

**Kaja Kaźmierska**  
 Department of Sociology of Culture  
 Institute of Sociology  
 Faculty of Economics and Sociology  
 University of Lodz, Poland

## An Interview with Professor Fritz Schütze: Biography and Contribution to Interpretative Sociology<sup>1</sup>

DOI: <https://doi.org/10.18778/1733-8077.10.1.11>

The idea to conduct the interview with Professor Fritz Schütze had shown up during my conversation with Professor Krzysztof Konecki who asked me if I could do it, and I am grateful to him for this idea. Professor Schütze agreed to give the interview, and it was conducted in May 2013 when I had the great pleasure of being the guest of Professor Fritz Schütze and his wife Evi Schütze in their house in Wattenbach, Northern Hesse. Since we are both biographical researchers, we decided that the interview would have biographical background, and, as a result, it has a structure resembling an autobiographical-narrative interview. During the first session in July 2013, lasting approximately 1.5 hours, Professor Schütze recounted, in the frame of his biographical experiences, how he became a sociologist and how he developed the idea of autobiographical narrative interviewing. Then, on the next day, for more than three hours, we concentrated on the sto-

<sup>1</sup> I would like to thank Professor Fritz Schütze for his careful revising of the text, as well as Professor Marek Czyżewski for his proofreading and comments which helped to clear some contexts and meanings that could have been difficult to be understood by the reader.

ry he had told the day before. I asked for additional explanations and put some questions. Finally, in November 2013, we discussed some details, which I found important after revising the transcription. Then, I sent back the transcript to Professor Schütze, and he worked on it supplementing his story with more details.

Of course, the transcription of the interview presented to the reader is not a typical autobiographical narrative interview – neither in its presentation structure nor in the technical form of a scientific transcription with all its special markers. On the other hand, it resembles the narrative interview in which its author often stops at points of some biographical details or tells about some episodic events to illustrate the specific meaning of biographical experiences. I decided to keep most of such descriptions, pictures, anecdotes because, for me, as a listener, and, as a follower of Professor Schütze’s type of text analysis, too, they appear to be very inspiring illustrations of a biography as told in sociological perspective, or vice versa – the story of a sociologist as

told in the perspective of his biographical experiences. When working on the text (by doing a sort of an interviewer’s editing work), I also tried to keep the logic of the “main” or “first” story, that is, the story of his life, in terms of the chronology of remembered and re-evoked events, as well as in terms of the sequence of introduced topics. I marked this sequential order of recounted events and addressed topics by putting “subtitles” on top of the following parts.

As a result, it is not a typical sociological questionnaire interview in which the questions are followed by the answers. It is more the narrator’s composition of stories, in most cases not interrupted by myself; only sometimes I posed questions. I decided to mark the laughter of both of us since I find it important that the reader can feel the situational mood and the mutual relating to each other in order to understand the intentions and interpretive reactions of the speaker and the listener.

I would like to thank Professor Fritz Schütze for giving his time and sharing “his story” consisting not only of his biography but also referring to a rich part of (German) sociology.

\*\*\*

**Kaja Kaźmierska:** I would like to ask you to tell me how it happened that you became a sociologist and then stepped into biographical studies, and how you developed them as a sociologist.

**Fritz Schütze:** Let me ask you, would you like to have some biographical background or just focus on

how I worked in sociology and how I developed the autobiographical narrative interview?

**K.K.:** I think it is a very good idea to give some biographical background, please tell your story as you would like to.

### Family Background

**F.S.:** A story about me as a sociologist must be linked to my life history; so, I cannot fade the “before” out of my awareness and just start with the topic of becoming a sociologist. When I was a little child, I would live with my mother who was a teacher, and she and I were living in Bielefeld together with my grandparents. My father became a prisoner of war in Wroclaw. I guess it was the very end of the war. He was captured by Russian troops, maybe on the 5<sup>th</sup> of May 1945, or something like that, because Wroclaw, that was called *Festung Breslau*, was kept by the Germans almost up to the very end. Then, my father would be brought to Russia as a prisoner of war. And he always said that the Russians did not have enough food for themselves to eat, too, and the prisoners of war were not treated differently compared to ordinary Russians. Instead, they were treated in a fair way, but there was very little to eat. So, he got the chronic disease of *nephritis* – a very serious kidney disease.

And I can remember that my mother took me to the train station in Bielefeld to meet him when he was coming back from Russia. I do not know in what place in West Germany these men returning from the Russian prisoner of war camps had been collected, that I cannot remember. At the train station

in Bielefeld I saw a strange man, and it was quite difficult for me to get into an intimate relationship with him as I had with my mother. There were some situations in which we would like each other, for example, when I was interested in doing my own metal constructions with some type of a children's construction set. He was a natural scientist, and he liked that I had some leaning to it. But, generally speaking, at least I felt it this way, I was a disappointment to him because his generation would have expected to have children who would be strong boys, you know this type of a *fit* boy. He was not educated as a Nazi while being an adolescent and a young man, but nevertheless he was affected by the mood of the times, as I would surmise most of the young Germans in Nazi Germany would have been. And, when he had been in the prisoner of war camp, he had imagined that he would have a strong boy at home, and that his son would be a healthy boy. But, the boy he finally encountered in Germany was not healthy. Every month I would have an angina with high fever, and so on, and it was difficult.

Then, when I was 10 years old, I got, maybe connected to these anginas, it is not clear, *osteomyelitis* – a severe bone marrow infection. I had this for 8 years and therefore, I spent my second decade of life in hospital. Last week, I had to sit in a committee for a sociological PhD dissertation in Duisburg University, and enigmatically I felt very happy in this building totally unknown to me, and only later I realized that it was an old hospital building. So each time when I go to hospital, I immediately feel at home because after a short while, I know everything about it because I spent some part of my life in hospital – more than 5 years.

Of course, it was not that nice for my father that he would now have a really sick boy. But, sometimes the interactions with him would be quite nice, I have to admit. I have to underline that, although he was a natural scientist, a chemist, geologist, and geographer, he was very much interested in history, and I learned about crimes of the Germans in Nazi times from him very early. He would not fade it out; he would tell me, for example, about what happened in Wroclaw/Breslau, and what harm the Germans would cause to the Russians. He was not a professional soldier; during the war he had been a student, and then, he was put into the army, and in between soldier-service periods, he could study a little bit. At the very end of the war, he became a leader of one of the army companies in Wroclaw, but he was not a professional soldier. Yet, he had lots of this what we can call the “Prussian stern face,” and so on, and he had a voice which was extremely loud, and instead, I cannot speak with a loud voice at all.

So, he came back from the Russian POW camp in 1948; he was there for four and a half years. And when he came back, he was very sick. After some months he recovered some bit from the kidney disease, and then he had to go on with his university study in Münster in Westphalia. [During those days there did not exist a university in Bielefeld.] I can remember *his* study in Münster, where we had moved to, and not so much my own. He was a chemist, like your husband, and I can remember all tests he had to write. In terms of his body, he was much older than his official age was. To study was some bit difficult for him, I can remember all of this. Since he wanted to be-

come a school teacher of chemistry and geography, he had to study philosophy, too. Therefore, we would have these regular philosophy meetings at home in our small apartment in order that he would prepare together with other “older” students, having been soldiers, for a quite disliked philosophy examination. I can remember a very nice lady as the philosophy teacher of this bunch of “mature” students by the name of Dr. Annelies Ludat-Deniselle (probably one of the first female doctors of philosophy of Humboldt University Berlin). She would teach these “older guys,” like my father, and prepare them for the examination, and I myself was always present and very interested, and learned a lot of philosophy when I was 8 years old (laughter). Later, Dr. Ludat became a very good friend of my wife Evi and I.

