



INTERVIEW

Interview with Daniel J. Levinson

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INTRODUCTION

What follows is an interview with Daniel J. Levinson, conducted by the Austrian sociologist Dietmar Paier in November 1992 in New Haven, Connecticut. The interview was taped and transcribed; the following version is an abridged version of this transcript that has been slightly edited for readability. Comments on the background to some of the stories told as well as on the contemporary figures mentioned were added by Christian Fleck and Andreas Kranebitter and have been limited to the necessary minimum for comprehension. We wish to thank Dietmar Paier and Sterling Lord Literistic, Inc., for permission to publish this interview, as well as Fabian Gruber for work on the transcript.

INTERVIEW

I: I would like to talk to you about your encounters with Else Frenkel-Brunswik and her role during your research for “The Authoritarian Personality.” When did you come to know her and Theodor W. Adorno?

L: Well. I came to Berkeley in 1941, which must have been two years after or so after the Brunswiks came. I graduated from UCLA in 1940 and spent a year there. I had just covered the field of personality theory and research, which became my main interest, and my two main professors in that were Nevitt Sanford and Else Brunswik. Erik Erikson was also there at that time, and there were many others who were important to me in other ways like Egon Brunswik. I would say that she and Nevitt Sanford and Erik Erikson¹ were the three main psychoanalytically oriented members of the department. But Erikson was primarily a clinician and gave us theoretical ideas. Else and Nevitt were very much interested in science and in doing empirical research, from a rather positivistic, Vienna Circle point of view. But a major theme in her work was translating psychoanalytic ideas that came out of

¹ Erik Homburger Erikson, born Erik Salomonsen (1902–1994), was an Austrian-American psychologist and psychoanalyst. Moving to Vienna from Frankfurt am Main in the 1920s, he underwent analysis with Anna Freud and was awarded a diploma from the Vienna Psychoanalytic Institute in 1933. Fleeing to the United States that same year, Erikson held positions at Yale and Harvard universities before joining the University of California at Berkeley’s Institute of Child Welfare.

clinical work into quantitative research, I would say. Her approach had something in common with Sanford's, but was also rather different.

I: So, how did this idea to undertake such a big study like "The Authoritarian Personality" emerge? Who had the idea?

L: Well, as you've talked to different people, I imagine that's been one of the biggest questions. From the beginning, people have wanted to know, who was the real source of this work. Is it Else, is it Nevitt, is it Adorno? I was a graduate student who was about where you are now at that time, so... How it all began...

I: Yes, that would be fine.

L: Alright. We began in 1943. A man, Mr. Blumenthau, gave 500 dollars through the University for a study of antisemitism, that would end antisemitism forever.² A rather grandiose idea. He owned some theatres, he had become rich after being poor and he did various things for social research. He donated 500 dollars, and somehow it got to Nevitt Sanford. Nevitt was asked if he wanted the 500 dollars to use to carry out a project on antisemitism, and no right-thinking faculty member would turn down an offer like that—and he hired me to work on it.

I: Did Blumenthau contact the Department of Psychology?

L: No, the administration. And I don't know quite how it got to Nevitt, but within the Department of Psychology Nevitt was a fairly obvious choice, although there were others. And then 500 dollars seemed a good basis for starting, and I was employed on it and I probably got 50 dollars a month for a year. We began in an absolutely typical positivistic quantitative way in the culture of academic psychology of that time: we constructed a scale. And the interesting thing about it is that I found ways to formulate, you know, how these attitude scales work. We formulated items. If they were strongly antisemitic, nobody would agree with them, but if they were more moderate, we could get a wider range of responses between strongly agree and strongly disagree. So, that scale worked, it gave us a range of scores and then we had a questionnaire, and we found other things that correlated with that. Some hypotheses we had and some we were just seeing. That was associated with more politically conservative views and more traditional fundamentalist religious views. Nothing very surprising...

I: And that was starting in 1943?

L: Yes, in that first year, 1943 or 1944 we did that.

I: And was Sanford more an advisor to you?

L: Yes, I think it's fair to say that I constructed the scale. We published an article that came out in 1944. I was the senior author of the article, and I wrote the first draft and we worked on it together. I would've done various things with Sanford, and in general he was a good advisor for me, because he gave me a lot of freedom, personal support. So anyway ...

² For a detailed account of this beginning see Fleck 2011: 242f. According to inflation calculators, \$ 500 in 1943 would be worth roughly \$ 8,500 in 2021.

I: Yeah, I was thinking of this kind of relationship, because that's usually the way such studies are done.

L: Well, so my first year we completed this study and published the article. What I'm trying to think is, where Else came into it, and later where Adorno came into it. Do you remember which year Else had a Social Science Research Council Fellowship, when she went back East?

I: In 1942. She had a Social Science Research Council Fellowship, and part of the year she was in Chicago to give courses and seminars.

L: Ok, I'm wrong. The project began in 1942, because I began graduate work in 1941. I was a full-time graduate student in 1941/42 and then I took an unpaid position which Nevitt helped me find at a prison, San Quentin Prison, which is near Berkeley, Marin County.³ At that time the department was much smaller than it is now and there probably weren't more than ten graduate students altogether who were very interested in personality, and there weren't so many faculty members, so I knew how important Nevitt and Else were to me. I didn't understand until later how important I was to them, apart from how good I was. They needed students to work with and there weren't so many. And of course the war, we got into the war in December of 1941, just a few months after I came to Berkeley, so that was another context. Anyway, my first years as a graduate student I took courses with Nevitt, I met Else, I gravitated to them as mentors. In 1942/43 I was in San Quentin, I lived there, but I came into Berkeley on weekends and maintained my connections there. I got married in the middle of that year in January 1943.⁴ But I came on the weekends and I took a seminar, you know, did things on Mondays, so I took a weekday to be in academic contact with Nevitt. He was my advisor on the prison project, and I talked to Else about it. And also, he had a little money, so he wanted somebody to read some literature and go through that. But then I believe that he got the money for the antisemitism study early in 1943, like January or February or so, and asked me if I would do it. I was interested to do it, and then I spent two weekdays each week in Berkeley. Since I wasn't paid in San Quentin I could have my own choice ... I was in the Psychiatry Department, it was based in the Psychiatry Department, so I began coming back and working on this study. And then we completed the study, published it. That would've been in 1943. And Else was away 1942/43.

I: Yeah, the study covered the period from early 1943 to the end of the year and the article was published in 1944.

L: I think it was published in early 1944.

I: Was the plan to ...

L: We had no long-term plan, because we had no money. We didn't begin with the plan to write a monumental, four-year study of "The Authoritarian Personality." We began very modestly, this was one of several projects that Nevitt was in. He was an assistant professor in the department then, he was research associate at the institute of Child Welfare, he was involved in various things and so the antisemitism project was a rather small one that he delegated to me, also before Else went away—

³ For the authoritarianism research carried out at San Quentin State Prison in California, see Kranebitter 2022.

