty.

¹⁵ In the early days of UC Santa Cruz, the emphasis of organization was given to the colleges. But some people were concerned about bringing faculty that had disciplinary interests together and this gave rise to the formation of boards of study. That designation was utilized to downgrade any idea about departments, which at other UC campuses and generally in the U.S. university system, were the dominant form of organization. For a period of about ten years, boards of study served as the equivalent of departments but the boards became increasingly influential and finally were renamed as “Departments.”—William Friedland.

Rabkin: How so?

Friedland: Well: "We represent students; you represent faculty."

Rabkin: I see. The faculty team, and the students' team. Five on five. I see.

Friedland: Yeah. And we weren't prepared to get into an argument. If they wanted a majority, I mean, what do we care? If we can't be smart enough to convince them, then we deserve to be beaten! (laughs) We were quite open about it. We didn't make any big fuss about it. I think we had seven faculty at [the time]. The instant they said, "Seven," we said, "No. You want eight? That's fine." So, right from the very beginning, we had student participation. And as a matter of fact, several of the students who were board members in the first full year, remain loyal, dedicated (laughs) alums. Because that was a primordial, formative experience for them. And especially the notion that you can have one or two, but we're going to outnumber you.

Rabkin: What were those relationships like, on the board, among the faculty and the student representatives, and the staff?

Friedland: We didn't have many arguments. I mean, we had some arguments. I can't, frankly, *remember* those arguments, to be perfectly honest. The main argument was, what number? (laughs) We were oriented to a consensual frame of reference, to begin with. As, we're all in this together. You have this as your particular focus; this student has this as their particular focus. We, the faculty,

are not going to say, "This is better than that." We, the faculty, were sufficiently mature that— We didn't know *the best way* to achieve social change in California. And we didn't want to discourage students who were really enthusiastic about doing something. We *wanted* them to be engaged with what they were interested in. And engagement means that some of them are going to do things that we were not particularly happy about. Somewhere, probably in, I don't know, year seven or eight or somewhere in there, we had an older student, a returnee, who wanted to do something with—something like the Chamber of Commerce. He was mainstream oriented. We let him enter: "You *really* want to do that? Okay." And he finished fine, no problem.

Oh, I have to tell you, in either the first year or the second year, Chancellor McHenry called me in and said, "The president of Safeway has called me, and has asked the question: 'What are UCSC students doing picketing my stores?'" (laughs)

Rabkin: (laughs)

And McHenry said to me—what he said, really, was, "Educate me." And I explained to him the character of the department—the major, the requirements. I emphasized the fact that we did not instruct students where they could do their field study. *They* made the decision. As soon as he heard that, McHenry said, "That's fine. I'll get back to you." He sent me away. That was the last I heard of it. The fact that students chose—that was their academic freedom. So it was no problem for McHenry. I don't know what the president of Safeway did about

that, but we had other fish to fry. We got some flack from time to time, but that was the best argument that we could have, because students are entitled to academic freedom, too. And if they wanted to do something, as long as it was legitimate—that is to say, legal (laughs) but not on a university campus, they could do it. Anyhow, that was essentially the way the whole thing worked out.

In my last year at Cornell, I essentially commuted to Santa Cruz, I think, about three or four times. What I would do was, I would come out— The first session was details of recruiting me, and the conditions. I insisted on a promotion, so I got to be full professor. Oh, I also—this is exemplary of my distrust of complex institutions—I said, “You want to recruit me? I want to be professor of community studies and sociology.” I didn’t tell them the real reason. And the real reason was, I didn’t trust the institution to just fold up community studies within a year or two.

Rabkin: You didn’t trust that they would not do that. You thought it was possible that they would fold it up.

Friedland: That they might do that. Oh, yeah.

Rabkin: So you wanted a dual affiliation.

Friedland: I had enough experience with Cornell academia, and with my experiences at Berkeley, as a student, to know that these— And, also, my factory experience, you know, with Hudson (laughs), and all this experience—not

personal, but, you know, with Ford, and General Motors, to know that you can't trust big institutions. They will tell you something, and the next thing you know, the person that has promised you something sworn on a Bible about it, that this will never happen, that person has gone, and somebody's replaced them and says, "I don't care what that other person said." So I said I want to be professor in both departments. I will participate in both departments, as well as in Stevenson College. (laughs) And I said, when we recruit new people, you have to agree, they will be professors of community studies *and* whatever their discipline.

That was agreed upon in principle. It worked only for a couple of years. And then, when we wanted to recruit Carter Wilson, we wanted him also to have a seat in literature, but literature would not agree. We wanted him so badly that, essentially, we compromised that issue. After that it became standard: community studies only. But that will explain to you my distrust of institutions, which has been reliably proven over the years—including in the demise of community studies.

Now, I had no problem with being in sociology and with community studies—until sociology started a graduate program. And when they started a graduate program, then I— By that time I had stopped being chair.

Rabkin: Of community studies?

Friedland: Of community studies. And I then put my energies into building the graduate program in sociology for several years, until— [Pauses; Laughs]

Rabkin: You're rolling your eyes.

Friedland: —I don't want to go into that. Anyhow, I reverted to community studies.

Studying the Social Impact of the Mechanized Harvesting of Tomatoes

And at that stage, I was beginning to really build my own research program. Because when I came here, my research was on migrant agricultural labor. During the Stanford period—I learned, first of all, there are an awful lot of agricultural workers that are non-migrant, and there are very few blacks. There's a strong ethnic division, because there were many Filipinos still left. They were very old, already, but there were also some younger Filipinos. And the most important lesson that I learned was you can't study agricultural labor in California without understanding agriculture. I had to broaden my whole approach. That led me into the first major research program, which was the study of processing tomatoes, and the development of the tomato that could be machine-harvestable, and the machine that could harvest the tomato.

Rabkin: Shall we talk about that?

Friedland. Sure.

Rabkin: Great.

Friedland: All right. Because that was me breaking out of an exclusive labor focus. In order to understand that transition, I had to understand, first of all: what are tomatoes? (laughs)

Rabkin: (laughs)

Friedland: What's the system? How does it work? Just previously, in 1964 to '69, processing tomatoes had gone through a harvest mechanization transition. In about a five- or six-year period they went from zero mechanization to 100 percent mechanization.

Rabkin: Wow.

Friedland: One of the fastest ag-mech transitions that ever took place. I had to understand why it was so fast. And one reason why it was so fast was, in 1964, the Bracero Program ended. Now, the Bracero Program was a program bringing Mexican workers to California and Texas and other places, for specified periods of time, to work and get paid specified wages, and at the end of that period, they were supposed to return to Mexico. And most of them did. And those that didn't were illegal. That was the beginning of illegalization. But in the meantime, they had deposited, all over the state of California, stable workers who had their families in California. They might migrate for a day or two, or three, but most of them had developed a regular sequence of work.

Rabkin: Following the crops?

Friedland: Following crops in the immediate neighborhood, so in many cases they never left home.

Rabkin: They were sleeping in the same place?

Friedland: Yeah. Because that was the definition of a migrant worker: somebody who spends three nights— This was the formal definition, U.S. Department of Labor. I've forgotten the number of days. Somebody who spends so many nights away from home, working. Lousy definition. So I had to understand all of this business. I had to understand how the two key components of this process cooked up at Davis where the machine guy was inventing a machine that could harvest a soft thing like tomatoes, and a plant guy was developing a vine that would be capable of being harvested by a machine, and that could beat the hell out of the tomatoes without killing them. How that happened and the whole politics of it. So we're dealing with politics; we're dealing with international agreements, because the Bracero Program was an international agreement between the United States and Mexico.

I found myself dealing with a much broader subject than migrant labor. I'm looking at social consequences, because, you see, the transition in California was so dramatic that at least two articles—actually it was more than two articles, but two scholarly articles—appeared in the literature explaining how this

mechanization transition was the cat's whiskers. It was a great victory. The ag economists loved it because they could say, "Look, it cost so many dollars to develop all this, and look at the millions of dollars we've gotten back." That was called "the returns to research." So, in effect, you could put dollar figures on this very nicely. And you could say, "Wow, big success, see?"

Rabkin: So the research that made this possible, both the plant genetics research and the mechanization research, had been done at UC Davis?

Friedland: Yes. And the two guys that went through this— The first guy, who was the genetics guy, he got into this because he was an Extension person, and as the Bracero Program started getting close to its end, the tomato growers would say to him, "Why don't you do something useful for us?" They recognized that they were going to lose their labor force eventually.

Rabkin: Because—

Friedland: Because the Bracero Program was going to end.

Rabkin: I see.

Friedland: It had been under attack ever since the end of World War II.

Rabkin: And the tomato industry was dependent on Bracero workers for harvest?

Friedland: Well, without Braceros, nobody knew how they would get all those tomatoes harvested.

Rabkin: There wasn't another source—pool—of labor?

Friedland: No. No. No. So, as a matter of fact, in 1964, when the Bracero program ended, we had only a handful of machines adopted. Sixty-five, '66, '67, '68. I can pull out the study and show you the number of machines and how they went to almost 100 percent in that short period of time.¹⁶

Rabkin: Almost 100 percent of the California tomato crop, harvested mechanically.

Friedland: Processing tomatoes, yeah.

Rabkin: This is harvesting and processing?

Friedland: This is harvesting *for* processing. Not fresh market. Later on, they started using the machine for fresh-market tomatoes. That's a different story, because, you see, you *have* to harvest processing tomatoes ripe, whereas you cannot harvest fresh tomatoes ripe, because the machine beats up the tomatoes, no matter how good the machine is. So they had to set up this process of: the

¹⁶ William H. Friedland and Amy Barton, *Destalking the Wily Tomato: A Case Study in Social Consequences in California Agricultural Research* (Davis: Department of Applied Behavioral Sciences, Monograph Number 15, 1975).

machine harvests them, and they move the tomatoes immediately to the processor.

Rabkin: So it's an integrated process, harvest and—

Friedland: Yeah, it's an integrated system. Before, they were carrying small buckets. (laughs) In addition to which, you see, the cost of the machine then shifted the character of the growers—because if you were going to invest in a machine, you had to get your investment back. And in order to do that, you had to increase your acreages. So what happened was, the transition involved going from 50,000 workers to 18,000 workers. A very substantial drop. But it also led to going from 4,000 growers to 600 growers.

Rabkin: Wow.

Friedland: Which then served as the basis for the suit that CRLA [California Rural Legal Assistance¹⁷] organized against the University of California.

Rabkin: Tell me about that suit.

Friedland: Oh, yeah. I had a typical academic orientation. I saw some possibilities of doing this research, and maybe it would be useful, all right, somewhere.

¹⁷ A public service legal organization aimed primarily for farmworkers and other rural residents—William Friedland.

Rabkin: How did you envision it might be useful, socially?

Friedland: I didn't know.

Rabkin: Didn't know?

Friedland: No. I had a typical academic— Most academic activists say, "If I call the world's attention to this problem, the world will pay attention to it and do something about it."

Rabkin: And how and why did you see this transition to mechanization in tomatoes as a problem?

Friedland: Well, mainly I saw it as an *academic* problem. That is to say, these two articles that I mentioned, they said, "Cat's whiskers!" Okay? And I wanted to show, when you go through a transition like this, it's *not* just cat's whiskers. There are going to be other social consequences. Let's find out what all the social consequences are. I knew, from my industrial experience, that workers would be exploited. I *knew* that. But that's all I knew. I didn't know that growers would have to get out of the industry unless they wanted to get bigger in tomatoes. I didn't know that.

Rabkin: Yes. Right. Right. Which is where the lawsuit ended up focusing. I'm just thinking about the labor end of this particular problem, though, which is

interesting, the way you describe it. Because we're not talking about workers in the United States, permanently in the United States, being deprived of jobs by this particular mechanization—because in fact, the workers who'd been harvesting these tomatoes had been being imported through the Bracero Program, which was ending. And so there was in fact going to be a labor shortage. So the fallout for labor, whatever it was, was not straightforward.