**K.K.:** (laughter)

**F.S.:** My father could manage to become a school teacher, which was called *Studienrat* during those days. He was *Studienrat* in a gymnasium for girls. And he loved that, he was a good teacher – maybe much better than a teacher he would be for boys and young men. You know, my relationship, as son to my father, was typical; it is just one piece or example out of the father-son relationship of millions of people in post-war Germany – of boys of my age who would remember their strange fathers. That is the reason that I dwell on this so much. The manners of these fathers had been developed in Nazi times, or even before, in the times of the Weimar Republic, as some sort of *post-Prussian* period with all this press on the “strength of manhood.” I hated it a lot, and lots of other young German boys hated

it, too. Those young boys, separated for years from their almost unknown fathers, would have developed a very strong and trusting relationship with their mothers, and, of course, it was for these fathers, as husbands, very difficult when they saw all of that. And, in addition, they would have pictures in their mind how their boys should look, and in reality, they were very different.

I think that my father was much more positively affected towards me than I could realize; for example, to other people he talked nicely about me, but at home, it was sometimes not that easy. However, there was one thing which was really nice: he always had a job of “the main teacher of the class” (*Klassenlehrer*), and this job was especially important when the class had to be prepared for the *matura* examination.<sup>2</sup> In former days, in order to get a *matura* (*Abitur*) in Germany, each school student had to write an essay about her or his personal development and what she/he wanted to do after *matura*, and the main teacher of the class had to write a quite meticulous assessment, too. I can remember, such a teacher's assessment would be about three pages for each student. As a natural scientist (laughter), it was not that easy for my father to write it. So, he used to call me to his working room. I, then, was 15 years old or so, and he would tell me about a young lady who was 3 years older than me, he presented her to me in terms of achievement, personal difficulties, and character, and I had to write the assessment down. (laughter)

<sup>2</sup> F.S. used the (Austrian) term *matura* as translation of the German term *Abitur*. The *Abitur* is the secondary school leaving examination that hands out the license to enter university.

**K.K.:** (laughter)

**F.S.:** In cases like this I was quite worthwhile for him, but otherwise, from time to time, our relationship would be much tenser and awkward. For example, he had hesitated to let me get a library card for the city library because he thought I would read too much belletristic literature and philosophy, and so on, and I should better study physics and chemistry. (laughter)

And later on, he had two other children with my mother. But, then, when I was 15 years old, my mother died, and later on, when I was about 20 years old, he got married again with a nice and very Catholic lady. [He was Catholic, and I and my mother was Protestant.] The father of the second wife of my father was one of the central guys of the Catholic Centrum party in Weimar times, and later on, in post-war times, of the Christian Democratic Party (CDU). Then, I got a second sister, a half-sister who is 20 years younger than me. However, not after a long time, the kidney disease of my father came back. This recurrence probably was the result of a mistake of medical doctors because he always took some liquid to reduce the swelling-up of the mucous membrane of his nose, and this was a poison to his kidney, and they should have known this. I had not known it, but later I studied it and realized what had happened. It took just two months that he would die.

Anyway, he could still come across Evi, my future wife, and he liked Evi very much. After his first encounters with Evi I became a quite remarkable young man for him because I had this nice girl-

friend, and later wife. (laughter) I guess he essentially started to change his image of me when he met Evi. I brought her home when I was 18 years old. And, something like that happened (and I love this): he would ask her: "Would you really like to get into contact with this guy? He is difficult, think twice about it." (laughing)

**K.K.:** (laughing)

**F.S.:** Anyway, he was quite impressed. Evi looked nice, she had a good outfit, and she cooked well, and he liked all of this, obviously. He had not expected that I would have such a nice girlfriend, so he was really impressed by that. (laughing)

**K.K.:** (laughing)

**F.S.:** And since then, our father-son relationship got better and better.

**K.K.:** Could he see his granddaughters then?

**F.S.:** Yes, he even could see my first daughter Irene. My father died when I was 26 years old. Although I was very sad, his death was not an extremely severe problem for me, *in terms of my own personal identity development*, since I was rather settled already. Instead, my mother died when I was 15 years old, and this was extremely difficult for me. I had a very, very deep relation with my mother, and it was awfully difficult for her when I got that seriously sick. At the beginning of this disease, my parents might have thought that I would be in danger of dying. [But, I myself never thought I could die.] After half a year, my parents took me out of

the university hospital, where I had not gotten the appropriate medical treatment and nursing care, to a special hospital in the countryside. So my mother had to travel there every week or every second week by train, since we did not have a car during those days, and all of this was very difficult for her.

My mother was a school teacher for all of the classes of the West German standard public school, which was called *Volksschule* (people's school), it was not a gymnasium. Every child would attend the lower level of this school up to age ten, but children from non-academic milieus would usually go there until the age of 14 finishing class 8. My mother was very much interested in literature. She had originally studied medicine, but she could not stand it if people would die. She had very successfully accomplished what is called the *physicum* – this half-way examination on the natural science stuff for medical students, and then she stepped out of studying medicine. She also could not stand ether, and in former days nothing else was available for administering a narcosis. That is the same with me, if I smell a little bit of ether, I get sick immediately. [I had lots of operations because of my bone marrow infection, and I was almost "dying" each time from my sickness caused by ether. Today, it is so different with all of these beautiful narcosis devises, this sophisticated anesthetics stuff.] So my mother had to step out of her study course of medicine, and then she became a school teacher, like my aunt – her wonderful sister, 2 years older than her, who became later, after the death of my mother, something like my "second mother," she was married to a countryside doctor. My aunt first became a gymnasium English teacher. She was one of the

few who went to England in the 30s. During those days it was some bit extraordinary that you would go to England; it was almost like a sensation. And later on, she became the helper of her husband in their medical practice. So, she did not continue her profession, but my mother did, and that was extremely important for her.

### Where the First Inspirations Came from...

**F.S.:** In hospital, in the first year of hospitalization, I was 10 years old, but nevertheless I realized that the medical doctors would make mistakes, quite a lot of mistakes. I remember that the head doctor with his entourage, maybe 10 people, would stand around my bed and would keep speaking with these Latin phrases, and I did not know what it meant, but I did realize that they had done and continued to do wrong things. My infected bone broke in the inappropriate plaster cast, and they did not believe me that exactly this had happened, and they started to laugh at me. But finally, when they took the cast off, they realized that the bone had actually been broken and had in-between grown together in a wrong way. So, they had to break the bone again and fit it together in a more adequate way. So, I felt lots of mistrust towards this whole crew of medical practitioners. On the other hand, this established my peculiar relationship of "special interest" to the medical profession and to the professions in general. I was always interested in professions since those days. Of course, I cannot say that I got the idea to study sociology right then – being just 10 years old. (smiling)

**K.K.:** (smiling)

**F.S.:** But, this is where my interest for the professions somehow came from. And, there was the interest for the Latin language since all the time, looking at the sick parts of my body, the medical doctors would use it as some sort of secret argot. The Latin language had another very important specific function for me: since I was only for short visits at school, and had to go back to hospital all the time, Latin became “place keeper” for me because you learn it at home in bed or in hospital, and you could become quite good at it, although you had not attended school for a while. And this would rise and keep up my status at school. Therefore, in terms of respect from teachers and co-students addressed to me, Latin was quite important, although maybe some bit it has also damaged my brain because the Latin language has all these complicated syntactical structures, which the German language is fond of, too, as well as the Polish language, as far as I know. So, it is very difficult for me to write in short, simple sentences; this might be caused by this inhaling of the Latin language as a child.

Throughout my whole career in the gymnasium school was not really a big problem for me because the teachers were very nice, and my school friends were very understanding and helpful, too. Of course, there were a few difficult situations because during the first years I was something like a cripple, although I did not see myself as a cripple. In the beginning, I could barely walk, and there were some boys who would beat me up or try to do this. However, I would defend myself to a certain degree. I could not beat them in terms of body power. But, for them those fights turned out that they would get into personal difficulties since they would be

despised by other school students, and this soon caused them to abandon such a practice to use bodily power in order to shape their relationship with me. So it was a really nice school situation. I could come to school whenever I wanted; if I wanted to come at 10 o'clock, it was okay, but I could also come at 12 o'clock.