⁴ Daniel J. Levinson was married to Maria Hertz. In an informal conversation at the beginning of the interview regarding gender differences in Else Frenkel-Brunswik's role as a mentor for himself and Maria, Levinson mentioned that: "Maria knew Else in Vienna a little bit. She, Maria, was a few years older than me and also left Vienna in 1939 and went to England, but she had been a student for three years at the University of Vienna, was interested in psychology, and she knew the Böhlers and Else. I remember it because Maria didn't form the same mentorship relationship with Else like I did, it's an example of the gender difference."

she must have gone off in the summer 1942. So there was a period of about six months or so in the first part of 1942 when she was around I was working on this. I discussed it with her. She probably discussed it, probably the three of us talked about it, but Nevitt spent part of his time in the department in that office on campus and part of his time at the Institute of Child Welfare, which is about three or four blocks away. She was primarily at the ICW and I would go up there sometimes. I'd meet her up there usually, but they were somewhat connected. They and Erikson, the three of them were the psychoanalytic members of the department that didn't know quite what to do with psychoanalysis. She was there largely because of Tolman⁵ bringing Egon over, and Jean Macfarlane⁶ of the ICW had played a big part, did you come upon her name?

I: Yes.

L: She was a professor in the department and she was head of this research program at the ICW. And I believe that she was instrumental in bringing Erikson and Sanford together, she and Harold Jones⁷, have you come across that name?

I: Yes.

L: Macfarlane and Jones were the two senior administrator people in the ICW. Jones was more in child development than Macfarlane, and they brought in these new people with new ideas, but they themselves were not psychoanalytic. They had a hard time, sort of. They supported Else and Nevitt, but it wasn't their thing, so that brought Else and Nevitt more together. So the three of us talked in 1942, before Else went away. In 1942/43 she visited different places, she was getting interested in social science, anthropology, sociology, etc., and also met Henry Murray⁸ at Harvard. There was a new intellectual current in psychology and social science at that time, I mean, psychoanalysis was part of it and anthropology was getting interested in the study of culture and personality, which is on the boundary between the two fields. She was meeting with people in this field. The study of personality was just coming in, it didn't exist in American psychology before.

I: In that year or in that period Else studied together with Alfred Kroeber⁹ at the Department of Anthropology in Berkeley, that's right.

L: She knew Kroeber. I would say that the three of us were forming a vision of social psychology. It was rooted in academic psychology, psychoanalysis, as well as in the social sciences, particularly anthropology. Sociology began moving that way somewhat later, there weren't so many sociologists. Sociology started more in the late 1940s and 1950s, it was a counterpart to the culture of personality.

⁵ Edward Chace Tolman (1886–1959) was an American psychologist and professor of psychology at the University of California, Berkeley, from 1918 to 1954. During the McCarthy era, Tolman was one of the most outspoken critics of the Berkeley campus's demand that each employee must sign the infamous loyalty oath.

⁶ Jean Walker Macfarlane (1894–1989) was an American psychologist and professor at the University of California, Berkeley, from 1929 to 1961. Macfarlane founded Berkeley's Institute of Child Welfare in 1927.

⁷ Harold Ellis Jones (1894–1960) was an American psychologist and professor of psychology at the University of California, Berkeley, from 1931 to 1960. From 1935 he was the director of the Institute of Child Welfare (later renamed the Institute of Human Development).

⁸ Henry Alexander Murray (1893–1988) was an American psychologist and professor of psychology at Harvard University, and with Christiana Morgan co-developed the "Thematic Apperception Test" (TAT), which was used extensively in *The Authoritarian Personality* (Adorno et al. 1950).

⁹ Alfred Louis Kroeber (1876–1960) was an American cultural anthropologist and the first professor appointed to the Department of Anthropology at the University of California, Berkeley, as well as director of the University of California Museum of Anthropology.

But Else, Nevitt and I had a kind of a vision of a social psychology that wasn't just personality. We were interested in placing the person within the social, within society.

I: That's the general idea of TAP.

L: But it was new, I mean, it was new in the world, relatively new. I mean, it had begun in the 1930s but it hit the United States in the late 1930s and was really beginning to form in the early 1940s, and I would say that we represented that in Berkeley. I think what Else did mainly in her fellowship year 1942/43 was to see what was happening in the East. People like Clyde Kluckhohn¹⁰ and Henry Murray at Harvard, who wasn't explicitly social, but was oriented that way and was also trying to study personality in more quantitative ways but drawing upon Freud and Jung and others. I mean that was the spirit of the times.

I: So, the context was, there were three members to form a group and the vision was there, but there was—compared to the large study...

L: Yes, how would that evolve? The first step was this little antisemitism project, which Else knew we discussed. But during the year from December 1942 to 1943, when she was away, we completed that study.

I: How did the study become such an ongoing project?

L: I'm trying to remember how it was, when she came back in the summer of 1943. She was very excited with the previous year, she had been travelling, she had been nomadic. Now she wanted to get into new work and we talked about this small project. We had no further funds at that point, but I knew that I wanted to do my doctoral dissertation on that and she wanted to do something. We just decided to continue the work together, the three of us. We established not a large project but a kind of partnership and her part in it was, I would say, crucial to the evolution, to where it led. She decided to do a case study. She interviewed a woman who was a psychiatrist and was connected in some way to the department and who—good fortune—was antisemitic [laughs], but also understood that she was antisemitic. She understood that it was a crisis, but she felt that way. I believe she came from the southern part of the United States, she had various kinds of prejudice. But anyway, she was not a typical antisemite but she had enough of that, so it seemed worth talking with her—and so Else invited her and they sat a few hours, several times, to talk about her life and her views about it and other things. Then Else wrote a case study, which she must have done in the fall of 1943, and which probably came out in 1944 or 1945. That was her contribution. She talked about it with Nevitt. I don't remember whether she was the sole author or whether she and he authored it together, but I think she was the author.

I: What was Nevitt Sanford doing? It seems in the beginning every part was distinct? I just want to figure out the relationship between the three of you. Was it more of a loose group?