Friedland: No, but you see, what a normal economist will say is, "If you don't have enough workers, what you have to do is raise the wage." Right? Supply and demand. If the demand is great, and the supply is weak, you increase the price of labor, and then you will attract people.

Rabkin: And then people living in this country might actually want to do the job.

Friedland: Yeah. Yeah.

Rabkin: Thank you.

Friedland. That was the way they were thinking. And that's not the way the world works. Once they actually cut off the labor supply, the growers had all kinds of problems. One of the things they did was they began to recruit more women. Up until that time, *braceros* were all males. Now they had to recruit women, because women were in the household in the U.S., and therefore available for labor. Not all of them, obviously, but enough of them that you

could begin to draw upon that labor supply. But the key element was to keep the price of labor low.

Rabkin: Yes.

Friedland: Now, that was only part of the consequences. The decrease in the number of growers flowed from the capacity of the machine. If the machine could harvest seventy-five acres, then what that meant was, if you wanted to buy a machine, you'd better plan to plant seventy-five acres. Immediately, that began to change the process among growers. And that was where Ralph Abascal from California Rural Legal Assistance [CRLA] figured he had the hook to bring a suit against the university. Because here, the University of California was not only getting rid of workers, they're also getting rid of growers. He was the general counsel of CRLA. And he read our piece "Stalking the Wily Tomato," and in it he saw a hook. (laughs) Because if you look at the original legislation, the intent was not to decrease the number of growers, it was to *sustain* the growers! Because, you see, when the agricultural legislation was adopted in the 1860s and 1880s and later, what happened was, in American history, people would emigrate from Europe, and many of them went into agriculture in the U.S., because they were doing agriculture wherever they came from.

They went into the cities. And they went into the cities because city life is not only better, but wages are better. So the whole ag economic orientation was, if you can lift up material life in the rural sector, people will continue to stay in the rural sector. Well, that turned out to not be true. But the whole apparatus of the

U.S. Department of Agriculture has been to try to improve the standard of life of rural people. Well, population kept on escaping. In World War I they said, "How're you gonna keep 'em down on the farm once they've seen 'Paree'?" Urban life is a lot more attractive than the rural life for most people.

Okay. So the whole orientation was—and the orientation was consciously developed in the University of California, because the state legislature gave them funds to eliminate labor in agriculture. That is, the research at Davis was paid for by the state legislature, which had given more money to UC to do research on agricultural mechanization. And when I was just in my early days here, I made a number of trips up to Davis, and Isao Fujimoto¹⁸ at Davis would say, "Let's go look at the Agricultural Engineering, so I can show you what the ag engineers are doing." And we went and we saw, for example, a machine that would mechanically harvest peaches! [Pauses] Now, it didn't work, but there was this guy, a regular member of the faculty there, you know, working on this machine, spending I don't know how many thousands of dollars. The machine was obscene, because [if you] turned on the machine, they would blow up these kind of arms, with little barriers, and they would penetrate into the peach tree, and when you would shake the peach tree, the peaches would fall off and roll down the arms, okay? And watching this machine with, I don't know, dozens of these penises suddenly becoming erect—

Rabkin: (laughs)

¹⁸ Isao Fujimoto is a well-known, now emeritus professor at UC Davis who founded, among other things, the UC Davis Graduate Program in Community Development.

Friedland:—Oh, wow. (laughs) That was a real experience. (More laughter)

So Abascal saw the possibilities. I did not originally see these possibilities. When Abascal called me and said, “Let’s talk,” what Abascal wanted to do was to bring a number of sympathetic social scientists into discussions about how CRLA could manage a major suit against the University of California. And he recruited at least half a dozen of us, maybe a little bit more. And we then worked collaboratively with the CRLA people. They would convene us from time to time, once or twice a year, until they actually brought the suit. I spent five days (sighs) testifying, (laughs) as an expert witness. That process with CRLA went on for about seven or eight years. Because there was a lot of prep work; there was discovery. I worked on discovery with them. You know what discovery is? Okay. I had written something else called “Social Sleepwalkers.”¹⁹ It was a very small monograph, in which I argued that the University of California should set up an entity within the university, an academic entity, I called the “PEMU”—PEMU. What did PEMU stand for?

Rabkin: Sometimes acronyms outlive their origins!

Friedland: Yeah, right. It was to set up an evaluation—Predictive and Evaluative Methods Unit. Okay? The PEMU would consist of social scientists who would work with ag scientists who wrote proposals to get funding for research. There

¹⁹ William H. Friedland, *Social Sleepwalkers: Scientific and Technological Research in California Agriculture* (Davis: Department of Behavioral Sciences, Monograph Number 13, 1973).

was always one question: What good will this research do? They had developed a formula: "Feed the hungry millions." If you read the proposals on file in Berkeley—and that was part of the legal discovery, and that was part of my contribution was to read proposals, and sure enough, they all used this argument, that this will "feed the hungry millions"—because they were all locked into the Malthusian arguments, you know, the population is exploding—"So the Malthusian outcome won't catch up with us." So I was asked to study a whole bunch of these proposals to see what themes emerged, and that one emerged in almost every single proposal.

Abascal and the lawyers were trying to figure out how to frame the *legal* issues. We were not useful to them on that, but we were useful to them on dealing with social science issues. The first trial went into mistrial when the judge almost died. So there was a second trial, and in the second trial, the judge said, "I knock out this argument, I knock out that argument, but that argument I sustain; this argument I sustain." Then he asked both sides to be prepared to answer the question of relief. That is to say, if there's something wrong, there's got to be relief to deal with that. I had written my first monograph, "Social Sleepwalkers," about setting up a prediction and evaluation unit, saying, "Look, we're now doing environmental impact analysis; there's no reason why we can't do *social* impact analysis." And so Abascal read that one, too, and said, "Yeah." That then became the central argument that CRLA put forward as the relief that they asked the judge to impose on the university.

Rabkin: Some kind of funding for social impact prediction?

Friedland: Yeah: funding for a new entity within the university to do social impact analysis and prediction and evaluation. In other words, you helped the original writers of the proposals to formulate reasonable predictions as to what will happen, and then five years later or so, you evaluate to see what actually happened. Got it?

Rabkin: Yep. Thank you.

Friedland: All right. So the PEMU became the relief argument. And that was done with a group of lawyers and social scientists meeting at Harvard. (laughs) We met at Harvard for a couple of days, and they came up with that as the—Relief's the wrong word; I've forgotten the word. *Remedy*—I think that's the word.

Rabkin: Ah. Okay.

Friedland: As in, what's the *remedy* for this?

Rabkin: Okay. Like mitigation.

Friedland: Yeah. Yeah.

Rabkin: Why, by the way, was your piece called "Social Sleepwalkers"? What did that refer to?

Friedland: Oh. Arthur Koestler, the Hungarian writer, had written a book called *The Sleepwalkers*, which I read, and this had to do with the scientists in the early stages of the Enlightenment—people like Copernicus, and Koestler labeled them “sleepwalkers.” Because, “They didn’t really know what they were doing. They were just studying things, and they kept on finding things that flew in the face of Catholic cosmogony. They weren’t really interested in upsetting the social order.” So he called them “sleepwalkers.”

This came to me when I had a couple of students I sent to work in labs at Davis, when I was just beginning to get my feet on the ground as far as research was concerned. I couldn’t go to Davis because I had too much community studies things to be busy with. So I sent them there. I said, “Your job is— I’ll get you in there. You’ll work in the lab of a scientist. I want you to keep field notes; tell me what happens day by day.” What happened day-by-day was clear. The Davis researchers really didn’t know what they were doing. They were sleepwalking. That is, they had an idea about something, and they wrote it up, and then they would go and ask for research money through the experiment station. And usually, they got that money. They really didn’t think through, if you succeed with this research, what will the consequences be? The best they could come up with is, “Feed starving millions.” That was the best thing they could come up with. But that was a vacuous kind of consequence.

So my idea was, well, if you can do environmental impact analysis and that can be academically legitimate, why can’t you *social* impact analysis? Of *course* you

can. And, of course, what you will find is, when you do it, you're going to make mistakes. But if you evaluate over a period of time, systematically, what happens is, you can learn where somebody who did a prediction made a mistake—forgot this variable, or did not have appropriate knowledge about something that was not obvious. So over a period of time, you build up a body of knowledge. It was academically legitimate, but the University of California didn't want that. (laughs)

Rabkin: But this court case ended up *requiring* that there be a certain amount of that study?

Friedland: The judge ruled, so he wanted a remedy. We met in Cambridge, and we talked this through, and the lawyers put it into legal terminology, but it was based on the *Social Sleepwalkers* monograph. In the meantime, the University appealed. On appeal, the judge's ruling was knocked down. And the question became—you go up to the next stage, the California Supreme Court, and at that stage we would be dealing with a Republican governor's [George Deukmejian] appointees. He had the majority of appointments on the State Supreme Court. Reagan was the president; he had the majority of appointments in the U.S. Supreme Court. So CLRA dropped the case. It would have been knocked down at the state level, and to go to the U.S. Supreme Court—maybe they would have accepted it; maybe not, but CLRA didn't see a prospect of winning this case, for political reasons.

Rabkin: Did the publicity that the case received have any social impact?

Friedland: Tremendous publicity. Tremendous. The case has never really been written up, except in law journals.

Rabkin: That's it?

Friedland: Yeah.

Rabkin: So, when you look back on this experience, what's your assessment of the effect of your research, and of this case coming to court?

Friedland: Well, my assessment is roughly something like this: You want to produce change, there's not one way to produce change. Abascal figured out a *legal* way. I never would have figured that out. So that was a good experience, because it proved to me that researchers and lawyers could talk to each other if there was enough interest on both sides. So you have to have other kinds of approaches. For example, at one stage, I tried to start a *legislative* approach—that is, to try to get the legislature to say to the University of California, "You've got to do research that will affect people other than agricultural growers." And I can give you the idea I wrote with one of my students—it's called "Production or Perish"—in which we laid out a legislative strategy by which the legislature could assess whether or not the university was dealing with other constituencies besides growers.²⁰ It didn't work. Why didn't it work? Because I was not

²⁰ William H. Friedland and Tim Kappel, *Production or Perish: Changing the Inequities of Agricultural Research Priorities* (UC Santa Cruz: Project on Social Impact Assessment and Values, 1979).

prepared to spend my life in Sacramento, in the halls of the legislature, bird-dogging legislators. I was congenitally unfit for that kind of activity. I don't know what might have happened if I had started doing that, but I cannot think of a worse kind of experience for the rest of my life. (laughs)

Now, in that particular case, I was hoping that somebody would pick it up. I mean, it really, really laid it out, specific goals— We didn't lay out, "It should be 76 percent," but we said "x percent should be dedicated to this particular kind of goal; y percent should be to this particular goal," etcetera.

Rabkin: Percent of—?

Friedland: Percent of the research money. I can give you a copy of "Production or Perish."

Rabkin: Great.

I'm going to have to wrap up for today.

Friedland: Yeah, me, too.

Rabkin: That was a quick two hours.

Rabkin: Okay, this is Sarah Rabkin; it is Monday, November 26th. I'm with Bill Friedland once again, in his office at College Eight, for our third interview. And,

Bill, when we left off at our last interview, you had been talking about the California Rural Legal Assistance lawsuit that you worked on with Ralph Abascal, and to which you provided research. That was the suit that accused the University of California of illegally using taxpayers' money on harvest mechanization research, with the result of displacing small tomato growers—reducing significantly the number of growers in the state, which had serious impact on rural life in the state. That suit, as you said, had some initial victories, but it eventually lost on appeal. At the same time, it brought to the attention of the public some of the issues having to do with the relationships between agricultural industry and the University of California, and conditions of rural life in the state.