The only really bad learning subject I had at school was the English language. Whenever I opened my mouth, everybody started to laugh. But, my school fellows did not react that way because they would like to ridicule me; instead, my pronunciation was that awful, that they could not help themselves but to laugh. The reason for my remarkable achievements in English was that I had never *heard* this language. You know, in those days, the English language was not around very much, you did not hear it, and so it was the only time that I would have a “five” as a teacher’s assessment, a *fünf* in the German language, what meant that you could not pass if you would have two “fives” at the end of the school year. I received this really bad, but correct marking of my English teacher just one time as an in-between half-year assessment when I was 15 years old, and then, the mentioned philosophy teacher, Dr. Annelies Ludat, would successfully help me to improve my English competence. Nevertheless, I never dreamed that I would be able some day to speak and use the English language quite easily. I never thought that this would be possible some later day.

Because I spent 5 years in hospital, I needed to have something to do there. Of course, I was happy when I had long novels and history books to read. And I cannot exactly remember how it occurred that

somehow I came across the long and short novels of Dostoyevsky, but – thinking back – this encounter and preoccupation with Dostoyevsky in hospital I would really like to connect some bit with my then upcoming idea to study sociology. Although I did not know what sociology really was, under the headline “sociology” I wanted to study something like what Dostoyevsky had described and wrote about. Maybe I came to him through Boris Pasternak’s *Doctor Zhivago*. I cannot remember that I would read *Doctor Zhivago*, but I listened to it: it was read on the radio when he had gotten the Nobel Prize. I had to develop a technical device of making sure that my parents could not hear the radio when they would come into my room: in the very moment my parents would open the door, the radio had to go off (laughing) in order that they would not realize that I was listening to the broadcast of Pasternak’s novel all the time. Anyway, I read all the novels of Dostoyevsky in hospital, except *The Adolescent*. I did not come across *The Adolescent*, the only novel formed as a clear-cut fictitious autobiography of the 5 grand novels of Dostoyevsky (which later became important for me in the context of analysis of the textual structure of autobiographies); I did not know why, but I read all the other Dostoyevsky’s novels, the grand and the small ones, in hospital. And out of this, I am quite sure, the idea emerged that I would like to study something like sociology, and I knew then already that it could not be psychology because there seemed to be no social dimension in it. Therefore, I had a certain idea of sociology, although probably I did not even know the name of this social science discipline in the beginning. As I have mentioned, in order to apply for the *matura* in those days in Germany, you had to write an essay, something

like a few pages of an autobiographical statement, and at the very end of such a statement you had to make plausible what you would like to study. So, I produced this statement, and I can remember that I used the term sociology. And everybody would ask me what the hell sociology would be. We are talking about the year 1963/64, yeah? And nobody would know what it is, and I had to explain it to everybody, although I would not know it either. (laughter)

**K.K.:** (laughter)

**F.S.:** So, I did this. The other possibility would have been to become a physicist or to become a physician. To become a physician Evi did not allow because she said I would commit suicide when my first patient would die; so she would not allow me to choose medicine as a study subject, although I would be quite okay as a medical doctor I would say (laughter). I was very much interested in physics, and in school there came into existence what would later be called “special achievement courses.” You had to put some extra effort, special effort into such a chosen subject for advanced school study. And I chose physics, maybe a little bit in order to construct some sort of accommodation between the interests of my father and my mother regarding my future academic development. My mother was very much interested that I would study classical languages and literature, and she would provide all the interesting books for me. It was a big problem for her to get all the children’s books, and later on, other literature for her son in hospital who would read a new book every second or third day. And she would write to me a letter

everyday, and I would write back to her every day. And later on, when I had been able to leave the hospital for the first time, I always had to visit the school which would be the nearest because I could not walk, and finally, my mother managed to get a flat near one of the two classical gymnasiums in Münster teaching Latin and Greek. So, I had to go there and my father did not like it. So maybe in order to establish some sort of a compromise, I took the physics subject as a special achievement subject of mine, but even “from my heart” I was very interested, and still I am interested, in natural sciences. So, I am a bit of an odd type of sociologist, and even today I keep believing that the way I would analyze ongoing interaction, or even a piece of narrative interview, is not that different from the method Newton would use when watching out for the falling apple. Taking physics was not something which had been pressed on me; I had chosen it not only because I wanted to get some acceptance of my father, but in me was some original interest for it, too. And my father liked this, although he was a chemist; he liked that I did at least physics. And this was quite a remarkable pick of mine since we had an outstanding teacher of physics who was also brilliant at Latin and Greek. When we went into classical mechanics, we would read Newton’s original Latin publication *Philosophia naturalis principia mathematica* (the latter two words serving as an *ablativus absolutus*). In class, we would read Newton’s main book in the original Latin version, and in-between he would construct with us all the experiments for studying the phenomena Newton had written about. Even up to now, I know the Latin terms for all the phenomena Newton was writing about. And we had

this very remarkable teacher in mathematics, too, and for me, it was something like enlightenment when we studied *calculus* without using numbers; so, we had to solve classical *calculus* questions without using numbers, instead, just thinking and writing an essay would be allowed. This was something extremely interesting for me, and, maybe looking at background constructions,<sup>3</sup> is a quite comparable “structural observation.” I loved this type of mathematics very much.

### Learning to Do Liaison Work in Protestant and Catholic Milieu

**F.S.:** Before in the interview I was thinking if I should put this topic in my story because this relation is totally unproblematic for me today. And then I thought this is something quite important. My father was Catholic and my mother Protestant, and in my generation the difference in type of folklore sentiment connected to Catholicism and Protestantism was felt very much, although maybe in theology there was not a big difference. In folklore terms, there was quite a lot of differences, for example, I can remember when I would go from our family home to the *Martin-Luther-Schule* and vice versa, from there back to our family – to and from the *Martin-Luther* school, where, by the way, my mother used to be a teacher, I had to circumvent in a hidden way the catholic *Uppenkamp-Schule*, situated half way in-between the 1 hour distance from my home to my school, in order not to get beaten up by the Catholic school boys attending that school.

<sup>3</sup> Background construction is a feature of spontaneous narratives described by Fritz Schütze as a central symptomatic textual indicator of extempore narratives. See, e.g., Schütze (1987; 1992a; 1992b; 2008a; 2008b).

**K.K.:** How did they know that you are a Protestant?

**F.S.:** They knew this because I would not come out of their own school; instead, I would come from another school, and they knew that two kilometers away there was this protestant *Martin-Luther-Schule*. Because there was too little room in that school, my mother taught in the morning, and I attended school in the afternoon and vice versa. So, I was what was called “a key child,” a child who would have the apartment key around her or his neck. My father was at the Chemistry Institute of Münster University, and so I was on my own, and I had to be very careful how to pass that Catholic school (laughter). During those days, different groups of children just forming Protestant and Catholic groups had beaten each other up, which is something odd, really odd; but, as far as I know, felicitously, this does not happen anymore today. During the last 60 years, the relationship between Catholics and Protestants luckily became much better. Of course in Poland, the potential for such a cleavage and conflict relationship is much lower since you have just these 2% Protestants within the overall population, but in Germany it always was, and still is, a 50-50 ratio. Something similar happened in other parts of Germany; in the narrative interviews I collected later are a lot of stories there about similar search and catch episodes (in many of them Catholic children are chased by Protestant ones). The folklore conflict between the two confessions was the reason that I did not like so much to live in Münster because Münster was a Catholic city and Bielefeld a Protestant city. I felt very much that way; I even suffered some bit from this conflict, and so I always looked very much

forward that I could go on vacation to my grandparents in Bielefeld. Later on, I could even impress Anselm Strauss with it. When he was in Germany, my friend, Gerhard Riemann, and I took him to the countryside of North Rhine-Westphalia, and we always followed up the division line between the two religious confessions. On this trip, crossing the religious division line several times, I used to say: “Look, when the cows will be red and white, it will be Catholic country. But, when the cows will become black and white, then we will enter the Protestant part of the country.” We crossed the religious line several times, and each time we would go into the churches to see if they were Catholic or Protestant, and it really worked. (laughter)

**K.K.:** (laughter)

**F.S.:** And Anselm, quasi-seriously asked himself what would be the sociological rule for this coincidence between the colors of the cows and the religious confessions. (laughter) I know that in his teaching in San Francisco he sometimes joked about statistical correlations using this very enigmatic phenomenon as example. Nowadays, the cows are from different breeds coming from all parts of Europe, and this, so beautiful, “sociological rule” does not work anymore, but in those days, it was that way. And what was some bit difficult for me was that my father had lots of sisters who all were very nice, but one of them, a very nice lady, was extremely Catholic, and so I knew from her that my father would be in danger to go to the purgatory because I, as his child, would be Protestant and not Catholic.