L: That's right, it was loose, but we had a lot in common intellectually and we had a sense of... We were bending together to do a noble project in a world that allowed it, but where it was a little strange. I mean, for instance: Tolman was interested, Tolman had been studying animals, learning those rats, but he was becoming a social psychologist. Tolman was a great psychologist, I believe, and he was

¹⁰ Clyde Kluckhohn (1905–1960) was an US anthropologist who wrote extensively on the Navaho in Southwest America. He taught at Harvard, where he was both a member of the Department of Social Relations and director of the Russian Research Center. In the early 1930s, Kluckhohn had studied in Vienna, where he also underwent psychoanalysis.

chair of the department, so he supported that. So there was support for it, but still, we were doing something special, and we had that sense of developing a way of thinking and a way of doing research that was unusual and was special. You have to remember that Else was still finding her own way in this country, she was then in her early thirties. It was true for both Else and Nevitt. You are working with others who are much further along and you feel indebted to them and they provide a context and some of the structure for what is done. Typically, somewhere around 30, you go out more on your own, and you are going to do business for yourself. Intellectually there's continuity with the past but also changes. And Else did that, was doing that in part with her research at the ICW— motivation, child development—and that was important. But I think she was fitting into the structure of the Institute, and I think what we were beginning was her first business of her own, although it was with others. I think this was true for Nevitt Sanford also. He had just finished graduate school, he had done his PhD and came to Berkeley I think around 1939 or 1940 and worked at the Institute, but on a large project. Now he had a chance to make his own project. So for me as a graduate student there was a wonderful opportunity to do my own work, but with people who were further along. I mean to me, at 22 or 23, they were ten years older and seemed a hundred years older. It was wonderful to have older people who were interested in me and also appreciated me.

I: So, a scientific outcoming for all three?

L: Yes, that's right. That's my view of that. That we each had our own very strong individual, independent reasons for being in it, but we created something that was collectively special and different from what anyone of us would have done alone. And so it was very much a collective enterprise. Else might have done some things of that sort, and would have done it in a different way because her interest was more in personality and less in ideology. I saw antisemitism as an ideology, which I wanted to relate to their ideologies—nationalism and political ideologies—and relate ideology to personality, and that was a particular theme for me. Nevitt was more focused on personality as such. I don't know that we had any money for the project. I think then I was a teaching fellow for Egon Brunswik and others in the department and I always thought Else was getting paid at the Institute.¹¹ Apparently not. So, I was heading towards my dissertation, I don't think we had any money. That was 1943/44, and now we come to Adorno and Horkheimer.

I: Ah, so although in the years of 1943/44 there wasn't much money for this project, you continued...

L: We were working very hard, yes, that way we became more of a triad rather than two dyads.

I: So, we reach the second year of the study.

L: Yes, that's right. And as I say, the big intellectual development then was Else's case study in developing more ideas about antisemitism as an aspect of personality. I think her case study then brought our work to the attention of psychoanalysts. See, what Nevitt and I did was more standard academic psychology. There were ideas about personality, but they were hardly developed. Her case study, then, was concerned with what would have been called the psychodynamics of antisemitism.

¹¹ "However, due to the so-called Anti-Nepotism Regulations that were in force at California universities at the time, and to the fact that her husband, Egon Brunswik, had obtained a regular appointment at the department when his Rockefeller fellowship was terminated, her position was only that of a research assistant." (Fleck 2011: 25).

Well, Ernst Simmel¹² was a psychoanalyst in Los Angeles, and he had written an essay about anti-semitism, the essay of a clinical psychoanalyst about things like the place of projection in antisemitism or so. There were very few psychoanalysts then in San Francisco and Berkeley, there were some, but not many. There were more in Los Angeles, including Simmel and Otto Fenichel.¹³ Else probably knew some of them from Vienna. They would come up to meet with the San Francisco psychoanalysts.

I: Well, I think that at least she had contact to the Los Angeles analysts through the San Francisco analysts: Siegfried Bernfeld,¹⁴ and Hildegard and Bernhard Berliner,¹⁵ and she knew them quite well.

L: Yeah, right. That's right! And I had one connection myself. My wife Maria had an aunt in L.A. who was a psychoanalyst, Frances Deri.¹⁶

I: Frances Deri? Hildegard Berliner mentioned this name!

L: Right. So, we knew the Berliners and the Windholzs,¹⁷ and Anna Maenchen¹⁸... There were a lot of connections, since they all mainly came from Germany and Austria. So, I think we were invited to present something...

I: At the psychoanalytical meetings?

L: It was not at the meetings. What we were doing was too far from clinical psychoanalysis. It was very marginal to them, they did not quite know what to do with that. Anyway, there was a contact, and then, Teddie Adorno lived in L.A.

I: So you met Teddie Adorno in L.A.?

L: No, there are another couple of links to it. But he lived in L.A., he knew Simmel and met with him, and he was connected with psychoanalysts there, partly as a refugee community, and he had that interest. And Max Horkheimer was in New York, and the old Institut für Sozialforschung was mainly

¹² Ernst Simmel (1882–1947) was a German-American neurologist and psychoanalyst. In 1944 Simmel coordinated a symposium of California-based psychoanalysts and social scientists on antisemitism, which was also attended by R. Nevitt Sanford and Else Frenkel-Brunswik. The contributions were published in 1946 (Simmel 1946) and have made an important contribution to the study of *The Authoritarian Personality*, since they connected Frenkel-Brunswik and Sanford with Horkheimer (see Fleck 2011: 238 and 250).

¹³ Otto Fenichel (1897–1946) was an Austrian-American physician and psychoanalyst who fled Austria via Oslo and Prague to Los Angeles in 1938. He was one of the contributors to the 1944 conference on antisemitism organized by Ernst Simmel (1946). With Otto Fenichel, Bernhard Berliner, Emanuel Windholz and others (see below) he was one of the founders of the San Francisco Psychoanalytic Society in 1942.

¹⁴ Siegfried Bernfeld (1892–1953) was an Austrian psychologist, psychoanalyst and educator. Like Else Frenkel-Brunswik, Bernfeld was born in Lemberg (today Lviv) and studied philosophy at the University of Vienna.

¹⁵ Hildegard Berliner (1907–2004) was a German-American child psychologist. Her husband Bernhard Berliner (1885–1976) was a German-American psychoanalyst and is said to have been San Francisco's first émigré analyst.

¹⁶ Frances Deri, née Franziska Herz (1880–1971), was an Austrian-American psychoanalyst who emigrated to the United States in 1935 and practiced as a lay psychoanalyst in Los Angeles.

¹⁷ Emanuel Windholz (1903–1986) was a Czech-American physician and psychoanalyst. From 1936 to 1939 Windholz was the president of the Prague Psychoanalytic Study and emigrated to San Francisco in 1939.

¹⁸ Anna Maenchen (1902–1991) was a Lithuanian-born American psychoanalyst, having studied philosophy and history at the University of Vienna in the 1920s, where she was in close contact with Siegfried Bernfeld and Anna Freud. After her escape from Vienna to the United States in 1938 she moved to Berkeley and was a training analyst at the San Francisco Psychoanalytic Institute.

in New York but with members in L.A. and Chicago and other places. So anyway, Horkheimer had just, by some magic, persuaded the American Jewish Committee to create a Department of Scientific Research. He got them to make it. They had a small group and mainly put out rather simple-minded propaganda against antisemitism. The AJC is one of the most politically conservative groups in the country. And this Marxist from Germany got them to start a Department of Scientific Research and to make him the director of it!