Friedland: Yeah.

Rabkin: And you talked, in connection with that, about your research that you provided to CRLA in support of that suit, on the kinds of research being done at land-grant universities. And we talked about your “social sleepwalkers” term, and the title of the monograph by that title that you published.

More on Community Studies

So let's move on from there, then, to talk about your relationship with research in general, and research and teaching, respectively, as they related to your interest in social change.

Friedland: Yeah. Okay. Getting community studies started occupied me for several years. For one thing, we were the first interdisciplinary department at UCSC. There were a lot of people that simply did not understand interdisciplinarity on the one hand, and more people who did not understand the notion of putting students out into the community, and saying to them, "We want you to make your experience *out there* sufficiently cohesive so that you can produce a product, a senior thesis reflective of your learning experience."

The success of that orientation became clear with the very first external review that community studies had, when, I think it was a committee of three, came to assess community studies, interviewed everybody. At one stage we said to the committee, "Here is the library of senior theses. Why don't you just pick out some at random? If you want a particular topic, we can dig that out for you. And why don't you take half a dozen of these senior theses with you at the end of the day and take a look at them, and get some sense as to what the product is."

Rabkin: How many years into the major was this?

Friedland: This was, I would say, probably about six, seven, or eight years into the major, when the campus was just beginning to move into the external review process of all academic units.

Rabkin: And this external review committee was made up of academics from other institutions?

Friedland: Right.

Rabkin: And were they, themselves, interdisciplinary, as a committee?

Friedland: Ah— (laughs) Usually, they had some kind of mix. But the way the system worked was, we the department suggested some nominees for the committee, and the dean would consult around the campus, and ultimately the dean would choose the committee. So the department did not choose the committee. We could nominate potential candidates.

And from the very first external review, the reviewers in effect said that these senior theses were almost on a par with Master's theses. So that, in effect, legitimated the academic quality of the program. And this was from outsiders.

Rabkin: Interesting. So even though your students in community studies had the option of creating senior theses in a variety of forms—as you said, they could be representations of what they'd learned; they didn't have to be pieces of social science research—it sounds as though a number of students did in fact conduct social science research, and write it up in a scholarly form that impressed these committee members.

Friedland: Yes. Also one of the early developments of senior theses was some kind of *visual* presentation, which was also kind of path-breaking, in the sense that this was before students began to make all kinds of audiovisual materials. So we were moving in that direction at a very early phase. And the reaction of the

external reviewers was that this was quite notable, so that we felt legitimated. And that process was maintained until—I would have to dig out the year—the pressure of carrying the student load, and getting senior theses from the students, became increasingly problematic, because there were simply too many students. So we began to experiment with new ways of substituting the senior thesis. The senior thesis remained an option, so students could choose that. And we went into many discussions about, should we have an honors track with a senior thesis in it? Ultimately, we had three or four options that students could choose, representing their concrete learning as a result of the experience.

Over the years, we were a relatively normal department. New faculty had to be hired, faculty had to be reviewed. We acted like any department on campus. New interdisciplinary departments became created, so that we were no longer the novelty on campus. People still, very often, did not understand the fact that we wanted students to go off campus and *participate with some kind of community organization*—that students were not going to be running around loose. And as a matter of fact, we said to the students, “We don’t want *you* to create an organization, because if you create it, then you’ve got a responsibility to continue it. That takes too many years, and we don’t want that to happen. So we will have some person in the organization, in the community group, who will be your academic sponsor, as well as having an academic advisor on campus.” Later on, that had to change some more.

A key figure in the whole process was the field studies coordinator. From the very beginning, the field studies coordinator was an *academic* appointment. We

defined that role as having academic quality, which meant that it was not simply an administrative role. The reason for that is, the field studies coordinator was essentially the bridge between the faculty and the campus, on the one hand, and the community organization on the other. So the coordinator had to understand what the nature of these organizations were. Over a period of years, we developed a considerable amount of experience, which was embodied in the role of a field studies coordinator.

And then—I'll have to dig out the year, but roughly about ten years in or something like that, Mike Rotkin bid for the job as field studies coordinator, and he was appointed.²¹ He began to routinize—bureaucratize partially, stabilize—the whole process, making a library, essentially, of organizations and types of organizations where students could do field study. The field studies coordinator then would help students find field study placements. Subsequently, as a matter of fact, besides the field studies coordinator working on the field study component, Mike Rotkin became a lecturer, and participated in the actual teaching process. He became a key figure in the whole operation. From the very beginning, the field studies coordinator participated in faculty meetings. I don't know if I mentioned this before, but the faculty and the staff met together in department meetings along with students.

Rabkin: Were these the meetings that were also joined by the student representatives to the board, or department?

²¹ An oral history with Michael Rotkin is forthcoming in 2013—Editor.

Friedland: Right. So that we, in effect, embodied within the department meeting the major components: faculty, staff, and students. We did not have representation from the outside. But we did have, in the early years of the senior thesis completion, an oral examination on the thesis. In the beginning, we insisted on, if at all possible, to have somebody from the organization sitting in on the senior thesis oral exam.

Rabkin: Would this be equivalent to the doctoral dissertation defense?

Friedland: That was the model. But it wasn't so much the defense that takes place in some universities, where you really have to *defend* the thesis. It was essentially a discussion over the senior thesis. We wanted to informalize it. After all, these were undergraduates. (laughs) We did not expect students, for example, to embody a whole relevant literature, which we would at the doctoral level. We encouraged students to draw upon the literature, but we did not expect that draw to be comprehensive, but much more specifically related to the student's field study activity. So that was a key process, too.

Other Research Interests: Agricultural Mechanization of Iceberg Lettuce

To get back to my personal research: I began with my interests in agricultural labor, but quickly discovered that I had to understand more about agricultural processes. And doing research on agricultural mechanization, I took the case study of processing tomatoes, which took place in California beginning in 1964, and which was completed by 1970. It was one of the fastest transitions to mechanization in the literature on agricultural mechanization. So that research I

did immediately following the “Social Sleepwalkers” monograph, and working with a graduate student, Amy Barton, we completed that study. I then projected doing a study of agricultural mechanization of iceberg lettuce. In that case, iceberg lettuce, the research of how you could harvest iceberg lettuce by a machine was well underway in the University of California, on the one hand, and at the U.S. Department of Agriculture field station in Salinas, on the other.

So there were two kinds of systems, each of them having certain similarities. One had an X-ray to determine the density of the head of lettuce, and the other used, I believe, gamma ray. Each had various ways of clasping the lettuce, and uplifting it, and putting it on some kind of conveyer.

The mechanization research was well under way, and what I suggested and got some funding for was a research project to examine: what would the conditions be under which this mechanization transition would take place, and what would the social consequences be if that mechanization took place? The intention was to set something up that would bolster the argument that the University of California should have a PEMU—predictive and evaluative unit—that could facilitate the process of ag scientists doing research projecting what the social consequences of their research might be, with a small faculty that would work with ag scientists interested in having somebody work with them to help them think through what the social consequences could be. And then, to set up a process by which, after the research was done, to go back after some period of time and evaluate what the social consequences actually were, and how the actual predictive process could be improved.

That was the idea of “Social Sleepwalkers,” and that was one of the remedies that was suggested to the judge in the suit on agriculture mechanization. In any case, all this became moot when the appeal that the university made was successful, and the suit was rejected.

In developing my research with two particular commodities: processing tomatoes and iceberg lettuce, what was becoming increasingly clear to me is that if you look at California agriculture, what you have, on the whole, is a series of discrete production systems of specific agricultural commodities. So this began, then, to crystallize as “commodity systems analysis.” I broached this at the 1981 annual meeting of the American Sociological Association and that then led to a publication²², which was followed several years later by a second publication.²³ This then began to influence rural sociologists, and some others, in doing research on commodity *systems*.

Rural Sociology

Now, there’d been a sizeable amount of research on what could be called commodity issues, like the first agricultural mechanization research that I did. In effect, what you do is you take a commodity, and you look at one particular issue with respect to that commodity. But you don’t take, so to speak, the totality of the commodity, from “field to fork,” in effect. Slowly but surely, this became

²² William H. Friedland, “Commodity Systems Analysis: An Approach to the Sociology of Agriculture,” in Harry K. Schwarzweller (ed.) *Research in Rural Sociology and Development*, Volume I (1984) Greenwich, Connecticut.

²³ William F. Friedland, “Reprise on Commodity Systems Methodology,” *International Journal of Sociology of Agriculture and Food*. Vol. 9. (2001) No. 1: 82-103.

acknowledged as a legitimate academic orientation. There were several different approaches: there are commodity chains; there was the French approach, “*filières*”—networks—in other words, a network analysis. And there were a variety of different kinds of conferences that were held about this. I, for example, organized a conference in 1991 to look at fresh fruits and vegetables. And that drew a very interesting collection of Americans, Brits, Australians, New Zealanders, Mexicans. I think we had a Brazilian or two. That was a truly international conference. We produced a series of working papers, but we did not try to get a book out of that.

I should shift over now and talk about the mini-conference format, which our research committee of the International Sociological Association started putting together. In 1978, after going to a one-day conference in Davis about the situation in American agriculture—because there was yet another economic crisis in agriculture—we began to cluster as an intellectual group. This was followed by an International Sociological Association meeting in which the Americans met with a number of Italians, Canadians, some French researchers interested in agriculture. We began to emerge as a coherent group with a formal entity: the Research Committee on the Sociology of Agriculture—and later, on, “and Food.”

Here I can say a few words about our “invisible college.” This grouping was a very kind of loose grouping that we call—not just in the social sciences, but in the sciences—an invisible college. There’s a book that sets out the analysis of

invisible colleges.²⁴ Invisible colleges are interesting by virtue of the fact that there are many of them; some become formalized, like our International Sociological Association Research Committee 40. Many of them are just informal groupings of people interested in a particular topic. I ran into this as a graduate student at Wayne, with two of my fellow graduate students who were interested in death—

Rabkin: Hmm!

Friedland: —and who fell into a small grouping of other social scientists interested in death, and, as a matter of fact, maintained their contact over years. And then there are a number of other such groupings. Highly specialized groupings. And in one case—what’s the expression? Pardon me while I dig out the expression. [Walks to bookcase, retrieves a document] “Environmental change which produces mutagens.” This began as an informal grouping, people who—mainly scientists, almost entirely scientists—who found disturbing examples of chemicals that produce mutagens that got released in the environment. So: environmental mutagenicity emerged as a field, and ultimately as an academic society, the Environmental Mutagen Society.²⁵

Rabkin: Are you saying that genetic toxicology began as one of these, sort of, invisible colleges?

²⁴ Diana Crane, *Invisible Colleges: Diffusion of Knowledge in Scientific Communities* (Chicago: University of Chicago Press, 1972).

²⁵ Scott Frickel, *Chemical Consequences: Environmental Mutagens, Scientist Activism, and the Rise of Genetic Toxicology*. (New Brunswick: Rutgers University Press, 2004).

Friedland: Oh, yes. It began as an invisible college. And, in this particular case, is now a highly successful professional society.

Working with people like this, you have a kind of a formal membership, but people kind of float in and float out. There's kind of a core of people that stay with the invisible college—our invisible college—over the years, and new people come in, and that core kind of keeps the organization going. Essentially what we created was a series of focused mini-conferences on specific topics—the very first one being a meeting that took place within the International Rural Sociological Association meeting in 1988. That was the foundation meeting, in effect. I organized that one. And the focus was “*Ancora di Agrarifrage*,” which I deliberately mixed up Italian and German: *Ancora*—“again,” with *Die Agrarifrage*: “the agrarian question.” That meeting was very successful. I did not try to organize a book out of it; I made a mistake on that score.