**K.K.:** (laughter)

**F.S.:** And this thinking, that I would cause harm to my father just through my religious adherence, was some bit difficult for me. On the other hand, and it's the same with Evi, we both learned very much about catholic culture, too, and we both could have passed to be Catholics perfectly in terms of church behavior and religious folklore. And sometimes we need to enter catholic surroundings because we love it very much, but we had to learn to love it.

**K.K.:** How did you cope with this Catholic and Protestant upbringing; how did it happen that you managed to divide it and join at the same time?

**F.S.:** In a certain sense I love that there are different brands of Christianity because all of them have something peculiar and specifically interesting. The confessions can discuss controversial topics, and sometimes they even quarrel with each other, too; it is very lively overall social world of Christian religion with interesting arenas of theological discourse. For example, there were intensive discussions between Protestant theologians and Catholic ones regarding the question of how much Luther would have been a heretic; the result was that his theology would have been "orthodox" or even "good Catholic"; the discussions came to the point that the principles of Catholic and Lutheran theology would be basically the same. As an adolescent, I was considering several times to convert (from the Protestant to the Catholic confession), but then I always realized I had this religious tradition of my mother and therefore, I did not want to do this. In my perspective, there are mainly folklore things which are different between the two confessions, and as a child you feel

a bit cut through because there are these very different folklore habits. But, you have to take into account the development of two (or even more) traditions of something like 450 years of religious folklore development, and as a child, I felt this very much, and I suffered a lot by this religious divide. On the other hand, when I was a young man, and did my university studies (and even before, in school), I got into an intellectual relationship with all this new theological research of Protestants, like Bultmann, for example, and of Catholics, like Karl Rahner, for example, and of people like, and then I realized that it is some sort of competence of mine that I understand very much the Catholic perspective and I started to accept this; and later, I even started to like that.

So, I am the only Protestant in my family of origin left over, my brother and my sister, and my very much younger half-sister are catholic. I am not sure how much this religious divide is powerful today, I really do not know it. Obviously, we can see: Angela Merkel is the head of the Christian Democratic Party which by tradition is much more Catholic than Protestant, but she is the daughter of a Protestant minister, and our president, Joachim Gauck, was a Protestant minister. So, maybe the former bellicose contest between the Catholic and Protestant confession is gone today. In addition, of course, we have to take into account that in East Germany only 21% of the whole population belongs to a Christian church. The GDR government was very effective in erasing the religious roots of the East German culture – totally contrary to the Polish situation. But, exactly in the German areas of reformation things like that happened. If you

look more closely, you will find that even in the 19<sup>th</sup> century there was lots of stepping out of the Protestant church in the East German areas of Reformation times (in Sachsen-Anhalt, Thuringia, and Saxony), but at the beginning of GDR times the affiliation to the Protestant church still was 70-75%, and at the end of GDR it had gone down to 21%. These are the real divides – and not between the Protestants and the Catholics of today. I really love the different religious cultures of Germany and Europe, and I like to play with them some bit. Of course, there were a lot of ecumenical things going on in Evi's and my life: in later years, when we already lived in the village of Wattenbach in the surroundings of Kassel, we organized an ecumenical Santiago de Compostela pilgrimage<sup>4</sup> in five stages; we did this together with Catholic people. It was basically Evi, me, and 2 other persons who organized this. There was some disappointment that all these attempts to understand each other stemming from the *Vaticanum Secundum* had withered away during the last 30 years after the "renaissance times" of the second Vatican Council, and therefore, we started these ecumenical travels. But now, I hope, this cordial interest for each other comes back again. However, I do not believe in the unification of the various Christian confessions into one common Church; instead, I think that this religious pluralism is very nice. But, the social and religious relationships, including the folklore relationship between the two confessions could be much better, and life could be much easier for children of mixed religious families.

<sup>4</sup> In Santiago de Compostela the sanctuary of St. James is situated. It is the destination of the Way of St. James, one of the most important Catholic pilgrimage routes that originated in the 9<sup>th</sup> century. The route was declared the first European Cultural Route by the Council of Europe in October 1987.

Having been positioned between two confessions in my family of origin, this overall experience taught me to search for compromise. Looking back from today, I like this inter-confessional experience, although it was sometimes quite difficult, but I had to do it in my family. And in more generalized terms, when there was the students' revolt of 1968 at Münster University, I became one of the specialists for the liaison work between the protesting student groups and the professors. For example, when quite radical students had locked up, or "imprisoned," the professors of law in the faculty room in the basement of the law building, and they could not get out for several hours, there was an uproar amongst these very powerful law professors, and they wanted to take the students to criminal court. The student union asked me to approach the most "dangerous" of these law professors and prod him to hold back his legal accusation against a long list of students he had announced to take to criminal court. I personally had to go to his quite impressive private house and had to do the negotiations with him, which luckily turned out to be successful. I have got lots of stories how I did this type of liaison work. Even today I sometimes pursue this, and people afflicted and affected by serious quarrels sometimes even do not realize that I try to do it. I like to be in situations of taking different perspectives. During those days of my studies in Münster of course I did not know that the topic of "liaison work"<sup>5</sup> would be an important topic of the Chicago tradition sociology.

<sup>5</sup> See: Hughes (1972:296-309, especially 303f, 306-309). See also in Fritz Schütze's research the application of this concept to the cooperative activities of transnational civil society workers in Europe, e.g., Fritz Schütze and the German team of the EuroIdentities research project (Lena Inowlocki, Ulrike Nagel, Gerhard Riemann, Anja Schröder-Wildhagen, and Bärbel Treichel [2012]).

## Taking up Studies – Towards Sociology

**F.S.:** My parents moved to Münster, which is 80 kilometers of distance from Bielefeld, when my father started – or recommenced – his studies after he had recovered from his kidney disease he had caught as a prisoner of war. [During those days there was no university in Bielefeld. North Rhine-Westphalia, the largest land of West Germany, had two universities – one was in Münster and the other in Bonn.] I had stayed with my grandparents, whom I liked very much, some months longer in Bielefeld. But a little bit later, I came to Münster, too, when I was 6 or 7 years old, since I had to start elementary school. My parents stayed in Münster all the time and therefore, when I was 20 years old, I attended the University of Münster. The University of Münster was one of the few universities teaching sociology to an outstanding extent during those days. The others were Köln and Frankfurt, and in addition, to a certain degree the University of Hamburg and the Free University of Berlin did that, too. And in Münster, the founding father was Helmut Schelsky who wrote about the skeptical generation. He was a really good sociologist, though for the first time he had become a professor of sociology in Strasburg 1943 at the near end of the war (a position he could not realize), and, of course, he must have had some good connections or, at least, an accommodation relationship to the Nazis, otherwise he would not have gotten this professorship in Strasburg. But, you could not feel anything of those possible former concessions to Nazi influence in his teachings during the 60s. During those days Schelsky was not rightwing in his thinking, he was very liber-

al, very skeptical, so this was quite a lively study situation.

However, when I came to the university on my first study day, I went to one of the seminars of the younger sociology docents (not to Schelsky!) and I thought, “This is totally unscientific!” and I thought, “This is not a science at all!” (laughter) So I went to the student inscription office of the university and changed to physics as my main subject

**K.K.:** (laughter)

**F.S.:** because I felt that this subject “sociology,” as I had listened to it in the morning, was not a science at all. And Evi said, “Just find out in the first semester”; so I did physics as my main subject and sociology as my second in the first semester. Practically, I studied both subjects in the first semester, but officially, I was just a student of physics in the school of natural sciences. However, in physics we had to measure and calculate the tension strength of metal springs, and for me, that was quite difficult to measure and to calculate. It took lots of time, and through this awkward experience I realized that it would not be that interesting for me to spend my lifetime doing things like technical mechanics. So, for the second semester, I changed back to sociology as my main subject, and this was again connected with a change to that other school or faculty.