So, Horkheimer became the director of this group and he asked them for money so that they could do their own research, and they should provide funds to scientists—social scientists in universities around the country—to study antisemitism. The money was made available and then there were six or eight projects around the country. Ours was only one. The AJC in May 1944 invited a group of American scholars to a conference on religious and racial prejudice. So that was the beginnings of their attempt to develop a series of studies through the Department of Scientific Research.

It was headed first by Horkheimer, then Samuel Flowerman¹⁹ followed and succeeded him as the head of it. They put out these volumes, and in each of them there's at least one member of the Institute [for Social Research]. There's our group, then there's Bruno Bettelheim and Morris Janowitz²⁰—Bettelheim had some connections with them, I don't think he had been part of it. And Nathan Ackerman²¹ I believe was from the Institute and Marie Jahoda²² was the former wife of Paul Lazarsfeld,²³ and then Paul Massing²⁴ was part of it and...

¹⁹ Samuel H. Flowerman (1908–1958) was a psychologist and psychotherapist who directed the Department of Scientific Research of the American Jewish Committee until 1951, initially together with Max Horkheimer, after his resignation alone. Together with Horkheimer he was the editor of the five-volume *Studies in Prejudice* series.

²⁰ Bruno Bettelheim (1903–1990) was an Austrian-American psychologist and professor of psychology at the University of Chicago. Having been released from the Dachau and Buchenwald concentration camps, Bettelheim wrote a widely read psychological study of his camp experience, "Individual and Mass Behavior in Extreme Situations" (Bettelheim 1943). Morris Janowitz (1919–1988) was an American sociologist and was appointed professor of sociology at the University of Michigan in 1951 and finally at the University of Chicago. He was well known for his research on propaganda and the military. Together, Bettelheim and Janowitz wrote one of the five volumes of the AJC's *Studies in Prejudice* series, which dealt with authoritarianism among military veterans (Bettelheim/Janowitz 1950). On the tensions between Bettelheim and Janowitz on the one hand and the Institute for Social Research on the other, especially after Horkheimer's and Adorno's remigration to Germany, see the correspondence files on the republication of this study (YIVO, AJC-Archives, RG 347.17.10 GEN-10, Box 20).

²¹ Nathan W. Ackerman (1908–1971) was a New York based American psychiatrist and psychoanalyst. In 1957 he founded the *Family Mental Health Clinic* in New York, today named the *Ackerman Institute*. Together with Marie Jahoda, Ackerman published the volume "Anti-Semitism and Emotional Disorder" (1950) within the *Studies in Prejudice* series.

²² Marie Jahoda (1907–2001) was a social psychologist. Born in Vienna, she lived in the United States from 1945 until 1958, working initially as assistant to the directors of the Department of Scientific Research of the American Jewish Committee. In this capacity she regularly attended the meetings of the Berkeley Public Opinion Research group. After her resignation from the AJC she worked at the Bureau of Applied Social Research and became a professor of social psychology at New York University. From 1973 onwards she was at Sussex University in the UK. She is known as the lead author of *The Unemployed of Marienthal* (1933) and continued writing on the social-psychological consequences of work and unemployment.

²³ Paul F. Lazarsfeld (1901–1976) was born in Vienna and came to the United States in 1933 on a Rockefeller Fellowship. In the 1930s he collaborated with Horkheimer's Institute for Social Research and directed the Princeton Radio Research Project, where he hired Theodor W. Adorno for a music study. In 1940 he became professor of sociology at Columbia University, where he founded the Bureau of Applied Social Research. He acted on the advisory board of the *Studies in Prejudice* study.

²⁴ Paul Wilhelm Massing (1902–1979) was a German sociologist and was affiliated with the Institute for Social Research. After having been imprisoned in one of the "early" concentration camps in Nazi Germany, Massing emigrated to the United

I: Leo Löwenthal²⁵...

L: Löwenthal... This is the broader context. First the Nazis and then the war, WW II, brought the conservative Jewish groups into a more liberal point of view or more readiness to look at things, and Horkheimer was the catalyst for that and the organizer. He played the administrator function, a very powerful administrator function. So he contacted us, I mean ...

I: He did?

L: Yeah, he was the moving force in this. He brought scholars together from all over the country. We were in that first group, but then what we were doing, people knew about it. Alright, that first article was published in 1945, but Else must have written it in 1944, and it was Frenkel-Brunswik and Sanford. And see, it wasn't called a case study, typical of those years of that culture of psychology. It was called "Some Personality Correlates of Antisemitism." The title suggests quantification, that was what she did in 1944. And then "A scale for the measurement of antisemitism." That was the first publication in 1944, that year after we finished it, and it's Levinson and Sanford.

I: So, when exactly did Adorno join the project?

L: I think that would be the fall of 1944.

I: Did Horkheimer suggest Adorno?

L: Oh no, he didn't suggest Adorno. He said if you want to do a project, we'll pay for it if Adorno is in it. So we considered Adorno as something between a gift and an imposition, because it was a condition. And it was obviously complicated by the fact that the three of us were in Berkeley and the main work would be in Berkeley but Adorno would be in L.A. So we would meet once a month or so, 8 or 10 times a year.

I: Could one say that you accepted Adorno because it ensured that you got the money for the study?

L: It enabled us to do it. I mean, we could do a larger project. It was not a lot of money, but we may have gotten, I don't know, 20 or 30 thousand dollars a year. So we got the project and when the project was defined, we then gave ourselves a name. We just took "The Berkeley Public Opinion Study" [BPOS], right? And the old German institute was now the Institute for Social Research, so it was a joint project between these two entities. Officially Sanford was the director of the BPOS and Adorno was co-director of this project representing the Institute for Social Research. That was the formal arrangement. We didn't know him before so it was a somewhat forced marriage, although our relationship evolved over time, which is something to talk about. But we began very far apart. We were for the Vienna Circle and logical positivism and measurement and statistical analysis—and I am emphasizing that because I feel now that we were very far toward an extreme in that. I mean, now I would be much more in the middle, but that was what we were then. We then tried to find common ground. What we had in common was an interest in psychoanalysis and in individual per-

States in 1935 with his spouse Hede Massing, who worked for the Soviet foreign intelligence service (see Massing 1951). Within the *Studies in Prejudice* series, Paul Massing published the study "Rehearsal for Destruction: A Study of Political Anti-Semitism in Imperial Germany" (1950).