And the pattern then got set, so that it would depend upon an individual to say, “We ought to have a conference on *this*”—“X.” So, for example, one of the more successful ones was one that took place *not* at a meeting of one of the regular societies, but where one of our colleagues got funding—Alessandro Bonanno got funding, and we gathered at the University of Missouri at Columbia to deal with globalization. That introduced the issue of globalization at a very early stage. In effect, we were saying, “Well, we’d better get into globalization, because it’s obvious that it’s happening very rapidly with food.”

Rabkin: What year was this?

Friedland: 1991.

Rabkin: Okay.

Friedland: So we then developed two patterns: a specific conference *outside*, or a specific, focused mini-conference *inside*. The inside could be with the Rural Sociological Society, or with the international bodies. And we produced a number of books. For example, I organized a mini-conference at the Rural Sociological Society in 1988 on the political economy of agriculture and a book came out of that.²⁶ At another conference, we were kibitzing around in the halls, as we very frequently would do, and one of us said, “We ought to have a mini-conference on *agency*. Agency had emerged as a hot topic among sociologists, and so we agreed on the spot: since we deal with agriculture and food, let’s have a mini-conference that deals with agency related to agriculture and food. And out of that came the book *The Fight Over Food*.²⁷

Rabkin: And how do sociologists define “agency,” or think about that idea?

Friedland: Agency is the conscious and deliberate organization of an individual or a group of individuals in how they act in society. In most cases—for example,

²⁶ William F. Friedland, Lawrence Busch, Frederick H. Buttel, and Alan P. Rudy (eds.) *Towards a New Political Economy of Agriculture*. (Boulder, Colorado: Westview Press, 1991).

²⁷ Wynne Wright and Gerad Middendorf (eds.) *The Fight Over Food: Producers, Consumers, and Activists Challenge the Global Food System*, (University Park, Pennsylvania: Pennsylvania State University Press, 2008).

in daily life—most of us do not express agency. Most of us are creatures of habit. We have been socialized. If the traffic light is red, we stop. We don't think about it. But if, for example, it is two o'clock in the morning, and we come to an intersection, and there's not a car in sight, the signal is red— Shall I act, and violate the norms? Can't see a thing in sight—no human beings, no cars. You exercise agency. Some people do not. Some people will stay there and wait for the light to turn green. Some people will express agency and break through the patterns, because they feel that it's appropriate to the time and place. I'm giving you a simple example, but it's the action of an individual or a group of individuals—clusters—in dealing with daily life and the need for change.

Rabkin: What kinds of questions did you ask about agency in relation to food and agriculture?

Friedland: For example, why would people express agency and start buying organic food? In the beginning, you have to express agency. First of all, it's hard to find, initially. Secondly, it's more expensive. Thirdly, it's not in regular supply; you've got a lot of breakdowns in the supply system. So why should some individual express agency; what's going on in that person? It's an expression of agency. There are many others. So our book has a number of papers that deal with the issue of the expression of agency. And we've had, I would say, probably something on the order of almost a dozen mini-conferences over the years.

Anyhow, this was part of the process by which the invisible college maintains intellectual growth. Somebody has an idea; you kind of kibitz it around, in one

way or another, with people, and then what you do is you organize a conference. So we had a conference when the rural sociologists met in Santa Clara, on: Why do we have a variety of different kinds of movements developing around presenting alternatives to the “normal” systems of agriculture and food?

By having mini-conferences, what we did is, we gave a kind of development of continuity. We also, by the way, I should mention, developed our own journal; originally this was a print journal, and now it is an online journal.

Rabkin: What’s it called?

Friedland: The *International Journal of Sociology of Agriculture and Food*. We’re up into, I think, Volume 19. We started off with one issue per year; now it’s three issues a year.

Rabkin: Do you trade off the editorship of that journal?

Friedland: It rotates. If somebody has an idea that they’d like to be an editor, and they can find some support in their university, then generally what happens is, people who were the first editors give way to the second ones. I think we’re on the third or fourth tier of editors. The Antipodeans, from Australia and New Zealand, were carrying the load until recently but it has now passed to some colleague at Cardiff University in Wales.

I've been active in our invisible college almost every year, except for the year in which I had some heart problems and missed a rural sociology meeting. But almost every year I've gone, in one way or another, to these meetings. There are a number of others like me, a core or heart, and around them a grouping, with people coming into the heart and taking over. I went to my last meeting in 2013 at the International Rural Sociological Association in Lisbon, this last summer. And that's because I'm now shifting my interest exclusively to wine.

Rabkin: Before we move on from that: You were presented with an award at one of those meetings, were you not?

Friedland: Yes, I was presented with an award called "Excellence in Research," given to one person in the Rural Sociological Society. I got a second award, this last year, as a Distinguished Rural Sociologist, which is kind of amusing to me, because I've never really thought of myself as a rural sociologist. I joined the Rural Sociology Society because they were the only audience interested in agriculture and food. So I became by (laughs) default a rural sociologist. And the Rural Sociological Society has a group of people— Well, the best way to describe it is that after the first year or two of participation, the Rural Sociological Society had between 25 percent and 35 percent of the papers presented at annual meetings dealing with agriculture and food. So that was a big group. The geographers have a small group within their association. The International Sociological Association also supports us. Rural sociology, essentially, has been the intellectual organizational home for me.

Integrating Academic Work and Activism

Okay. Where shall we go now? Let's talk about my increasing concerns about—what shall I call it—the integration of academic work and activism. The university is an interesting institution. In fact, it does encourage activism in the external world. It does by, for example, having professional schools. So, if you have a business administration school, one of the things you want is to have the students do work related to the business world.

Rabkin: So you're using the word "activism" here, now, to refer generally to innovation, and simply active *participation*, in the world beyond the university. Is that right? Not necessarily in service of social change.

Friedland: That's right. But the university classically, especially the land-grant university, has historically had three major foci of academic activity: research, teaching, and extension. Extension is moving it out, moving the knowledge out.

Rabkin: M-hm. And that part is sometimes referred to as "service."

Friedland: That's right. There are many different kinds of words which are used, but the point is, there are three basic kinds of activities within a land-grant university. What was notable was, historically, how this activity outside got focused around centers of economic power—in the community; in the nation. So agricultural extension, for example, got tied in with increasingly large-scale agricultural producers. There's a long literature on this, which I will not cite, but

it's a long and very substantial literature as to why this process happened, and how it happened.

In the University of California, a book is in process which deals specifically with the discouragement that took place at key moments in the history of the University of California that discouraged the establishment of rural sociology, i.e., the study of what's happening in the rural world itself.²⁸ Agricultural extension became tied to large-scale agribusiness. And when the organization of farm workers became a serious problem, the University's response was to build a small unit concerned with the *management* of agricultural labor, not with the problems of agricultural labor.

One of the fundamental orientations of community studies, and also my personal orientation, was to try to change that process—which has changed, to a certain extent within the university, although the agricultural setup remains fundamentally the same. It's geared at large-scale agribusiness rather than the problems of agricultural workers, or, the issue of consumers, of consumption. I mean, the agricultural setup had an Extension segment which was geared at women, not in agriculture, but women who were *married* to farmers. Just like 4-H was a youth component, and that was geared at developing leadership which would then fit into the established organizational networks of large-scale agriculture.

²⁸ Emmett P. Fiske, *The College and its Constituency: Rural and Community Development at the University of California, 1875-1978*. (Ann Arbor: University Microfilms. Dissertation, University of California, Davis, 1978).

What we were trying to do was to get at *other* communities—not just in agriculture, in community studies. Community studies did have students going out into agriculture in one way or another, but we were concerned with getting them into communities that had been ignored by the University of California.

In roughly about 2000, I began to reflect, in my own experience, on the issue of this balance, or imbalance I should say, of research as against teaching. By the year 2000, roughly, I had already developed a reputation as a scholar, as a researcher. And I had a reputation, because of my attachment to community studies, as being a *teacher* involved in social change processes. But I remained frustrated with my incapacity to get my research focused around activism. How do you play the role of an activist when you're in the university?

This crystallized when I was asked to do a contribution to a book which sought to get, mainly sociologists, to talk about how we were oriented towards this business of activism.²⁹ In writing my paper, my chapter, I was really struck by the fact that I did not see how to utilize the tomato study the way Ralph Abascal did. Abascal was a lawyer, and he came up with a *legal* approach. I certainly didn't see anything wrong with that; I thought that was terrific. But I noted that my incapacity to think, as a sociologist— How do I grab something to *use* that research, rather than being the classical academic activist, namely you research a kind of a problem, and you throw it out into the world, hoping that somebody will pick it up.

²⁹ William F. Friedland, "Searching for Action Research and Teaching," in Barry Glassner and Rosanna Hertz (eds.) *Our Studies, Ourselves* (New York: Oxford University Press, 2003) pp. 59-70.

In the chapter that I wrote for this book, I was quite explicit about this: that as a researcher-activist, I was a failure. I was very happy with the turnout of our students in community studies, many of whom have taken this business of activism seriously and devoted their lives as alumni in working in various communities for social change. But I could not see how *I*—I had not yet figured out how I could do that.

And what happened was that as the alternative agri-food movements—first, organics; fair trade; slow food—as these alternative movements began to proliferate, and as, in effect, we entered a new phase in how people thought about agriculture and food, I usually found myself attending the initial meetings—like in California, the organics meeting; I did some meandering around fair trade, personally; I watched the animal welfare people working in this area. And what I was struck by, in each case, was how isolated each individual movement was.

Rabkin: Hmm. By the way, when you say the organics meeting, are you talking about the initial meetings of what became California Certified Organic Farmers?

Friedland: Exactly.

Rabkin: Okay. So you were interested in the process of building a certification system for organics?

Friedland: No. I was not interested in that. I *watched* that process. And as I watched it, I said to myself, “This is going to be captured by the Big Guys as soon as it proves to be successful. And I did an analysis of how that would take place. That was striking to me, because, you see, it seemed to me that if the alternative movements failed to maintain their *alternativity*—that if they were successful, they would simply be captured by the mainstream economy, that they would adapt to that economy, and become less and less alternative. Just for example, I read an article just recently which talks about “organic junk food,” (laughs) a new development. That is, organics were originally intended to be small; now that organics have been captured by large-scale agribusiness, now we’re beginning to see organic junk food.

So, increasingly, I was concerned about this issue of, how do you maintain your alternativity? Because all of these movements start off as alternative. They don’t like the way the established system works, and they are opposed to it. The same thing is true of fair trade, slow food, and you go down the list of the dozen or so movements that do exist. What is striking in each case is not only that some get captured, but that each one maintains its own individuality. One would think that, somehow or other, if they were to work together, wouldn’t that be interesting? (laughs) That kind of working together has not taken place. One of the mini-conferences that I organized was one on convergence—what are the prospects for convergence? In which, essentially, I invited people to produce papers saying, “No, no, convergence will never take place, because of this reason, that reason, the other reason.” Or one might take a particular movement

and say, "This movement has to maintain its integrity." And what was clear is that, so far, it is clear that there is not going to be convergence.

Rabkin: Convergence of these atomized movements into—

Friedland: —these individual movements that tend to be strictly isolated from the other movements. And this became, increasingly, a problematic for me, until I read a book by three of our British colleagues called *Worlds of Food*.³⁰ What they said, essentially, in this book was, "The alternative movements are *the* social movement of our time."

Rabkin: The alternative food and agriculture movements.