Now I have to just mention that as an important second subject I studied general linguistics with special impact on sinology. So, I got used to the literature and the language structure of the classical

Chinese of the 4<sup>th</sup>/5<sup>th</sup> century B.C. I studied sinology for 5 years and got quite deep into the classical language and literature, like the writings of the philosophers Confucius, Mencius, and Hsün-tzu (Xunzi). So I have gotten quite a sense for very different language structures and very different cultural worlds. [But I was never really good at the classical language.] And then, of course, there were all these new developments in linguistics; for me, especially important was not only Noam Chomsky but also Kenneth Pike. The latter was especially important for the development of my thinking. I will show you this book: *Language in Relation to a Unified Theory of the Structure of Human Behaviour*. I guess Kenneth Pike is the guy who became the long standing director of the Sommer Institute of Linguistics, this American Christian institution which would study the Indian languages and would develop the grammar of Indian languages in order to translate the Bible in these languages to proselytize all types of Indian tribes (and later other “natives,” too). We know that the work of this institute was quite criticized in terms of endangering native cultures, in terms of enforced assimilation to Western life styles, and in terms of capitalistic neocolonialism. But, Pike’s approach, with its peculiar connection with a formal linguistic perspective and an elementary ethnographic perspective, as well as his peculiar connection between an “outer” natural science type, behavioral observation perspective, and an interpretive-understanding perspective, was very important for me. He was the scholar who introduced the difference between the “etic” and the “emic” perspective of approaching language (and other socio-cultural) phenomena; he had derived this terminological differentiation from the difference between phonetics and phonemics.

In Münster, there was another professor of sociology, Dieter Claessens, who was a very interesting professor, too. Some bit he was in the shadow of Schelsky, although he was quite important in terms of socialization and family research. He was the scholar who brought Norbert Elias to Germany. In my third semester, in summer 1965, as I can remember, he brought a nice old man to our seminar course on philosophical anthropology (writers like Scheler, Dewey, Plessner, Viktor von Weizsäcker, Buytendijk, Gehlen) and said, “This is a man who did very interesting things before, during and after the war. He published his books in exile outside Germany.” Of course, nobody of us knew *On the Process of Civilization* or his book about the Court of Louis XIV (*The Court Society*). All these books were published outside Germany and out of print by then. Nobody, except Claessens and his younger co-workers, would know about these books, and Elias did not say one word about his so eminent achievements in the seminar, or more personally to us as students. And even today I still feel guilty that I did not carefully ask him about his work, when the Nazis had chased him out. During these days I even did not know that before Nazi times he had been a scientific assistant to Karl Mannheim. I did not imagine that later on he would become one of my most cherished classical sociology writers I loved to talk to my students about and worked on his analysis of the proceedings and interactions at the French court. I did not imagine that his concept of figuration I would later envision as most important for basic theoretical thinking in sociology. The only excuse is that I was still very young and not versed at all in sociology. Nevertheless, he invited me for a meal in a nice restaurant,

although I was just a student of the third semester, and he would talk to me, as I can lively remember, about contraception in Roman times. By then he was studying it, and I asked myself, “Why is he dwelling so much in our conversation on contraception in Roman times?” Later on I thought, he wanted to advise me that you need to be careful as a young student not to have a child too early (laughter) because he knew that I was married already (laughter). Perhaps, he thought I should take 1 or 2 years longer before I would have a child. Nevertheless, since Evi is older than me, we decided we should have a child soon: our daughter Irene. (laughter) Yeah, so this was my first encounter with this so lovable man and eminent sociologist, Elias. During those days it was unimaginable for me that later on this man would become so eminently important in German sociology again. He had been totally forgotten by the impact of the Nazi demon and the intellectual isolation of Germany during Nazi times and its parochial outlook to the world afterwards. Later, Elias was in the Bielefeld ZiF (Centre for Interdisciplinary Studies) for a long time; but then, I did not dare to visit this eminent and prominent scholar again.

### First Steps in Sociology

**F.S.:** As I told you, Helmut Schelsky was a very important professor of sociology in Münster, and he had lots of habilitation “children,” and one of them was Joachim Matthes, another was Niklas Luhmann, and numerous other German sociologists very well known today were habilitated by Schelsky. Helmut Schelsky could think basic-theoretically; he wrote a book about defining the posi-

tion of sociology,<sup>6</sup> which is even today worthwhile to look at again. It is oriented to a certain degree by classical German philosophy, but, on the other hand, it is informed by all the important empirical studies practically conducted by Schelsky in the 50s, and caused by the “conflux” of both intellectual sources, there is therefore a lot of basic theoretical and basic methodological thinking in it. On the other hand, Schelsky was very much interested in the application of sociology, too, and through this interest he became a central educational planner of the government of North Rhine – Westphalia. He planned and organized the establishment of the University of Bochum, and later on of the University of Bielefeld. Conjoining with the latter, the whole institute of sociology of Münster University, with two exceptions, was moved to Bielefeld.

The Faculty of Sociology in Bielefeld is the offspring of the Münster Institute of Sociology. And Schelsky understandably had expected that all the professors he had helped to come into existence by habilitation and/or by call to Bielefeld, after their establishment as full professors in Bielefeld, would not read his wishes from his lips anymore. They would follow up their own smaller or broader interests and therefore, having been “grown up,” they would not have that former respect to him as their academic father anymore. We, as scientific assistants of his “academic sons,” being in this sense his academic “grandchildren,” would naturally understand him some bit better, and we would have liked to keep him in Bielefeld. However, he did something that had never happened in the German academic world before: he took his pro-

<sup>6</sup> See: Schelsky (1959).

fessorial chair under his arm and brought it back to Münster University. After his return to Münster, he became quite conservative, he allegedly also started to drink and he allegedly could not handle it well; this is a very sad story. Immediately before his leaving from Bielefeld, I was one of the 3 speakers of the scientific assistants, and I tried to help as some sort of liaison worker. We, younger people, tried to do much for him to keep him in Bielefeld, but, of course, he should have gotten relevant words from his academic “sons” and not from his academic “grandchildren,” but, that did not happen, and so he left for Münster. We, as scientific assistants, were very sorry about it.

And 4 years before the start of the University of Bielefeld, still being a quite young student, I came across Professor Joachim Matthes, who later would become my “doctor father.” He invited me to work for him at the Institute for Social Research Dortmund at the University of Münster.<sup>7</sup> Dortmund is a big city in the highly industrialized “Ruhr Area” (by the way, with lots of former Polish inhabitants stemming from the migration wave before the World War as, for example, mentioned and even some bit documented in Thomas and Znaniecki’s volume *The Polish Peasants in Europe and America*) roughly 60 kilometers south of Münster. I had to work on the religious confessions, the sociology of churches, and on the theory and research in sociology of religion in general. Generally speaking, I had to sift through the books in these fields of study in order to help Joachim Matthes some bit to prepare

<sup>7</sup> Sozialforschungsstelle Dortmund an der Universität Münster (“Außenstelle des Instituts für Wirtschafts- und Sozialwissenschaften der Universität Münster”): An Institute of Social Research, which became very sociological after Schelsky had become the director of it in 1960.

his teaching and publication activities in the fields of sociology of religion. Joachim Matthes, although still quite young, by then was known as one of the most important sociologist of religion in Germany. When Schelsky had established the Faculty of Sociology in Bielefeld University in 1970, Joachim Matthes got a call as Full Professor of general sociology and as Professor of sociology of knowledge and religion in Bielefeld, and he asked me to accompany him to Bielefeld as one of his scientific employees and (later, after my PhD) as scientific assistant. So, I started to work in Bielefeld in 1970, and moved with my little family (with my wife Evi and our two daughters by then) to Bielefeld.