²⁵ Leo Löwenthal or Lowenthal (1900–1993) was a German sociologist and philosopher affiliated with the Institute for Social Research. Together with Norman Guterman he published "Prophets of Deceit. A Study of the Techniques of the American Agitator" (1950) within the *Studies in Prejudice* series.

sonality and in society, and the relationship of the individual to society. All three of us in Berkeley considered ourselves sociologically oriented, but I think we didn't understand how limited our sociological thinking was. We were very much involved in operationalizing concepts and measurement. But Adorno couldn't care less about measurement...

I: I've always wondered what a man like Adorno does with an approach he isn't used to at all?

L: Well, I think there are a lot of things we did for him and a lot of things he did for us. I consider it, all in all, a marvelous collaboration.

I: Was it?

L: Yeah, not that it was always happy or even mostly happy, there were lots of conflicts, but I believe that the end result was different from what would have been—and much better than what would have been—if any one of us had been stranded alone or if it had just been a Berkeley project, or just been a project by the Institute... There was some kind of creative work in it.

I: So what was his influence?

L: Well, he helped us think more broadly and stand back a little from measurement. He was somewhat free from the concept of operationalism, which has a kind of tyrannical quality. You can't talk about something unless you say how you measure it. I can say now that's ridiculous, but I didn't believe that then. We felt we had to find ways of measurement for whatever concept. And then we tended to get involved in the measures, whereas Adorno represented sociology much more fully than any of us could. We were all interested in sociology and anthropology, I was just doing a lot of reading in personality and culture. I read Erich Fromm,²⁶ Wilhelm Reich.²⁷ I would say the way I came to think of it, implicitly if not explicitly, was that we started with the study of ideology and that anti-semitism was a form of ideology. Then we wanted to link ideology to personality, to look more deeply within the person. We also wanted to relate ideology to society, and the family as a link between society and personality, as an institution of society, part of the sociological structure of society, but also as a medium, as the context in which personality evolves. I mean that basic way of thinking was very important. Adorno was primarily a sociologist. His use of psychoanalytic ideas sometimes seemed naive to us, just as our sociological ideas must have often seemed naive to him.

I: He wasn't very engaged with empirical research, but more in terms of enlarging the view on different related fields, research objects...

L: Yeah, that's right.

I: And did he help to put together the different fields in terms of an integration of the whole subjects?

²⁶ Erich Fromm (1900–1980) was a German-born American psychoanalyst and social philosopher, originally affiliated with the Institute of Social Research. After his eventual departure of the Institute (see Wiggershaus 1988: 298–305; Wheatland 2009: 81–87), the influence of his earlier research on authoritarianism (Fromm 1936) on the study *The Authoritarian Personality* was nearly neglected by the Institute (see Fahrenberg and Steiner 2004 for details).

²⁷ Wilhelm Reich (1897–1957) was an Austrian physician and psychoanalyst. Due to mostly political conflicts, Reich was expelled from the International Psychoanalytic Association (IPA) in 1934. His book *The Mass Psychology of Fascism* (1933) had a great impact on research on authoritarianism.

L: Well, I would say that we didn't fully integrate, as I look at it now. The book is more about ideology and personality and is not sociological. I think that reflects the limited sociology of the Berkeley group. Adorno got us to be more sociological than we were, but he couldn't take us further because he couldn't connect enough to empirical study. I'll give you a more concrete example. Adorno pushed us to develop the F-scale. I think all three of us always regretted calling it the F-scale, it's not a fascism-scale in fact. I don't know why, I mean that came from Teddie, but we didn't have to do it. I guess it's just one of those compromises we made. He made many he regretted, but we might as well have called it the "A-Scale" for authoritarianism. We weren't sure at the beginning what we would call the personality syndrome that we then came to call the "authoritarian." Teddie's idea was actually different and somewhat simpler. He just said, "Let's have items that have more indirect indicators of fascist tendencies." He didn't actually see this as a personality scale. He had a number of ideas about that. I remember one of them, the paradox that the Nazis could be extremely cruel, could kill a person or millions of people without guilt, but would be very sentimental over the death of their dog and see that as somehow tragic. He thought that a very cruel person could be sentimental in that way so he would say, "Think of statements about the death of a dog." But you can be sad about the death of your dog without being a fascist, obviously, and he had no idea about how to make an item for it, and he didn't ask himself, "What do I mean, what is it about the theory of personality...?". He pushed us to develop a scale and he was important for developing the F-scale. Then each of us tried to think of items, statements you agree with, and the agreement to the statements is a reflection of fascism or authoritarianism or whatever we were going to call it. So each of us thought of items, and I'm sure thought of a number of them, and we probably had a hundred altogether as a pool that we worked from. But we could use almost none of his items because he didn't know how to do that. It was a way of thinking that simply didn't have any meaning for him. That's an example of how we helped each other, how we made use of him even though we also didn't use what was not useful. Some kind of creative synthesis happened, and overall we were a collective enterprise with contributions from all of us. So my own interest in sociology was very much influenced by him.

I: Could one say that Adorno's intellectual influence was bigger than his contribution in terms of empirical research?

L: Well, anything that was quantitative. If you take the chapters he wrote, he analyzed the interviews. He analyzed the same interviews that Else Frenkel-Brunswik analyzed. If you look at the book: You see that I am the author or co-author of almost all chapters in the first section, as I was doing a lot of this quantitative work and I was mainly responsible for the development of the F-scale. We all contributed ideas for items in discussions of theory, it's very collective, but I was sort of the technical expert on the use of scales. I also did interviewing. Part two, see, is called "Personality as revealed through clinical interviews and projective material," and Else is the author of all of those chapters, as one mode of analysis of the interviews. And then there's part three, "Qualitative studies of ideology," which is another analysis of the same interviews, and Teddie is the author of those.

I: He used Else's interviews then?

L: Well, Teddie did no interviews, Teddie couldn't do a research interview. Someone said something interesting, he would start thinking about the meaning but then forget the push. But Else and Nevitt and I worked out the interviewing, the structure of the interviews, what we would cover with them, and the theory of the interviewing. That was done together and there were a number of interviewers. Else, Nevitt and I did conduct interviews. As I recall, we did one or two interviews with each person. So, we had two or three hours of interview material. Teddie did it his way, he had all these different types. You see, Teddie ended up being in only one section of the book, but I would say that his influ-

ence was broader than that. And Else ended up in only one section of the book. Except for the chapter on the F-scale and the concluding chapter, which were by all of us, every other chapter has an author name, which is a way of distinguishing credit to some degree. Credit became a very big issue, as you can imagine, and Sanford then mainly did the case studies. But I think Teddie worked on a number of social science projects in this country with different people, and as far as I know, it never worked out very well.²⁸ I mean they couldn't come together to allow him a real voice. They kept insisting that he should start thinking right the way they did. Yet I think one of the creative things about this is Teddie had part four, in which he could present his own thinking. These are empirical studies but they are not quantitative and they are not what anyone else in the world would do.