Friedland: Yeah. *The* social movement of our time. And that was— (laughs) That was an amazing experience to me, because I actually went through two intellectual epiphanies, reading that book. The first was, I kept on seeing these movements as individuated. And they *were* individuated. Here were these Brits telling me, this is *the* social movement of our time. So the first light that went on over my head was, "Hmm. You got to think of them as a cluster." But if you think of them as a cluster, that raises the question, will they ever converge? This is one of the reasons I organized the mini-conference on convergence. The Brits essentially are thinking differently about this. They do not see organizational convergence taking place. What they see is dozens and dozens of grassroots—

³⁰ Kevin Morgan, Terry Marsden, and Jonathan Murdoch, *Worlds of Food: Place, Power, and Provenance in the Food Chain* (Oxford: Oxford University Press, 2006).

hundreds of them—grassroots organizations doing this, that, and the other, independently. But that these grassroots organizations and movements are shifting things very significantly.

Rabkin: So, in a sense, they constitute a collective movement. Is that what they're saying?

Friedland: Yes. But the individual movements remain independent. So organizational convergence is not going to take place. But there's going to be more and more emphasis on one aspect or another of agriculture and food. So, take for example: One of our people has been very much caught up with the animal welfare [issue]. And she has become an expert in this particular area. Well, in the European Union, where they are trying to organize sets of standards on how animals will be treated—because there is the demand for that within the European Union—she is now working with dozens if not hundreds of animal scientists, who previously were not really interested in animal welfare; they were interested in getting more and more production of pigs, chickens and so forth, issues like that. And now there's the beginning of a shift, as the result of the pressures of the European Union, where they say, "Well, there are different kinds of consumers of animals." You have "regular consumers"; you have Jews, who want kosher meat; you have Muslims, who want Halal—so they have different rules as to how to handle animals. And the European Union is saying, "Well, we would like to see whether or not we can establish some kind of standards that most people can agree on." So you've got this sense of

alternativity which is developing within the established networks of agricultural animal scientists.

Alternative Agrifood Researchers Without Borders

So this raised the issue for me: What might *I* do to start bringing people together? It took me several years to work this through. I started, essentially, with the model of the Harvard Business School. I don't believe in inventing the wheel all over again. The Harvard Business School is a very well established academic entity, which deals with business, and does it essentially with this process of research and teaching. I don't know that they do very much "extension" as we think of it in a land-grant university, but on research and teaching, they are excellent. But they focus on problems of business. They don't deal with problems of social change.

So how can you encourage the process of social change, I asked myself. And one of the things that I found, in working in our invisible college, is that a majority of the social scientists in our invisible college, in one way or another, felt uncomfortable with the established system of agriculture and food. It wasn't that they had an articulated view, either individually, or certainly not collectively. It's just that people said, "Well, I don't like the fact that the use of chemicals in agriculture continues to grow, despite integrated pest management." Or, "We still have problems with farm workers." And not just in the United States; you've got the problems in the Netherlands, and so forth. So there are a number of people that have what might be called a "liberal-ish" orientation towards their own research. I said to myself, "In our invisible college we have approximately, I

don't know, 100 to 250 people, because people wander in and out. And that represents a resource that we have. How can we bring that together with a focus on alternativity?"

You don't want to try to bring them together to work on one problem in one place, because different people have different interests—especially people who go through the process of doing a doctoral dissertation, getting a faculty appointment. They have a trajectory of research that they don't want to have disturbed. If they're going to be disturbed, they want to do it themselves. How can you, somehow or other, organize the collectivity who really feel uncomfortable with the fact that globalization is producing this continuing process in which the separation of material wealth between the top and the bottom is growing larger and larger all the time? And most of us feel uncomfortable about that. And they don't know what to do about it. What can we do about it?

Well, it seemed to me that we have a resource which is available to us in the form of graduate students. Many graduate students, for example—when organics was just beginning, many graduate students began to study organics began to do research on organics. The dissertations started being produced and the publications started being produced. And that's because graduate students, generally, are at the cusp of what is developing. And most graduate students, particularly in the social sciences, have a general liberal-ish orientation toward the world. We don't insist that they have that kind of orientation; they have it because of their personal interests. So it is their personal interests which are at

work, as well as the personal interests of established faculty, who don't want to be disturbed by starting a new project because they have a trajectory of research they want to continue. For example, I was working on globalization; I wanted to stay with globalization. I wasn't prepared to go out and focus on one of the alternative movements. I wanted to deal with a much broader process called globalization. I was concerned with the issue of income distribution.

Anyhow, thinking about it, and talking to my colleagues, I said, "Let's take the model of the Harvard Business School." I called it the (Un)Harvard (Un)Business School. Because we'll have a different kind of orientation than a business school. And what we'll do is, we can't reproduce the Harvard Business School, which is in a fixed geographical location, in a fixed building or set of buildings on the Cambridge campus of Harvard—

Rabkin: —with a big endowment—

Friedland: With a big endowment, that's right; we're not going to have a big endowment. So we are scattered all over hell and gone; what can we do to bring ourselves together? Well, what we can do is we each, individually, have areas of specialization. So, for example, I specialize in globalization. I specialize, to some degree, in agricultural labor. I have other specializations as well. Each one of us has anywhere from two to five to six or seven or eight specializations. And if a graduate student is interested in doing a dissertation on a particular topic in a particular place, we have, probably, somebody who has experience in a

particular place—like Africa, or a country in Africa—and on a particular topic within Africa.

In other words, on any given campus, the most we will have is two, three, four, or five faculty members interested in the kinds of materials that we in the invisible college are interested in. And, as a matter of fact, once I tested this at a number of conferences and found encouragement, I began to recruit faculty to become part of the faculty of the (Un)Harvard (Un)Business School. Which then got a new name, because some people thought the name was frivolous. So it became the Alternative Agrifood Researchers Without Borders. I wrote an article on this which got published in *Rural Sociology* and which is now circulating around hither and yon.³¹ And some people have found their way to joining the faculty. What the faculty are expected to do is to consider working with one graduate student annually, initially on a single project for a semester or a quarter. If the faculty member and the student agree, that that faculty member will then become a regular member of the student's graduate committee or dissertation committee, bringing that faculty member's expertise to bear on the research of the graduate student.

Rabkin: Even if that faculty member is not affiliated with the particular institution where the student is enrolled.

³¹ William F. Friedland, "New Ways of Working and Organization: Alternative Agrifood Movements and Agrifood Researchers," *Rural Sociology*, 75.4: (2010), 601-627.

Friedland: That's right. And I said to myself, we'll do this on an informal basis to begin with, because if we try to do this on a formal basis, we have to deal with every university that our faculty members are attached to, and every university has its own bureaucracy, and working your way through those bureaucracies is a nightmare. Instead, what we will do is, we will do it informally, and if it works, then we can accumulate data on how it works, and then we can go and ask for formal acknowledgement of this process—which, after all, is somewhat established between universities in any case.

Rabkin: To have extramural advisors on a graduate student's dissertation.

Friedland: Right.

Rabkin: So have you produced a directory of yourselves with your various specialties?

Friedland: Yes, I produced a directory of our faculty. The last time I had it up on the website, there I think were forty-one of us. We have something like 142 areas of specialization, so that we could match almost any student's interests. The requirement is that the faculty member and the student have regular relationships, as if the faculty member was on the student's campus. We have the technology for that through systems such as Skype. So that in the introductory session, a student would say to a faculty member, "I see you're interested in such-and-such. I would like to do a directed study with you on—" —and lay it out the way the student would with a regular faculty member. The faculty

member would consider that. If the faculty member was interested, they'd agree to meet, let's say, once a week, via Skype; they'd agree that there will be regular assignments, in which the student will produce something in writing, which can be shipped by email very easily, and they can serve as the subject of the meetings between the two. And if, at the end of the semester, they both feel happy about it, they can concretize it by having that faculty member being invited by the student, with the student's advisors on the campus agreeing to that process.

Rabkin: Is there any barrier to this in the faculty employment circumstances? That is, that a faculty member at a given institution would be essentially adding a graduate student to their roster without getting any institutional recognition for that addition to their workload. Is that an issue?

Friedland: That's an issue, sure. But you work it through with the faculty member in the institution of the student. The faculty member has to agree, and the faculty member has to be prepared, if necessary, to deal with the issue of, how do you get some kind of credit, and acknowledgement of the external faculty member? And eventually, you try to routinize this and formalize it, by saying— You go to the Graduate Council at UCSC, and we say, "We now have experience doing this with x students. Here is the outcome; here are the dissertations, here's the whole process"—because we've maintained copies of the student papers and the assignments—"We want the faculty members on our campus to get credit here for having an extra graduate student." Yes. I'm on this campus; I know this campus; I know the Graduate Council to some degree. I would become the person who would work it through on my campus.

Rabkin: How has this development affected your sense of the possibility of effecting social change as an academic?

Friedland: Well, aside from the fact that the people who have joined the faculty have agreed that they are unhappy with what is happening with income distribution and power distribution, what we would expect a graduate student to do, if this graduate student is participating in this process, is to do a dissertation, but in the process of getting the data for a dissertation, you have to define research problems. Well, the research problems stem from the literature and the faculty that you deal with. But if you're working with a group of people that's, say, in an organics group, or a fair trade group, or an animal welfare group, those people who are the activists will very frequently say—or maybe not so frequently, but will very frequently think—“Why aren't you doing some research that *we* could use?” So *they*, in effect, can become research-sensitive.

So one of the things that we would want the graduate student to do is to encourage that process, and to formalize it as a research problem, to be part of a dissertation.

Rabkin: So that the needs of communities and organizations and entities on the ground, outside the university, begin to influence the direction of research being done at the university?

Friedland: I'll phrase it slightly differently.

Rabkin: Please.

Friedland: To do research *in addition to* what the academy demands. Look, before a student goes out and does field study, the student has to have some idea of what the field study ought to be—what the intellectual problem is. And that flows from the *academic* demands—the state of the literature, in particular: What are the intellectual problems that can be examined in this particular location? But what we want is for the student to be sensitive, while in the field, to what might be a researchable problem that the *community* can utilize. And what that means is, the dissertation might contain some of that—but that at the end of the field study period, or at the end of the dissertation, the graduate student can go to the group that the student has worked with and say, “You gave me a research problem. Here it is. I worked it out with you; we talked it through among ourselves. I’ve now included that in a dissertation, or I’m giving it to you separately, and this is something that you can now utilize.” So that there’s been a tangible payback.

Rabkin: Yes.

Friedland: “I’ve drawn upon your experiences to benefit me.” Classically, when we do field study, there is no payback. The payback is to the world. That is to say, we throw the dissertation out, and we throw the book out, and say, “Here, world, do with this what you will.” And this is the literature in the field. So

instead what we want to do is we want a chunk of that research to become useful as payback.

Now, it does another thing as well, and that is, if the graduate student realizes that there is a kind of a payback as a norm of doing field research, the graduate student will become increasingly sensitive to the problems of the group with which he or she has worked. So in taking the payback, you begin to densen the relationship between the graduate student—now with a Ph.D., looking for a job, etcetera—and the community group. So you can possibly get a more coordinated experience between the two, in which the graduate student is invited to come do research; the graduate student is invited to the organization to provide reports, etcetera. To show what research can do. Which also, then, makes the entry of a graduate student more palatable to the group. Because most groups know— In the 70s and 80s, there was a real reaction in many activist groups against having students come in and work.

And in a sense, you see, this takes the whole approach of the community studies department. You put a student out; the student is expected to work inside an organization and do something *useful* for that organization—not just to pull out material which can be used to generate a senior thesis at Santa Cruz. There should be a payback. There have been a lot of complaints about the exploitation of groups by students, and therefore, a reaction saying, “No, we don’t want to have students.”

Rabkin: In my department, environmental studies, I've heard a little bit about graduate students engaged in something they're calling participatory action research.

Friedland: Mm-hmm.

Rabkin: Are you familiar with that model, and is that similar to what you're talking about?