Two years later, I did my PhD in Münster, and for this I had to travel to Münster again. You know, I was in the position of a research employee, although I had not finished my studies formally. (laughter) As I said, Joachim Matthes was doing research in the fields of sociology of religion and I helped him some bit to publish his books in this general fields – partly by reading and extracting a lot of books and empirical stuff on religious practices and the churches. It was very generous of Matthes that I could also read all the famous studies of the classical social science writers from Baron Paul Thiry d’Holbach over Ludwig Feuerbach and the young Marx, over Durkheim and Max Weber up to the, by then, most recent studies in the U. S. and in Sweden; you have to take into account that *I got paid* for all of my reading work. But, later on, it proved to be some bit too much for me to study all these religious topics; so I myself never wrote something mentionable about religious phenomena. You can only find some traces of this in footnotes of my writings.

Sometime later I was very much occupied, through Matthes, by the analysis of present-day religious texts, especially sermons of Catholic bishops, of very conservative Catholic bishops. Matthes wanted to find out a sociology-of-knowledge type of method how to analyze these practical productions of religious world views. For this purpose, we would work together with research assistants of the Catholic and Protestant faculty in Münster. And doing this in those days at end of the 60s, we, Evi and I, received lots of visits in our small apartment by Catholic priests who would be scientific assistants, for example, assistants of Karl Rahner, and they loved to come to us and get some cooking of Evi, and after that, we would start to work to do the analysis of those sermons. But soon I realized that we were not able to do really convincing and satisfying sociological research in analyzing those religious practice texts. Perhaps, I had not really understood and realized that Matthes had already developed certain building blocks for such a sociology-of-knowledge type of text analysis in his very remarkable habilitation thesis<sup>8</sup> on how the federal legislation on social services was prepared, discussed, revised, and decided on in the West German parliament in the year 1961. Even today this legislation, more than 50 years ago, shapes the institution of social work and social services in the re-unified Germany. Looking back from today, I know that Matthes' 1964 analysis of the debates in the German Parliament in 1961 and before, especially how the two churches were able to put their imprint on the parliamentary discussions, encapsulates lots of seminal ideas how to empirically analyze public discourses.<sup>9</sup> Today, you can

find something comparable, although much more complex, and of course, drawing on much more basic-theoretical background, which was created and/or came into the sight and discussion of the social sciences only much later – in the habilitation thesis of Marek Czyżewski.<sup>10</sup>

Anyway, during those days at the end of the 60s, it was not possible for me to bridge the gap between Matthes' interpretive sociology-of-knowledge type of text analysis and the new concepts of analytical philosophy, like those of John Austin and John Searle, which Matthes himself had entrusted to me, that is, he had asked me to make use of. [And, probably Matthes himself did not clearly see the seminal basic theoretical and methodological potential of his habilitation thesis for discourse and text analysis by then.] To put it short, in those days, although I liked our explorations into the field of sociological text analysis very much, I did not see how to put the combined new ideas of analytical philosophy, of neo-positivistic text analysis, and text critique (like those of Ernst Topitsch and Hans Albert<sup>11</sup>), as well as of phenomenologically inspired sociology of knowledge (like that of Berger and Luckmann) into practical use of text analysis. Of course, you could interpretively muddle through and on with the text analysis of sermons, you could somehow make use of some of the ideas of analytical philosophy and neo-positivism, and you, indeed, got something interesting out of the concrete text analyses. However, we could not develop a really stable canon of methods for sociological text analysis. Today, I feel confi-

dent to have such a backbone of methods for text analysis, and I could go back now to such sermons and I could show what would be the principles of such an analysis.<sup>12</sup> All these exercises in the analysis of religious text were very interesting for me, but I wanted to be able to study social phenomena on the empirical base of texts in a straightforward and clear methodic way, and this, in 1969, nobody could show us. I was expected to write a PhD dissertation in the field of sociology of religion using strategies of sociological text analysis. I did 250 pages on the concept and features of invisible religion, you know, this very intriguing concept of Thomas Luckmann.<sup>13</sup> But, I wanted to make an empirically researchable set of features out of it, and I realized that I was not able to accomplish this. I was totally unhappy about my failures, and I dropped this really interesting and basic-theoretically complex topic. Looking at my shortcomings, I realized that I should basically know how social reality is put into language by the members of society and how it is expressed through language. I realized that I should study the work steps and layers of the presentation of reality through the medium of language. This is in short, how I came to the most important subject of "my sociological life."

At the same time, Matthes prodded me to read Harold Garfinkel and Alfred Schütz. So I started to do this. Of course, I had to read most of this in English, and I was just able to read English like I would be able to read Latin. I could not pronounce the English words at all, but I was nice-

ly able to read English texts, especially Harold Garfinkel's *Studies in Ethnomethodology* and Alfred Schütz. Joachim Matthes had told me that Alfred Schütz, as a refugee-immigrant to the U.S., was not that easygoing in his use of the English language. So it would be quite easy for me to read his 3 volumes of *Collected Papers*. I had read his German language habilitation thesis *Der sinnhafte Aufbau der sozialen Welt*; I had done this in my second and third semester, and, by then, for me, this magnificent treatise was almost impossible to understand; but Matthes had advised me, "Just read the *Collected Papers*, this will be much easier for you," and it was really true. Then, I read George Herbert Mead's *Mind, Self and Society* and Anselm Strauss's *Mirrors and Masks* in the German translation. And then, in 1967, the very important book-length joint review of Jürgen Habermas about the new developments within the Anglo-American social sciences, language philosophy, and methodology came out. Habermas had read all the newer American and British texts in the just mentioned fields, and now he explicated what it was all about and he commented on them in a really circumspect way, and by this he laid a really new ground for the logic of the social sciences.<sup>14</sup> This book was extremely important: that we would now have a very circumspect review and assessment in the German language of all these quite complicated books with ideas that would be very new in the parochial German situation not much more than 20 years after the Nazi times with all its burnings of books, chasing out of very good sociologists, and mental isolation. For us, young sociologists, after having read Habermas' review book, it was

<sup>8</sup> See: Matthes (1964).

<sup>9</sup> See: Schütze (2009:18-31, especially 20, 30f).

<sup>10</sup> See: Czyżewski (2005).

<sup>11</sup> See, e.g., Topitsch and Albert (1965).

<sup>12</sup> See, e.g., Schütze, Lützen, and Schulmeyer-Herbold (1993).

<sup>13</sup> See: Luckmann and Thomas (1967).

<sup>14</sup> See: Habermas (1970).

clear it would be necessary to get in contact with these Anglo-Saxon researchers.

**K.K.:** Had you already graduated?

**F.S.:** No, I just wrote my PhD thesis, and this was not finished by that time. In those days it was quite uncommon to do a *magister* (MA); this new examination had been just introduced, but normally you did not do this if you wanted to stay at the university. I just did my PhD in the year 1972, and I did not write a Master thesis.

**K.K.:** So what was your status then? Were you a student until PhD?

**F.S.:** Yes, in legal terms, I was a student until my PhD, but, you know, in my function, I was almost like an assistant. I earned money because of Matthes. He wanted to have me as a young co-worker. I could have done this piece on invisible religion as Master thesis; actually, it would have been a quite nice Master thesis, but this was not common to do a Master at all. I even earned so much that we could live on this. I had married Evi when I was 22, when I was 24, we had Irene as our first child, and then Evi could get out of her work, and we could live on that the money that I earned in the university. Factually, in my function of teaching and research assistant, I was a scientific assistant, although not by status. (laughter) How Matthes could manage to do this I do not know, but in those days something like that was possible. [I was in one of the stipend foundations, too, but in most of my study time I did not need to collect the monthly money assignment from them, since I earned enough.]