I: How was the relationship between you as a positivist...

L: At least as far as empirical research is concerned, we were also interested in Marxism and the broader philosophy of science and so on, so we had a basis for...

I: What was Else's relationship to Adorno?

L: Well, I would say the first division was between the three of us and Adorno. We had to work out a modus operandi to work together and we had the problems I mentioned, but I also think we found a workable way and also always liked Teddie. I'm not sure what the personal relationship was, we weren't that close. We were together maybe 8 or 10 times a year for a day or two, so we didn't spend a lot of time with one another. That is one thing. Second, for purposes of the contract, Nevitt was the director of our study. So he dealt with Adorno and Horkheimer about things that had to be negotiated, you know, funds for the coming year or so. The main project lasted three years, so we got the funding for 1944 I would say in the fall. And it went on until 1947, I think, when the main work was done. In 1947 I left Berkeley, I got my degree and went to Cleveland to take a teaching position. The whole project disbanded and there were no more salaries when we wrote the book. The main writing was done after 1947.

I: But at least from 1944 to 1947 your financial support was quite enough to pay everybody?

L: There was just a lot of negotiating. We had our own offices.

I: Did you have a secretary?

L: Yeah, we had a full-time secretary.

I: Who were these people?

L: Let's see, there are several categories of staff members. Three staff members were graduate students who were each brought in to do a particular project. Betty Aron was hired to do the Thematic Apperception Test (TAT). She did the interviews and we also gave her the TAT, and then we had them analyzed in that chapter. We wanted to do a sample at San Quentin Prison, where we had connections to, and Bill Morrow arranged for the study and he did it, so that was his particular project and became his dissertation. And then my wife, Maria Hertz, was working at the Langley Porter Clinic in San Francisco. She spent a clinical training year there.

²⁸ Levinson is referring to the participation of Theodor W. Adorno in Lazarsfeld's Princeton Radio Research Project, the only empirical study Adorno worked on during his years in the United States (see Fleck 2011, chapter 5, for details).

I: Didn't Else spent a year there, too?

L: Else spent a different year there, but Maria worked in the clinic as a psychology trainee and during the year she began looking into some things relating to authoritarianism. Then it was arranged that she would give questionnaires to a sample of patients and then interview them and do something like the San Quentin study. There were two specialized samples. That was going to be her dissertation, but she wanted to become a mother and didn't take her exams. Anyway, those were three studies by graduate students, which became chapters, and in addition there were some undergraduates and graduates who were working on the statistics. There were some people who were doing interviews, probably four or five of them. See, we list the people who did the interviewing here, or people who helped. And Teddie had a small staff in L.A. who did various things but it couldn't be included in the book. It was interesting for essays but couldn't be used in a more formal research. So in Berkeley there was a staff of maybe 8 to 12 people. We had weekly staff meetings and invited others to come in, and had offices in downtown Berkeley.

I: Wasn't it in Shattuck Avenue?

L: On Shattuck, yes. But Else, I don't think the personal relationship to Teddie was a big factor for any of us. We knew him, we liked him, we sometimes had to find tactful ways to get him not to proceed with ideas we felt couldn't work. Sometimes we accepted it or he just did it... We understood that it was an incomplete integration but his intellectual contribution I would say was more contextual and I think, in some ways, just helped to broaden the scope of our thinking, although that breadth is not strongly reflected in the book. I think the book is very focused. And we were all ready to move that way and that was one of our strengths, but then he helped move us further.

I: But what about the relationships between you, too. Else complained that she didn't get much credit for the work, or not as much credit as she felt would have been her due.

L: Yeah, maybe I can address that better if you say a little about what you understand to be the case.

I: I don't know what she said. I read it in the papers of Brewster Smith.²⁹ On one of his personal notes it says that Else felt she wasn't credited enough for her contribution. I don't know what would have been the reason for that.

L: Alright. Let's try that one. Before, I didn't say what the hardships were. The relationship between Else and Nevitt was problematic. Not from the start, but as soon as we were seen as a project and we were in a larger world, then there were questions of credit. Each of them felt that they had made an important contribution and, at times, felt that the other one minimized or neglected it. But I would say there's a sociology of this, which is that the world around us wanted to know who was the real hero of this, who was the intellectual god of this, the source. There were those who said it was Nevitt, and those who said it was Else. Nobody said it was Teddie or me, but I believe that the world played a very big part in increasing the tensions and dividing them and did very little to support the relations. We had to do that. I felt that that was one of my personal functions in our group to help us stay together. But certainly in 1946 or 1947, this project was getting to be known in the country. In the 1940s, California was a little bit like Siberia. It was very far from the center of intellectual life in this country, which was in the East and a little bit in the West. I think Berkeley was known to be a good

²⁹ M. Brewster Smith (1919–2012) was a US social psychologist who graduated from Harvard in 1947. During WWII he served in the U.S. Army and acted as a liaison officer for the study which became known as *The American Soldier* (see Stouffer et al. 1949 for the first of the four-volume study). Brewster Smith held professorships at several US universities.

university, we didn't feel that we were unknown, but ... There was a lot of interest in the project. The American Psychological Association (APA) has divisions like the division on social psychology or personality. Both Nevitt and Else were active in the APA and the division on social psychology published a handbook of social psychology in 1946 or 1947.³⁰ Something like that. And they knew enough of our work so that Nevitt and Else were asked to write a piece in this handbook on prejudice. They began doing it and then decided to include me, so the three of us did this. And the names were in alphabetical order, which meant that Nevitt's name was last. That became an irritant to him, understandably. There was an interest in our study. I would say that by 1945 or 1946, it existed in the world of psychology and sociology. I think it was partly because of the times, that is the war, the fight against fascism, and the sense of mold on our great democracy was very important in American culture then. Our study signified that within psychology. I mean, our study more than any other represented that interest to do research that was of significance in the world, and was also well done scientifically, positivistic.

I: Was it that Sanford was the last in the listing, and that made him feel jealous of Else?

L: That's one of the things. There are things that just came to my mind as an example. Things happened with both of them, that made them feel that the world would underestimate their contribution.

I: Mm, both felt the same.

L: In my view, both felt the same, and in both there was a mixture of rationality and irrationality in that. I'm sure you or I would have felt the same in our own particular form in the same situation. But, you see, Nevitt never did any one distinctive piece of work, he was always the advisor or the manager of it. He had to deal with Horkheimer more than the rest. But there's no one part of this that's distinctively Nevitt's. It's clear the interviews are Else's, and the scales are mine. Now, even though he didn't directly do one part of work, he was involved in it all, so in some ways he could easily be neglected. On the other hand, it's also true that he was not directly responsible for one thing, that's a limitation. So he was in danger of being neglected. At the same time, he was identified as the director of the project. That's one part of it. And then Else for her part, you see, she did much of the writing, it was her work. But there were a lot of social forces limiting the credit that she got. At least until after the book came out.