Friedland: Yes. Except this is, so to speak, a normative requirement of the approach. It says to the student, "You should do payback." Now, how does this participatory action research work in environmental studies?

Rabkin: I don't know enough about it to say.

Friedland: Okay. See, this is another problem: there hasn't been much evaluation of this kind of activity. There is this, in contrast to the origins of field study at UCSC, and other places as well: The original approach of the faculty was: "Oh, you want to go off campus to study *x*? Sure! Five units of credit. I'll sign." No requirement to bring anything back; no requirement to assess that work. No requirement to confront it as an intellectual problem. Just satisfying the student's curiosity. In community studies, we said, "There's got to be a quid-pro-quo with the community." This approach is now built into the Alternative Agrifood Researchers Without Borders, the AARWB.

Rabkin: At the graduate level.

Friedland: At the graduate level. Yes.

Rabkin: So do you see this having wings? Do you envision it growing and taking off?

Research on the California Wine Industry

Friedland: Well, we've got a problem. (laughs) We have a problem—namely, I'm burnt out on this. This has now occupied me for the better part of six or seven years. I'm burnt out on this, but the burnout is even more profound. All that research that I've done on wine, and the grapevine and its products is sitting up there on the wall and in these filing cabinets.

Rabkin: We're looking at an entire wall of your office covered with bookcases housing, entirely—

Friedland: Wine.

Rabkin: —books and papers about, related to, wine and the grape industry.

Friedland: I have in draft parts of a manuscript that deals with raisins and fresh table grapes. Wine has not been done. And as you know, one doesn't get any younger. (laughs) And if I don't complete this book, this will all be lost. So, what I did is, I said my farewells to my colleagues, and I said, "I am not going to do

anything except get this wine section done, because until I can get it done, I can't put the whole thing together." So I am at that stage where this *has* to be done, and I'm putting aside all other issues except doing a short-term project with you, of an oral history.

Rabkin: For which I'm grateful. And the readers of the oral history will be grateful, too.

Friedland (laughs) Okay. So actually, what I'm trying to do is pass the responsibility to the AARWB faculty, and it's a slow process. I'm still working on it, so I'm not completely free of the (Un)Harvard (Un)Business School.

Rabkin: So you're in the process of making sure that it gets picked up by the other faculty in the program.

Friedland: Well, I hope so, because if it doesn't, then it will essentially disappear. That fits in with my approach to the process of social change: you find yourself in a situation in your life. I found myself in a situation in which I wanted to find a place in which I could survive in American society, using my experiences and my capabilities. That was in the academy. And initially, when I came into the academy, I was burnt out completely, and I had projected a non-political orientation. I was going to stay out of politics completely. And in effect, if the university was just going to support agribusiness—that's life. (laughs)

In 1964, I began to make a transition. I came back to the notion of some kind of political activism *within the established institutions*. My institution is the academy. What can you do within the academy? At Cornell, I created the Cornell Migrant Labor Program. That then grew into community studies at Santa Cruz, and into my research project, where the concrete applications came via California Rural Legal Assistance and the suit against the university for the agricultural mechanization research. I had to find some new way of acting beyond that, on my own. That took a long time for me. And—I mentioned the two epiphanies that I went through reading *Worlds of Food*. It took about a two-year period to think through what became the (Un)Harvard (Un)Business School, and then a couple more years of exploring it, working out the little difficulties that people raised, and then getting it started. I had the help of the sustainability folks on this campus—

Rabkin: At the Center for Agroecology and Sustainable Food Systems?

Friedland: —and Sustainable Food Systems, yup. Which provided a little financial help, and permitted me to get an undergraduate research assistant, who helped get a website started. The website is moribund at the moment because we haven't paid rent (laughs) to the people that run the computers. I have somebody that can provide the funding. But it's now hinging on getting the faculty to have a coherent orientation to taking over the Alternative Agrifood Researchers Without Borders. At that stage, we can revive the website; we can bring in at least two, three, four or five new faculty that have strayed in (laughs) or have been talked into by various people, myself included, to join. And then we have to

confront the issue of: why have Ph.D. students failed to utilize this opportunity? Which is an organizational problem. You start a new organization; you have organizational problems. The faculty itself is in place; the ideas are in place; now we have got to get graduate students to take advantage of the opportunity of working with faculty at a distance.

Rabkin: Is it partly a problem of communication or publicity—that graduate students who might be interested don't necessarily know about this opportunity?

Friedland: I am convinced that enough of my colleagues have advertised the opportunity. And when I ask the question, "Why aren't they taking advantage?" the main response is, "Graduate students have enough problems getting their graduate committees together on *one* campus, let alone somebody out there." So, that problem has to be overcome. I don't see that as an insuperable problem, but I am now at that stage where I'm not going to lead the effort to break through on that particular issue. I think my colleagues are perfectly capable. Most of them are seasoned academicians. We have a number of younger people who have also joined, and I think they are well experienced with organizational life, so that they can contribute to this issue. I think that the faculty can deal with this issue. But it's a matter of getting a group of people to agree on a division of labor so that they know what their responsibilities are.

I think the process of social change, when you create something new, is one in which you don't know whether or not it's going to succeed. What that means is that if you're going to be an agent of social change, you have to be well versed in

organizational change, well versed in social change. You have to recognize the difficulties that there are in creating new orientations. You have to be prepared to see your experiment not working. And you should not walk away from that depressed. Social change is a process in which many people participate. And when they start something new, the overwhelming majority of these experiments are going to fail—if not immediately, like community studies forty years down the pike. You can become embittered by that—but what I say is, if you're going to be an academic activist, you have to be prepared for failure, and not let it get you down. The question should be, "Why didn't it work?"

Rabkin: And that becomes the basis for your next move—thinking about that question.

Friedland: Well, (laughs) at my age—

Rabkin: Or, *one's* next move—

Friedland: At my age, I don't think I want to start a new project.

Rabkin: Right. I was just thinking in general: In response to that assessment, what encouragement should one take who's interested in social change? And it seems to me that you're suggesting that the question itself is the useful next step: asking, "*Why* did we not succeed that last time around?"

Friedland: Yeah. And where is it that you, taking your particular position (in the academy, in our situation)—where is it that you can do something that can produce a contribution to the *diminution* of the spread between the top and the bottom, economically, or in terms of power? Don't expect to make a revolution. I now am convinced there is not going to be a revolution. But the process of social change is a long-term process. We have the experience of the development of capitalism. Capitalism developed out of feudalism. Well, it took approximately 400 years for that process to become "successful," and for capitalism to become *the* economic system in which we operate. If you're going to produce social change, it will be relatively minuscule; it may not work. But if you want to do that, well, you should consider that a lifetime activity.

Center for Agroecology and Sustainable Food Systems

Rabkin: I wanted to go back to something you made reference to a little while ago. You mentioned CASFS, the Center for Agroecology and Sustainable Food Systems, and I was wondering whether there were any other collegial relations you had with people in that organization that might be worth talking about.

Friedland: Well, I have always had a long-term connection to the UCSC Farm, and CASFS. That connection essentially was 90 percent informal. I used to walk up to the university, almost every day, and very frequently I would walk through the Farm. I had my suspicions about the Farm. I have my suspicions about most attempts at change. I did not hear too much of a coherent intellectual orientation to the Farm. I felt that people that are involved in the process were good people; they were trying to make change, but they were essentially semi-

coherent in the way they thought about what they were doing. I therefore walked through and, in effect, watched and studied—not in a formal research sense, but studied, got to know the personnel who were there, saw the way the continuity existed, the crises that they went through. So that I became a kind of an expert around the nature of the Farm.

Chancellor [Robert] Sinsheimer asked Ken Norris and me to look at the Farm and Garden after the Alan Chadwick departure. Alan Chadwick was a brilliant person, but a bit of a lunatic. Alan Chadwick was vigorously anti-science. (laughs) And in a university, that doesn't make sense. He could draw to him lots of young people with enthusiasm. He deserves a lot of credit for what he did on the Merrill hillside. But in the long term, he had no future in the University of California, where he was actively and aggressively anti-science. It didn't make sense. Chancellor Sinsheimer said to the two of us, "We've got the Farm and Garden. The Farm has just been established; God knows what will happen with it. But the Garden on the hillside—that is a successful operation at UCSC. What should I do with this?"

Ken Norris and I went away and deliberated, and we said, "This is a worthwhile endeavor in which Santa Cruz should be engaged. It is alternative; it's trying something new; it's not attached to the standard ag-science network of the University of California. It's trying to produce a different kind of orientation. But we've got to recruit a person who believes in that kind of change, on the one

hand, but who has a scientific background and will not be anti-science. And the chancellor agreed with that, and that was when we recruited Steve Gliessman.³²

I still refused to, so to speak, become more formally affiliated, because for one thing, they had, at various times, Friends of the Farm & Garden, and I didn't want to be a Friend, because I see all this as an amorphous role. I was developing my own approach in mainstream agriculture, doing research in mainstream agriculture. I was suspicious that this thing might be here today and gone tomorrow. I thought the apprenticeship program was terrific; I thought it should be continued. I didn't spend a lot of time with the apprenticeship program. But if the people that were running the apprenticeship program said to me, "Come and do a lecture on large-scale California agriculture," which is my area of specialization, that was an easy task for me, and I always did it. So I had that kind of a connection—a substantive intellectual connection. I followed what was going on on a continuing basis through the various crises. And at one stage when there was a crisis of leadership, the then-acting dean asked me to become the director of the Farm and Garden. I said no. (laughs) I was already retired. I was determined to not get involved with administration, and I knew how the Farm and Garden had related to the rest of the campus, and I did not really approve of the fact that this was just kind of a bump on a log.

Rabkin: Not integrated into the life of the campus?

³² See Irene Reti, Interviewer and Editor, *Stephen R. Gliessman: Alfred E. Heller Professor of Agroecology, UC Santa Cruz*, part of the Regional History Project's *Cultivating a Movement: An Oral History Series on Organic Farming and Sustainable Agriculture on California's Central Coast* (UCSC Library, 2010). The Gliessman oral history is available in full text at <http://library.ucsc.edu/reg-hist/cultiv/gliessman>

Friedland: That's right. I had seen what had happened to the peregrine falcon operation, which had been tossed off campus after doing successful work—but not integrated into the work of the campus.³³ And I said to myself, I don't know whether or not this will be here another year. Years passed. I still was suspicious, but when the dean asked me to take it over, I came back with a counter-proposal which said, "If you want, Ken Norris, and Jim Pepper and I will form a steering committee for CASFS, with the intention of trying to think through a program for the future." And the dean said, "Good," and the three of us met, and we came up with the notion that there had to be a better integration between the apprenticeship program, on the one hand, and the campus's main work. So we said the social science dean should allocate a new FTE, which would be half the directorship of CASFS and half substantively with a department; that this FTE will be used for future directors. In other words, we will recruit a new person for this role, and then when that person wants to give up the directorship, we have to recruit a faculty member on campus to take it over. Because that's one way to integrate the process of CASFS with the regular campus. But we also said, "You've got to get the apprenticeship program integrated into the work of the campus." And that failed.

Rabkin: That part failed?

³³ The Santa Cruz Predatory Bird Research Group was founded at UC Santa Cruz in the early 1970s and was led for many years by Brian Walton. For a detailed history see <http://www2.ucsc.edu/scpbrg/history.htm>

Friedland: That part failed. But we did recruit Carol Shennan, as a result, and that gave us a ten-year leadership process. And then there's a long history with the present dean not appointing a faculty member to the directorship—which finally took place in this last year.³⁴ But there was this interim period in which that agreement was violated.

So I've had a long-term relationship with CASFS. And I did not make big financial demands upon CASFS. I needed somebody to help me with the computerization, development of the website, and similar tasks, in getting the Alternative Agrifood Researchers Without Borders off the ground. I got that support from them. They paid the rent for the website—a couple of hundred bucks a year.