Coming back to the work assignments of Matthes for me: there was the idea to analyze sermons, parliamentary speeches, and political and ideological texts – the latter written in order to structure the new societal landscape. You know, connected with establishment of the new law institutions, the writing of new law texts and the ordering of social services in West Germany after the war, you could observe the production of lots of ideological statements. I guess it was in Poland the same way after the breakdown of Soviet control, of course some bit different in content but in function probably basically the same. And I realized – let us, for example, take Mannheim's contrast set of ideology and utopia, or let us even take the Marxian concept of ideology as such – we were not able to analyze it with concise methods. So I knew we needed to find something new. Having had some knowledge about linguistics, which I have mentioned already, it became quite natural for me that I should find out how social reality is embodied in language. This finally was my dissertation, a very long piece of more than a 1000 pages in two volumes (laughter, showing the book) *Sprache soziologisch gesehen*.<sup>15</sup>

**K.K.:** (laughter)

**F.S.:** And even the original dissertation was roughly 1100 pages, and then I worked 3 years longer in order to get it published.

**K.K.:** And you added more pages?

**F.S.:** Yes, I added more pages in order to make it more understandable. The language of the disser-

<sup>15</sup> See: Schütze (1975a; 1975b).

tation about language, ironically, was quite complicated; and the linguistics professor, Professor Gipper, very righteously said that I should split the very long sentences; it would be awful to read such long sentences incessantly occurring on a thousand pages.

**K.K.:** But you never did.

**F.S.:** No, I did, I did this splitting of the long sentences (a language disease of mine I had never thought about), and this was very time-consuming to do, but I really did it. And in these two published volumes you will find the basic idea of the autobiographical narrative interview, you will find the basic ideas of interaction postulates and communication under constraint, as well as my basic thinking about conversation analysis, which was very intriguing for me. The other thing we did, we published in two volumes what was called *A Reader*<sup>16</sup> of the most important papers – or at least *some* of the most important, as we saw it during those days – of interpretive social science or “West Coast approaches” (not only sociology, but in addition, cognitive anthropology and ethnolinguistics) of the United States, and we did the translations with annotations, quite a long introduction, and the last chapter with our own perspective, how to put these wonderful insights of the American researcher to possible use within the empirical sociology of knowledge in Germany. Yes, I can say we dug quite deeply into it, and the two *reader* vol-

<sup>16</sup> See: Fritz Schütze, Ralf Bohnsack, Joachim Matthes, Werner Meinefeld, Werner Springer, Ansgar Weyman, eds. (1973). The volumes consist of translations of papers of Thomas Wilson, Herbert Blumer, Aaron Cicourel, Harold Garfinkel and Harvey Sack, Georges Psathas, Anthony F. C. Wallace, Charles Frake, and Dell Hymes.

umes were published in 1973, and my PhD dissertation in 1975.

### Establishing Contacts with American Representatives of Interpretative Approaches – Encounters, Collaboration, and Friendships

**F.S.:** In writing my PhD dissertation, I had realized that, looking at the small list of really dependable research methods we had at hand in the 70s for the analysis of socially relevant texts and language productions in general, it was not possible to study those complex phenomena of the social reality I had originally desired to study – complex phenomena which belong to the German tradition of social thinking, like religion and ideologies. I had realized that even if I took the latest sophisticated developments of concepts within the sociology of religion or the sociology of knowledge, like Luckmann's concept of invisible religion, it would not be possible in those days to accomplish an analyses of phenomena hinted to through those concepts in a methodologically stable way. [Today we have learnt quite a lot more in terms of research techniques, and today, an analysis of the enigmatic features of invisible religion would be much easier to realize.] Taking into account the small methodological and technical possibilities we had in our hands in the 70s, we should be humble and look at most simple phenomena in social reality, and find out step by step, looking over one's own shoulder, how to successfully harness the expression and marking power of everyday language. This is how it came to my “linguistic turn.”

After I had done my PhD thesis, I knew I should empirically study phenomena which would be

some bit more graspable than these sophisticated sermons and ideological constructions in the parameter of parliamentary debates. I wanted to concentrate on really simple stuff, and I had to develop the general idea of extempore storytelling as a means to get to personal experiences. If you would approach members of social circles and let them tell their personal experiences of sociologically interesting topics, and then you would analyze these narrative accounts, this would be one of the simplest ways to get some grasp of reality. In the context of social circles, that would be of sociological interest, you could attempt to collect extempore narratives of sociologically interesting topics, and through the analysis of those narratives, you would find out how these members of social circles would really feel. I realized that you could not find this elementary relating to social reality in those sermons and ideological constructions I had studied before. This was my basic idea after all my dealings with very complicated basic-theoretical, epistemological-philosophical, and methodological writings. I thought about something that would be socially relevant and not too complicated in order that we could develop our language-related research tools, and, of course, it had to be something that ordinary people would like to talk about.

As I said, I had studied linguistics as second subject, and I had Matthes who was very much interested in language. But then I realized that in the 70s lots of sociologists in West Germany would go into the direction of abstract, “derivatory” Marxism, a style of thinking that was called “structural Marxism,” and in the direction of Luhmann’s system theory. Only a tiny group of German sociologists would

go into the direction of symbolic interactionisms, phenomenology, ethnomethodology, and interpretative sociology in general. I had been all the time an interpretative sociologist, even “by upbringing” in several senses. Of course, I never was a follower of system theory and never was an adept of classical Marxism. I loved some works of Karl Marx and Friedrich Engels, for example, the *Eighteenth Brumaire of Louis Bonaparte* or *The Situation of the Working Class in England*, but basically, I was just an interactionist. Looking at the research-political might of all these structural macro-sociological approaches, I got the idea that it could be some strength in it if the few sociologists interested in language-related interpretive research would establish a stable platform of discussion, and then I started to think about other people. Of course, I was very much interested in the work of Ulrich Oevermann, who is a bit older than me; he worked in Frankfurt and Berlin. I had gained a lot from my reading and study of his PhD dissertation; so, I got in contact with him. And obviously, I wanted to study, what Luckmann’s assistants and co-workers did in their research in Constance; so I got into contact with them as well, especially with Richard Grathoff. We founded some sort of official *ad hoc* group “Sociology of Language” in the German Sociological Association in order to later establish a full-sized section in the German Sociological Association (DGS) called “Section of Sociology of Language.” After first negotiations with the DGS, it became the said *ad hoc* group, some bit preliminary accepted by the German Sociological Association (DGS) and by the German Research Foundation (DFG), but in the beginning, it was not institutionalized at all. Later, then, it became the Section of

Sociology of Language in the DGS and, again, even much later, the Section of Sociology of Knowledge in the DGS. [That means: in later days the name was changed in order to make sure that the topic of analysis would be the various layers of knowledge as a legitimate focus of sociological research, and not the working of language as such, which started to be much more seen as the research field of linguistics proper. Today, the section of sociology of knowledge in the DGS is very much controlled by the Foucault type of discourse analysis; the strong import on it might even become quite dangerous for biography analysis. Biography analysis is organized in another section of the DGS: that of *Biographieforschung*. I was not involved in the foundation of the latter, which occurred sometime later. Today, I am much more active in the DFG section of biography analysis.] Anyway, I got into a relationship with Thomas Luckmann, with Hansfried Kellner, with Richard Grathoff, the main assistant of Luckmann, with the two research assistants of Grathoff, Bruno Hildenbrand and Jörg Bergmann, with Ulrich Oevermann, and a little later with Hans-Georg Soeffner, too. I got important help by Ralf Bohnsack and Gerhard Riemann. Ralf, by then, was a research assistant in research projects in the Bielefeld Faculty of Sociology and partially, too, an employee in the newly founded Centre for Interdisciplinary Study in Bielefeld (ZIF); he had used Garfinkel’s approach for the analysis of the institutional processing of the delinquent behavior of adolescents by police and criminal court.<sup>17</sup> Ger-

<sup>17</sup> See: Bohnsack (1973). Later, Ralf Bohnsack developed the very important qualitative or reconstructive social research style of “documentary method” indebted to the legacy of Karl Mannheim. He wrote numerous important research books. Instead of listing them here, we just refer to his *Rekonstruktive Sozialforschung. Einführung in qualitative Methoden* (2003).

hard was still an advanced student preparing his empirical research on a settlement of the homeless and analyzing lots of speech materials he had recorded in this social setting; he was perfectly fluent in English. Especially Hansfried Kellner, Richard Grathoff, Ulrich Oevermann, Hans-Gerog Soeffner, and I would meet together, and we worked together in writing applications. We were the founders of this ad-hoc group of “Sociology of Language” and, later on, of the section by this name in the DGS. So, we established this Section of Sociology of Language in the DGS, and then, just to mention this in advance, at the end of 1978, I went to Anselm Strauss in San Francisco and I handed the organizational work for the section over to Jörg Bergmann.