I: Why?

L: Ok. One: She had no academic appointment. That made and still makes a big difference in this country. I mean, in Europe you have the phenomenon of "Herr Professor" and a lot of "Dozents" or whatever it is, and that's very hierarchical and it's very hard at the bottom or the middle of the hierarchy to make your name public. It's harder there than here because there's a more explicit structure or hierarchy. Things are somewhat freer here, but there is still a hierarchy, and on several hierarchies Else had a lower position than Nevitt although she was in fact the same. He was moving from assistant to associate to full professor, he could apply for grants. If money came in, it went to him, it couldn't go to her. We speak of being on the tenure track. If you're appointed as an assistant professor, ordinarily it means that you will be considered for promotion to associate professor and then to

³⁰ Around 1944/45 the American Psychological Association was reorganized and divisions were established. One of them was dedicated to "personality and social psychology." The production of a "handbook" might have been discussed back then, because the last one had appeared in 1935, but the new Handbook of Social Psychology came out only in 1954, edited by Gardner Lindzey and Elliot Aronson. Neither Frenkel-Brunswik nor Sanford contributed chapters to the three volumes of this Handbook.

full. If you have some other appointment, you have no assurance that your appointment will be renewed, you are not on the path leading upward within the faculty. There are lots of positions and she was like a research associate—I'm sure you have an equivalent there. What is not generally recognized is that the size of the non-tenure track group in a department may be as large or larger as those on the tenure track, and they are the disadvantaged ones. Second: Most people who are not on the tenure track are women—gender is an important issue—and faculty members who consider themselves to be liberal on social questions don't want to recognize how prejudiced they are and how many forms of authoritarianism there are within the university. You see it more when you are students looking up—as we move up, we tend not to notice it that much... I think it's very important and it was very important for Else. So because Else, as a marginal member of the department—and being a woman was a major disadvantage—couldn't apply for research funds, she worked on someone else's project at the Institute. She and Nevitt were colleagues but it was his project. Nevitt became director of our project not only because in some ways it was simpler. Horkheimer preferred to deal with a man and wanted to deal with somebody who had the more academic status in the department and so on. So, that is a fact, a woman who is marginal is like a wife with a husband. There are a lot of gender issues in this. Else was a very independent person and wanted freedom to do her own work. She would be very helpful to a lot of people, but Else was not what today we would call a feminist. She was not fighting for the rights of women in the university. She was just trying to find a way for herself. She never made it a political issue, that she didn't get a salary. Later on, in the 1950s I think, that nepotism-rule was taken away and there was a question of whether she would become a professor. Did she become a professor?

I: No.

L: She didn't.

I: No, but the decision was made that she should get a professorship. That would have been the year 1958, October 1958, I think, but she died earlier on March 31, 1958. There must be a record at the University of Berkeley, which says that the department agrees that further representation should be done in one or two main fields of the candidate, meaning Else, that Dr. Brunswik is a highly qualified person, and that an appointment as professor of psychology should be sought for her in connection with sociology and/or the Institute of Child Welfare.

L: In connection with that, not just psychology?

I: Not psychology.

L: Not psychology, Jesus.

I: And then they agreed that an appointment up to one half-time would be acceptable and that this appointment should be done. Also, if the money for that has to be borrowed ...

L: I mean, it's a position in the structure.

I: They wanted to give her a ...

L: A half-time professor jointly with another department. Now you see that is a horrible thing. You have to understand that. That's exactly the fitting ending to the whole story. Else should have been made professor years before. It's very hard now, it's a sociological, psychological ... It's hard now to

recapture that but by every criteria Else was one of the most distinguished members of the department and she could have been made a professor before that or somehow recognized. Even when they finally recognized her, they still made it a half-time appointment, they still made it in conjunction with a research thing, what was it?

I: Sociology and/or the ICW.

L: The ICW, right. There is something very grudging about it, and I believe that gender is an important part of that. Something they wouldn't do to a man, and that a man wouldn't accept, and that Else—although she was very independent, she was like many women in those times—just took for granted: that she could not have what her husband had, and she felt bad about it. She may have tried it in indirect ways, to get more recognition or to get more position or whatever, but the possibilities were limited and she was really mainly fighting to do the work, not to get the position. But after the book, starting in probably 1948 or 1949, both Else and Nevitt were doing very well. At that point the study of personality was a big field in psychology. The idea of this kind of social psychology was very interesting. They were seen as leaders in that field. My part in that book helped me get to Harvard in 1950 as an assistant professor after being an assistant professor in Western Reserve for three years. Nevitt's part in the book got him to be a professor at Berkeley around 1950—then he left because of the loyalty oath.³¹ And it made Else a celebrity but she was still a research associate. Nevitt began being elected or appointed to various offices. He became the president of the division of personality, social psychology, this sort of thing. But he wouldn't sign the loyalty oath and left and spent a difficult year in England not knowing what he would be doing next.³² Then he began another study, which was an important new project and then went to Stanford as a professor, so he was getting a lot of return, much more than Else. At the same time there were people who saw Else as intellectually more creative, and she was opening up and she was looking more intensively at certain issues, like intolerance of ambiguity in personality. She might have had colleagues or assistants working with her but it was very much her project, and she was on the production line as well as managing. Nevitt talked about his project, much of which was the work of others Else talked about her work. In fact he was not as productive intellectually, so he was open to the charge that she was the more creative force. At the same time, he was doing more things and was more prominent and so she could feel that he was getting too much credit for things that she did. I think that working on the interviews in TAP was probably the most intellectually interesting and creative part of the study. I would say that, knowing that it's not my part. I think there's more new and creative theoretical *Prägnanz* there than in scale, which is more about showing how ideas can be tested. But the world continued to divide them, I

³¹ In 1949, the regents of the University of California voted to impose a special anti-Communist oath on all faculty, initiating controversy that lasted into 1952. Forcing the faculty to sign this oath was legitimized because the University of California system was a public institution, and its employees were therefore seen as public employees similar to the people employed by the federal government. Most private universities did not follow this course of action. Several well-known professors declined to sign the oath and resigned from their post as a consequence. Besides Edward C. Tolman and Erik Erikson, Nevitt Sanford was one of them (see Paier 1996 for details).