Rabkin: CASFS did?

Friedland: Yeah. For a couple of years, they paid the rent. And then in the last year, they were so broke that that was impossible, and eventually what happened is, we didn't pay the rent, and they took us off the web. Now we've got the possibility because one of our faculty has—it's really small potatoes; it's 245 bucks, something like that. So we can get back on the web.

I think that whoever takes over the leadership of the whole operation— I think there should be an executive committee, and there should be a chair of the executive committee, and that person should become the driving force. Most of

³⁴ Environmental studies professor Daniel Press became executive director of CASFS in 2012.

the other tasks can be handled by members of the faculty without too much difficulty. We need a membership committee for new members, and we need a library committee to build the library. And neither of those committees take up a lot of time. The executive committee will take up more time, and in particular the chair of the executive committee. If we can get that executive committee— Right now we have—what is it, I think it's about five of our faculty members that say they are willing to take roles, and we need about three or four more. And as long as we can get an executive committee (laughs) —which I haven't introduced yet—we can put the whole thing together. It may take a year to get the whole package together. So that's left hanging, with me.

Rabkin: It sounds like you're very patiently shepherding this thing to the point where it can survive without your leadership.

Friedland: Exactly. I mean, I'm willing to be a member. I'm willing to be on one of the minor committees. But I don't want to be in any of the leadership of the whole thing. That takes up too much time for me, and my pace of work has slowed significantly, so I know I can't do the wine job with continuing with the (Un)Harvard (Un)Business School. (laughs)

Rabkin: Bill, is there anything we haven't covered that you'd like to address before we wrap the interview up?

Friedland: Let me take a look at my notes.

More on Academic Activism

Well, let me go back to the period of my burnout, and my re-integration (laughs) into some forms of academic activism. There are many different forms of academic activism. And there are many people that attempt to find some form of activism that fits their personalities and fits their concrete situation in the university in which they are enmeshed. I came back to activism by virtue of 1964, when the Free Speech Movement erupted at Berkeley, and which then became one of the major social movements of the next—'64 to '72, that's eight years. In that period, the student movement became a powerful movement, and I began to see possibilities when I was pressed by students at Cornell, who said to me, "You're teaching about African trade unions, but that's not particularly relevant to our situation; there's nothing we can do about it. What can you do in teaching that will be relevant?"

It took a while, but I saw the possibilities, because there were migrant agricultural workers in the Cornell area. I saw the possibility of building that into my teaching process. And the School of Industrial and Labor Relations at Cornell did not object to that, although they refused to give academic credit for students doing field study. We did the project without that academic credit. It was a successful project. It was successful at Cornell. The School of Industrial Labor Relations was happy with it. Cornell was happy with it, because when an organizer of the United Farm Workers union turned up and went to the personnel director of the university and said, "You have a Cohn Farm up here"—it was some, I don't know, thirty miles away or something like that. "We want

those workers organized into our union.” The first person that got called in to advise the personnel director was me. And he said, “What should I do?”

And I sat down with Dorothy Nelkin, my research associate. I said, “Let us take the Cohn Farm,” it was called—the Cohn Farm was something that the Cohn family had given to Cornell University as a piece of property, and it was a working farm (I don’t remember what crops they raised), and the dean of the College of Agriculture was the person administratively responsible. I said, “Let’s propose that the Cohn Farm become an experimental unit, like all the other experimental units that the agriculture school has, and that are perfectly acceptable.” So we sat down and we wrote a full proposal of converting the Cohn Farm to an experimental unit. I was in the process of being recruited to UCSC, but what I was hoping was that Dorothy Nelkin could take it over, because she by this time had considerable experience. We proposed it. We gave it to the personnel director, and he gave it to the dean of the College of Agriculture. And the dean sent in a bulldozer and *bulldozed all of the housing* for these migrant agricultural workers.

Rabkin: *What?*

Friedland: You heard me. (laughs) It happened after I left, actually. But the dean was not prepared to have this as an experimental unit. And, given the nature of deans of colleges of agriculture, that was not a surprising move. I mean, it was disappointing, you understand, because we couldn’t get the dean to see the value of having a unit which would explore different ways of handling migrant labor.

Rabkin: This was to be an experimental unit in terms of *social* experiment—is that right?

Friedland: Yeah. Yeah. What we did is, we laid out some of the kinds of issues that could be researched, and that would have been a beginning. We could have recruited faculty to become part of a faculty group—because there were faculty interested in this issue, not a lot, but there were three or four or five faculty that could have been drawn into it. So we could have started doing research in this whole issue. But he simply obliterated the housing.

Anyhow, why did I get off on this tangent?

Rabkin: You took us back to your burnout—

Friedland: Oh, yeah. Yeah. So I got kind of inspired by what was possible at Cornell, and then was even more possible at Santa Cruz, see, where, in effect, the issue of getting students field study credit was *not* an issue.

Rabkin: In fact, people at that point were getting credit for things you might not have wanted to give them academic credit for.

Friedland: No, that's right. If somebody went out and hugged a tree for a quarter (laughs) and got academic credit for it, and never did anything with it, never wrote anything with it, you know, what are you giving academic— Academic

credit is the particular coin of this institution with undergraduates and graduate students. You don't just let them do anything they want. There has to be some kind of intellectual process, (laughs) you know, which is related to other intellectual processes.

Rabkin: So you were re-energized by the possibilities inherent in that experience at Cornell, despite the ultimate outcome of that particular experiment.

Friedland: That's right. And I know that the students that went through the Cornell Migrant Labor Project learned a *lot*. That was a very, very different kind of academic experience for them. I know a number of them that went on to Ph.D.s. I wasn't particularly interested in producing Ph.D. candidates, you understand, but a number of them got so much involved in it that they saw possibilities within the academy. Some others became experimental with alternative developments within agriculture. So I know that they enjoyed their experience.

We had one student that essentially failed the test. He just couldn't do field study. We got him through the process, but that was not the intention of the process. We had to hold hands with the student to get him through the process. And that served us as, also, an issue that we could learn from as we started community studies. We quickly learned that if you have to hold a student's hand through a senior thesis, it simply takes up too much faculty time. So, for example, one of the things we did fairly early on was to say to students, "You want to come into this major, you have to sit down and write a proposal." And

what that did is it got people who hadn't thought through what they wanted to do at UCSC, let alone what they wanted to do when they grew up— so that that became a way of discouraging students who just seemed to think that community studies was interesting. They had to get their ideas together.

And that was an energizing experience. Community studies at Santa Cruz proved to be my intellectual survival mechanism. Because, among other things, at Cornell, in the last academic year, there was a terrific uproar when black students seized the student union and made a set of demands, and said, "We're not getting out of here until you agree to these demands." That was so shocking to Cornell that some of the fraternities started talking about getting guns and "cleaning them out." Yes. And the black students proceeded to get some guns to make sure this would not take place. So that in my final days at Cornell, the university shut down. Literally shut down, while the faculty and the students went through their various businesses—faculty saying, "You've got to make compromises"; some faculty saying, "This isn't tolerable; this violates our academic freedom"—

Rabkin: "This" being—?

Friedland: Having students armed on campus.

Rabkin: I see.

Friedland: It was a shocking period. And it was a period in which the acting dean of my school was called in, with all the other deans, by the president of the university to explain what his program was to resolve the problem. And the dean came back to our school, convened the faculty—and this was Industrial and Labor Relations faculty, which included a number of former trade unionists—and said that he had made an intervention at the president’s meeting, and he had said that, “We in the labor school”—as it was usually referred to—“have had experience with this in labor and management relations, and we have learned that if management shows a firm hand”—etcetera. And I was in my waning days. I attended that meeting, and I heard this. And not a single one of the people with trade union backgrounds said a word. And the acting dean *was a labor historian*.

Well, if I had not had the escape hatch (laughs) already in place, I would have been well on my way to being an alcoholic. The ILR faculty turned out to be the second most conservative faculty of all the faculties at Cornell. Only the veterinarian school faculty turned out to be more conservative. Even the law school faculty was more open-minded in dealing with this particular question. I’m not saying that this was an easy question to deal with; it was not. And at that time, when the black students came out of the student union, they were wearing bandoliers. (laughs)

Rabkin: What year was this?

Friedland: This was 1969.

Rabkin: So the Black Panthers were very much on people's radar.

Friedland: Yeah. Oh, boy, (laughs) did that get people's attention. I wrote a paper on it with my colleague Irving Louis Horowitz. It became a chapter of a book called— [goes to bookshelf] There it is: *The Knowledge Factory*. (laughs)

All right—how's that?

Rabkin: Is that a good place to end?

Friedland: I think so.

Rabkin: Well, thank you very much, Bill.

Friedland: Phew!

Appendix I: A Historical Note on the Evolution and

Demise of Community Studies

—William Friedland provided the following historical note during the editing of this oral history

During the decade of the 1970s, once the community studies curriculum settled down, the community studies faculty developed three organizational initiatives and I also began developing my own research program on California agriculture. The three initiatives were the extension of the campus program to workers in community action programs in San Jose and Fresno, the creation of a community studies Second Curriculum, and under administration pressure to create graduate programs, the establishment of a Master's degree on Social Documentation.

The Extended University program was encouraged by state funding to initiate new programs around the state essentially away from university campuses. We in community studies saw an opportunity to extend our undergraduate program to community workers around the state that had grown during the 1970s. Many workers in these programs did not have Bachelor's degrees. What our Extended University program proposed to do was to get community workers to treat their employment as field study and to look at their work programs with a critical eye. We picked San Jose and Fresno as the locations for this program.

The funding for our Extended University permitted us to recruit one FTE faculty member and a lecturer. The recruitment effort produced Nancy Stoller for the regular faculty appointment and Michael Rotkin as the lecturer. Both had extensive community organizing experience and, as well, extensive academic experience. We hoped to have them “trade” some courses with on-campus faculty who would reciprocate by providing instruction to the Extended University students. We also hoped to bring the off-campus students for occasional experiences for short-term contact with the on-campus faculty.

Planning and organization of the programs in the two locations took time but eventually we found that off-campus workers were interested in the opportunity to complete bachelor degrees and we soon had thriving programs in the two locations. On-campus faculty provided occasional input to the Extended University students but this mostly took place on an ad hoc basis rather than in systematic exchanges between the on- and off-campus programs.

The original plan, should the two locations prove successful was to expand the program to other city centers after the two programs had settled into a routine. This became impossible when then-Governor Jerry Brown, expressed his discontent with most of the Extended University programs of the other campuses. Except for our program and a similar one at UC San Diego, most did not show any exciting extensions of the University so Governor Brown cut off the special funding. It took the following three years to close our program since we did not want to leave our students with incomplete degrees.

The Community Studies Second Curriculum essentially sought to re-create the original format that had existed at Cornell: faculty members could seek to involve students in their own research programs with the understanding that

they would present their research in its full complexity so that students would learn “by doing” how research projects were planned and implemented. An assumption underlying the Second Curriculum was that a sufficient number of students would participate so that, in its entirety, this would permit the faculty member to earn the equivalent of teaching a course.

Second Curriculum projects were undertaken by several faculty members. I did at least three: two on my then-current agricultural research and one on organizing. The Second Curriculum proved to be too expensive since the numbers of students signing up did not equal the number of students taught in a regular course and the program was dropped after several years.

The third initiative was the creation of a Master’s program in what became known as Social Documentation. The community studies faculty had innumerable discussions beginning in the 1970s about beginning a graduate program. This proved to generate considerable tension since the faculty was divided between those that felt the community studies curriculum — preparation for field study, field study, and intellectual consolidation of the field study experience — worked very well and that there was considerable danger in initiating a graduate program which would inevitably undermine the undergraduate program.

For years, I had favored planning a Ph.D. program for two reasons. First, I believed that there were considerable numbers of individuals who had moved successfully into various forms of organizing during the 1960s and early 1970s and that many of them would be wearing out and getting tired after ten years and could benefit from the equivalent of a sabbatical in which they could approach their own experiences and that of others. I also believed that such a

program could begin a process of intellectually consolidating a new body of knowledge about organizing and that such a program would fit very well with the undergraduate program. I was also convinced that, given the history of the UC with respect to graduate programs, any department without a graduate program, and in particular a Ph.D. program, would inevitably be organizationally treated as inconsequential and would suffer from a failure to win adequate instructional resources, i.e., full-time equivalents (FTEs). Ultimately, the faculty agreed to undertake the development of a Master's program to satisfy the pressures for new graduate programs at UCSC.

Once agreement had been reached, the question became: what kind of Master's program would fit with the undergraduate program that it would not begin undermining it. Thus began a full academic year of exploration which ultimately ended in the idea of Social Documentation. Initially we thought of it as emphasizing the production of social documentaries but that it would also include the production of other forms of social documents. I was particularly interested in including the production of such "documents" as museum presentations, as well as other forms of social documents.

Getting approval for the creation of a "SocDoc" (as it quickly became known) Master's program within the University of California is no trivial matter. It took two full years of writing, editing, and rewriting proposals and pushing them through the various bureaucratic layers within UCSC, to be followed by layers of the UC bureaucracy, and finally layers of the state administration. We finally were successful only to be notified by the campus administration that there were no material resources to support our master's program. The reaction in the community studies faculty was understandable: after responding to UC and

UCSC pressures to create a graduate program and piloting it through three layers of bureaucracy to be told that there were no resources was profoundly demoralizing. It left a residue of suspicion about the seriousness of UCSC, UC, and the state. The faculty returned to operating the undergraduate program but significant damage had been done.

A decade or so later, a new social sciences dean proposed resuscitating the original SocDoc plan and asked the department to go through the rigmarole again. Again, the faculty split with the original opposition reminding us of how the department had been treated and again, a dean expressed his conviction that the original plan had fitted the undergraduate program nicely and offers were made for the provision of resources.

With the experiences of the past, the department agreed to resuscitate the SocDoc proposal and, with the support of the new dean, the process was gotten underway again. This time successfully. The department then began recruitment of a new kind of faculty and found a rich source of documentary makers clamoring for regular faculty appointments. The first recruitment was successful and the recruitment of the first cohort of Master's candidates gotten underway.

By the time of the third faculty recruitment for SocDoc, a development had unfolded that the undergraduate faculty had worried about from the very beginning: UCSC had established an active program in filmmaking within the arts division. Concern had been expressed about a SocDoc faculty wanting to associate themselves with the arts faculty in filmmaking. The department faculty had finally convinced themselves that such a development would be unlikely because the arts filmmaking faculty were primarily concerned with producing

films that would be esthetically pleasing and, therefore, of interest to a different audience than the one we hoped to reach through social document production.

It turned out that this assessment was incorrect and our SocDoc faculty took advantage of a deepening fiscal crisis and yet another “new” dean of social sciences to permit the SocDoc faculty to join the arts faculty and the dean to permit the undergraduate FTE faculty to decline to the point that he proposed the closure of the undergraduate program and the demise of the community studies department.

By the 2012-13 academic year, the community studies faculty had been reduced to a single FTE professor and a single senior lecturer, the department was terminated, and Oakes College agreed to take what was left of the program and to continue it in reduced circumstances for a limited number of undergraduates. The disappointments over the first SocDoc fiasco were again reinforced with considerable bitterness.

Thus, after some 40+ years of successfully operating a program that generated thousands of alumni who built socially active field study and a high degree of academic exposure that had been acknowledged as turning out undergraduates producing academic work at the Master’s level, the academic legitimacy of departmental organization was wiped out. The dean was reported as saying that a university could not be accepted without a physics or sociology department, but it could be accepted without a community studies department.

Appendix II: An Introduction to William Friedland

by Professor Michael Cowan

On March 4, 2013, Bill Friedland delivered a lecture sponsored by the UCSC Emeriti Association "Trampling Out Advantage: the Political Economy of California Wine and Grapes." Professor Michael Cowan introduced Friedland and kindly provided a transcript of his remarks for this oral history—Editor.

Good evening. My name is Michael Cowan. As the current president of this campus's Emeriti Association, on behalf of our members, and of the chancellor's office, I want to welcome you to this evening's lecture, which will conclude with a reception in the lobby to which you are all warmly invited.

For the past ten years, our association, with the generous co-sponsorship of the chancellor, has periodically presented public lectures by emeriti on topics of broad interest. As these lectures clearly demonstrate, formal retirement does not mean settling into a gentle routine of late breakfasts, leisurely lunches, and afternoon naps. Some emeriti have returned to the classroom on recall to meet critical curricular needs. Many have engaged in creative service to a variety of community organizations in and beyond the Santa Cruz area. And most have continued to actively pursue productive research agendas. Our speaker this evening, Bill Friedland, Professor Emeritus of Community Studies and Sociology and current holder of the title Research Professor, is an exemplary embodiment

of such on-going contributions to the well-being of the campus, community, and larger profession.

We might find it rather striking that someone who grew up in a borough of New York City should become an internationally eminent sociologist of agriculture. On the other hand, as Bill Friedland himself has observed, possibly with a slight wink, Staten Island was the most rural borough in New York.

Still, the road to Professor Friedland's groundbreaking research was not direct. It took him, among other places, to Detroit from 1940 to 1953 as an automobile shop worker and steward, and as an activist in the UAW and CIO; then to a doctorate and his faculty appointment in the School of Industrial and Labor Relations at Cornell as an industrial sociologist. It was at Cornell that he developed an innovative educational program that would have profound implications for UC Santa Cruz. He recruited a cadre of undergraduates to a spring semester seminar on field study that prepared them for a summer immersion as agricultural workers and researchers in migrant labor camps, followed during the fall semester by a seminar in which they engaged in an academic analysis of their experiences. This program, the Cornell Migrant Labor Project, was to become the model for this campus's own Community Studies program, which Bill founded in 1969 upon his arrival at UCSC as Professor of Community Studies and Sociology.

Although it is difficult to characterize adequately the general contours of Bill's multi-faceted academic career, two themes in particular stand out for me:

First, his commitment as a scholar to engage in rigorous research that contributes to a more just and humane society, one in which productive labor of all varieties is valued and honored.

Second, his commitment as teacher to challenging students to confront, by direct community involvement, the processes and problems characteristic of the multi-faceted society in which we live; to helping them develop the skills of rigorous academic analysis that will lead them to a complex, nuanced understanding of that society in its local, regional, national, and global formations; and to encouraging them to continue after graduation to apply their education to the betterment of the communities in which they will work and live.

Let me speak briefly to each of these themes.

During his over five decades as an active scholar—scholarship that has maintained its brisk pace since his retirement in 1991--Bill has authored or co-authored a dozen major books and monographs and dozens of influential articles. One of his important contributions has been to apply the perspectives of industrial sociology to a socioeconomic analysis of agriculture as a complex network of production and commodity systems involving, among other things, the corporatization of agribusiness, the mechanization of growing, harvesting, and processing, the profound impact on both agricultural workers and growers, and the rise of labor activism. He has written importantly about tomatoes, lettuce, and other crop-production systems, as well as more generally about the political economy of dominant and alternative forms of agricultural production.

Bill has also made important contributions to the sociology of knowledge in his analyses of the impact of the University's fiscal and political relationship to agribusiness on the kinds of agricultural research that scholars in the University have been encouraged and discouraged to pursue. He has consistently called on his professional colleagues to engage in socially responsible research that considers the impacts of agricultural transformations on workers, growers, and

consumers, as well as on the environment. He has repeatedly asked how academic institutions can encourage scientists to be less exclusively focused on increasing agricultural output and more concerned with the social consequences of their research, and has proposed, in the face of considerable resistance, that the University require social impact statements for all publicly funded research.

Such research has made Friedland a central international figure in the field of the sociology of agriculture and has resulted in several major professional honors for his contributions to the field. In 2005 he received a lifetime achievement award from the Rural Sociological Society. In 2012 he was named Distinguished Rural Sociologist by that society at its 75th annual meeting and lauded for his role as mentor and inspiration to many rural sociologists who study the social impacts of changing structures of agriculture. During the same year, a plenary session at the 13th World Congress of Rural Sociology in Lisbon, Portugal, was designated "Homage to Bill Friedland." This dedication is rarely given and was awarded in honor of his many years of research on alternative agrifood movements.

Bill's commitment to a searching examination of the impact of institutional arrangements on the lives of those engaged in productive work, and on efforts of workers to better their working and living conditions, has also led to his scholarly interest in student welfare and activism. His 1971 book, for example, *The Knowledge Factory: Student Power and Academic Politics*, co-authored with sociologist Irving Horowitz, remains in my view one of the most nuanced and insightful analyses of the student movements of the 1960s and of university administrators' and faculties' responses to those movements. This interest has also been central to his conception of the Community Studies program at UCSC.

Bill has often pointed to the achievements of Community Studies as one of his greatest sources of satisfaction. His powerful and innovative conception of the program, which graduated well over 2000 majors during its forty-plus years of existence, involved the systematic integration of three components: students' six-month field placements in community organizations; a prior preparatory seminar involving careful consideration of the ethical as well as the methodological and practical issues involved in participant observation in the field; and a post-fieldwork seminar focused on a rigorous academic analysis of those experiences and leading each student to the production of a substantial senior thesis.

But Bill's commitment to developing undergraduates' research skills and harnessing those skills to projects for positive social change did not end there. Under his leadership, the program's faculty engaged teams of students in extended research projects that focused on important social issues, among them several that over the years have focused on issues of concern to the Santa Cruz community and have typically included extensive systematic interviews with local residents and community leaders. Among such studies bearing Bill's own direct mark are the 1969 study, *Santa Cruz and the Freeway: A Study of Community Attitudes*, and the 1985 study, *Streetpeople and Straightpeople in Santa Cruz*. In addition to these valuable collaborative projects, Bill has generously mentored and collaborated with individual undergraduate and graduate students on several of his important agricultural studies.

The impact of such educational experiences have often been profound and lasting. For example, many Community Studies graduates currently serve as directors and key staff of social service non-profits and governmental agencies

throughout and beyond the borders of California. Many others have engaged in voluntary contributions to the welfare of the communities in which they work and live. Bill's gift to these graduates has been a gift that keeps on giving.

In 2007, Bill Friedland received this campus's first Distinguished Social Sciences Emeriti Faculty Award, an award bestowed by a committee of faculty and students in recognition of his extraordinary impact as a scholar, teacher, and community servant. I also like to think it was an award in honor of the considerable wit and good humor he has brought to serious topics. Think for example of the title of his wonderful book on the tomato industry: *Destalking the Wily Tomato?* Or think of the title of his lecture this evening on the political economy of the California wine industry, a title that echoes a line from "The Battle Hymn of the Republic" and also hints at a passage from *Revelations* and points, more directly, to John Steinbeck's epic novel, *The Grapes of Wrath*. It is with great pleasure that I present to you Professor Emeritus William Friedland.

About the Interviewer and Editor:

Sarah Rabkin taught in UC Santa Cruz writing program and environmental studies department for over twenty-five years. She holds a BA in biology from Harvard University and a graduate certificate in Science Communication from UCSC. Her book of essays, *What I Learned at Bug Camp*, was published in 2011.

About the Editor:

Irene Reti directs the Regional History Project at the UC Santa Cruz Library, where she has worked as an editor and oral historian since 1989. She holds a BA in Environmental Studies and a Master's in History from UCSC and is also a small press publisher, writer, and photographer.