³² Else Frenkel-Brunswik did not join the group refusing to sign the loyalty oath. In a letter to Edward C. Tolman, she reasoned that her research was the main line of contribution to society, not political action: "I am thinking of myself as a woman of 'thought' rather than of 'action'. [...] You may say at this point that what is now going on is the first step to further controls and we have to stop it right here and this is certainly a valid argument. On the other hand, we have to recall that the faculty agreed upon the anti-communist policy by an overwhelming majority. And though I do not agree with this policy, I feel that this battle is lost." (Letter from Else Frenkel-Brunswik to Edward C. Tolman, July 31st, 1950, AGSÖ 25/01.108).

believe. As far as I know they had very little contact after the book came out and no collaborations. I believe the rivalry between them was accentuated after the book came out.

I: I never heard about them being in contact or in closer contact after publication of the book.

L: But the fight wasn't between the two of them. The fight was between the two denominations, the two religious denominations that built on their names, which to some extent each one wanted because they did feel somewhat underappreciated. That was reflected earlier when we talked about the authorship of the book. It was essentially an injustice that Teddie's name came first...

I: Was it just to have the names in alphabetical order?

L: Yes... But the reason we chose alphabetical order was that if it wasn't alphabetical, we would have to decide who is first, Sanford or Frenkel-Brunswik, and that was harder to deal with than being alphabetical. The assumption I think that both Else and Nevitt made was that alphabetical means that it's equal, that nobody is claiming to have been the primary source, that we can all go on to develop the things that interest us. I don't have to claim to have been the primary source but I can show from what I went on to do that I was an important figure in it. I don't think either of them expected that the conflict would become as great as it was. See, that was the first time either of them became famous. They were known before but to some extent this became a kind of heroic book in American psychology. I thought that without being involved in these struggles, and very often I was referred to it or people introduced me like that. And it started being called a monumental classic, and all these ridiculous terms. My own feeling now is that in absolute terms it's a pretty good book. In relative terms it's unusual because psychology didn't know then and still doesn't know how to do anything like it. I don't think there's an empirical study that's similar, even now, that has combined some degree of scientific quality and the intellectual substance. Partly this is because collaborative enterprises tend to fail. If I believed in miracles, I would say there is a kind of a miracle in this, that we actually kept going and sustained the work and got through the writing. Because the destructive forces around were so great, including the people who wanted to know who's the real mastermind, which is very destructive for collaboration.

They became famous, they became the famous authors of a book that represented the hopes of one major part of academic psychology, and psychology as part of society. We were fighting social prejudices and working for social good and all that stuff. And I don't think they were well prepared to become celebrities, I mean, that increases the rivalry.

I: So, in 1947 the empirical work for that study was finished.

L: And the funding was over.

I: You went to Cleveland, you mentioned before.

L: Yes, in 1947.

I: ... and became an associate professor in Harvard.

L: No, it's Western Reserve University where I went first, which is a good university, a good psychology department. It was very hard to get jobs and that way I was doing well. In 1950 I went to Harvard. I think that was just around the time the book came out and I think one of the reasons why Harvard was interested in us was that study.

I: Did you maintain contact with Else?

L: Yes, with both Else and Nevitt. Although not a lot, we didn't see each other a lot. I saw them at meetings in the early 1950s, well, between 1947 and 1950 when we were corresponding, during the writing. Most of the writing got done between 1947 and after I left. I had written my dissertation which provided the basis for a few of the chapters. You see, my dissertation was on ethnocentrism and it was an early form of that whole part one. And I had some other things in it that didn't go in the book. We ran a lot of correspondence around the writing. Each of us would send the drafts of our chapters to the others. There were two big decisions about our authorship, sort of mild forerunners... One was about the order of names, and going alphabetical was a way of avoiding conflict. I think one reason the three of us, and in particular Else and Nevitt, were willing to do alphabetical order was that Adorno wasn't so well known in psychology. We were thinking mainly in that context, so if people read that order they would still want to know which of the two of them was the one. It was only later when Adorno became more famous...

I: But he didn't become famous for empirical research here...

L: No, that's right.

I: It was never his style.

L: That's right. But there are still a lot of arguments about which one was the main one. But that was one issue and the other was that we decided to put names on chapters. We put all the names on the F-scale, because that seemed very collective, but the other chapters had either one or two names on them. In some way, that gave us some recognition for particular things. So we were in a lot of contact by correspondence. I don't think I went to Berkeley at that time but they would come East. It's very hard to say no to Harvard but I knew it would be a mixed blessing. If you go you may regret it, but if you don't go you'll regret it more. In the early 1950s, when Nevitt was my advisor, I went to Berkeley once or twice but he didn't ask me to be on the staff there. Nevitt went to study a college and I went to study a mental hospital, which was part of an attempt to become more sociological and still consider personality, things like that. I saw Else but not much, and I did see her after Egon died in 1955.

I: You hadn't been to Berkeley before that?

L: Well, I saw her in Berkeley after he died and I saw her at APA meetings along the way, so I had a sense of what was happening in her life, but not as much...

I: Well, I think she got a travel grant for Norway in 1950. I think that must have been after the book was published

L: Yes, right. Let me just think.

I: In 1954 she was at the Center of Advanced Study in the Behavioral Sciences, and that was an honor.

L: Yes, and if she could get that, she should have become a professor, too. But now, I saw her after Norway, in 1952 maybe, but I remember something of that talk that may be of interest to you. It was either right after or maybe a year after she got that, and she said that she was becoming less of a positivist. There were a lot of concepts that interested her that she wanted to study without having to measure quantitatively or to develop operations and definitions. I remember that, but also that I myself was moving that way, that's an interesting parallel. Her orientation was changing, she would

have been 43 to 44 then in 1952. I now understand that from my research. I believe that in a person's early forties, it's a time of important developmental change in adulthood, what I came to call midlife transition, and I think that she was going through an important transition. One result of it was her decision to look at the middle age ...

I: Yes, she started with that, but she didn't finish it ...

L: And it was difficult for her, I assume, for many reasons. I think that study reflected her own sense of becoming older, or becoming middle aged, and wanting to look at more complex issues, being less governed by quantification.

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Author biographies

Daniel J. Levinson (1920–1994) was an American psychologist, contributing to the fields of developmental and social psychology at the University of California in Berkeley, Harvard, and eventually Yale University. While a student at Berkeley, Levinson became part of the Berkeley Public Opinion Study Group and one of the authors of *The Authoritarian Personality*, with Else Frenkel-Brunswik, R. Nevitt Sanford, and Theodor W. Adorno. Of his later works, his book *The Seasons of a Man's Life* (1978) achieved great publicity.

Dietmar Paier, sociologist, conducted this interview with Daniel J. Levinson as part of his PhD thesis on the emergence of modern sociology in Austria and as the editor of the book *Else Frenkel-Brunswik (1996) Studien zur autoritären Persönlichkeit. Edited by Dietmar Paier (=Bibliothek sozialwissenschaftlicher Emigranten, Band 3)*. Today, Dietmar Paier is a higher education didactic expert, lecturer, systemic coach, and trainer in Vienna.