And the most knowledgeable person in terms of doing things and having seen lots of the relevant researchers in the U.S. was Richard Grathoff,<sup>18</sup> who later became very important in terms of his relationship to Poland, too. He had a relationship with Antonina Kłoskowska, not during those days,

<sup>18</sup> Professor Richard Grathoff died in November 2013. Having gotten the first notice of Richard Grathoff’s death by my Polish friends, I wrote to them: “[h]e was extremely important for the foundations of qualitative sociology in Germany. He brought all the American stars, like Goffman, Garfinkel, Cicourel, Sacks, Schegloff, Gumperz, and others to Germany and enabled the first three pivotal conferences in Bielefeld, Gottlieben, and Constance. He was the engine of founding the section of Sociology of Language (later re-named to Sociology of Knowledge) in the German Sociological Association as a first formal acknowledgement of the existence of qualitative sociology in the German Sociological Association (with lots of relevance for later decisions in the German Research Foundation). In addition, without him, I would not have met Anselm and Fran Strauss, my dear friends, and I probably would never have come to Poland (he had sent Marek [Czyżewski] to Kassel in 1981). Personally, I am very grateful to him for lots of stimulation and encouragement, although he also put a lot of work on my shoulders, as I told you. But, he really was a very generous and lovable man. In addition, he was a sociologist and phenomenological philosopher with very deep and seminal thoughts.

but a little bit later. He was a genius of fruitful research relationships, and this was only possible on the base of a very social and lovable personality. In addition, he was an eminent phenomenological researcher of the abysses of social reality. He knew many scholars of the interpretive research approaches in America since he had studied in America and gotten his PhD in the New School of Social Research in NYC. He had studied in a New School of Social Research in New York City. He had written this beautiful dissertation on social inconsistency *The Structure of Social Inconsistencies* (1970) using the English language. So, we got the idea to invite eminent researchers of the various approaches of American interpretative sociology. I do not remember *who* got and *how* we got this idea of mass invitation, but I am sure that the reason of being encouraged to do so was Richard; he was pivotal because he knew all these researchers, and he was such a gifted provider of incentives, liaison worker, and enabler. So, we did an application to the newly founded ZIF – *Zentrum für Interdisziplinäre Forschung* in Bielefeld and would put a remarkable list of names in the application, and the ZIF accepted, so they were invited to come over for a quite remarkable symposium. The conference took place in 1973. We did not meet Anselm Strauss by then. But Harvey Sacks, Harold Garfinkel, Erving Goffman, John Gumper, Susan Ervin-Tripp, and some others would be there. There was this cleavage between Aaron Cicourel and Garfinkel; Cicourel had been a disciple of Garfinkel and then they split away. [I cannot remember now when we invited Cicourel, Richard did this later. But, we were aware that it was not polite to invite both together be-

cause there was this tension between them.] So, all quite a number of these very interesting interpretive researchers came over. Maybe I left out one or two people. I can remember that Goffman presented his new book *Frame Analysis*. He was about to publish it, we got it as a manuscript. The conference was done in that way that young Germans would write commentaries to manuscripts of new research coming from America. And lots of young people would attend this conference, including linguists, and all of them would get some bit of orientation by the conference. Some of the famous presenters were later invited again to other conferences. For example, Harvey Sacks came to Bielefeld several times.

And then Richard said, “I can do more.” He had some good relationship to the Thyssen Foundation. It was a big German steel company, and they had a foundation for supporting “free science.” They gave a considerable amount of money, and he could organize other conferences and workshops in the surroundings of Constance. He was helped in this by Bruno Hildenbrand and Jörg Bergmann who I have mentioned already. [Richard had a research project financed by the *Thyssen Stiftung* on psychiatric patients; Bruno and Jörg were a research assistants in this project. The doctoral dissertation of Bruno Hildenbrand came out of this research project.<sup>19</sup> Both, Bruno Hildenbrand and Jörg Bergmann, later became well-known professors of sociology who would do qualitative research.] In my assessment, the most beautiful of these new conferences was the conference in the medieval hotel “Drachenburg” in Gottlieben, Switzerland,

<sup>19</sup> See: Hildenbrand (1983).

in the very proximity of Constance. That was the first time I met Anselm Strauss and his wife Fran Strauss. Fran would later say, “Fritz was barely able to speak a word of English or he did not dare to speak a word of English.” My very close younger friend, co-worker and former student, Gerhard Riemann, would be some bit the inter-mediator or even translator. We, Gerhard and I, got into lots of discussions with Fran and Anselm, and we tried to explain some bit what had happened in Nazi Germany. I got immediately in a deep relationship with Anselm and Fran, and this developed even more in later years.

The encounters with the American researchers of interpretive social science had a big impact on us, the young generation of German researchers in interpretative, qualitative, communicative, reconstructive sociology, and some bit on young sociolinguistic researchers in German linguistics, too. [For example, John Gumperz became a multi-time visitor of the Institute for the German Language – *Institut für Deutsche Sprache* – in Mannheim in the department Language and Society lead by Werner Kallmeyer.<sup>20</sup>] All these famous professors of qualitative research were very friendly and understanding to us, young German researchers. Goffman would come to Evi’s and my little flat; he was extremely witty and at the same time very modest. He looked more like an American barkeeper (as far as I had an image on such an occupational type of person from American movies), and you would never imagine that this would be a very, very prominent professor of sociology. Being then a professor of the University of Pennsylvania in Philadelphia,

<sup>20</sup> See, e.g., Gumperz (1994).

he had an old beetle car with an impaired heating, and in Winter times, he put a *Steinhager flask* with hot water, this kind of earthenware bottle, under his beetle seat in order not to get too icy. (laughter)

**K.K.:** (laughter)

**F.S.:** And Harvey Sacks was a very modest person who did a very interesting type of teaching, he was very concentrated, he would never look at the manuscripts, he roamed around on the stage, went back and forth, and would teach in a very intensive and lively way. And Anselm Strauss was a very nice and modest character, too, as you probably know yourself. And Garfinkel, too, was wonderfully friendly to his younger German colleagues. In the house of Richard Grathoff, he told us his life history, maybe 15-20 people would be around as listeners. We learned from him that he had had lots of difficulties as a young university teacher: since his way of thinking and his topics were so uncommon, he did not have enough students in the beginning and therefore, he got harshly criticized from the university administration in UCLA. Cicourel was a lovely and impressive man, too; his style of presentation was more argumentative and very lively. He spoke fluent Spanish, and he told us about his fights for the acceptance of the Spanish language in Southern Californian public administration. [One special interest of him was the Ladino language of the Sephardic Jews he had spoken in his childhood. He asked us, if there would be university teaching of, and research on, the Yiddish language in Germany.] Generally speaking, all these important professors did not behave like old-fashioned German professors, not

like these “doctor-professors,” as one of them put it. We, younger Germans, liked this very much and to some bit became a role model for filling in our own professorships later on.

In the 70s, there were lots of difficulties in Germany regarding this type of (interpretative, qualitative, communicative, reconstructive) sociology. Of course, this sort of sociology has an obvious German-language tradition, too, coming from Max Weber, Ferdinand Tönnies, Alfred Schütz, and Karl Mannheim, especially from Karl Mannheim. But, most of it was invisible in the 70s, because in the 50s and 60s it was the time of Talcott Parsons type of sociology, although, if you look more closely, there is lots of interpretative types of ideas in the work of Parsons. And in the 60s, with the student movement, and especially in the 70s, the structural Marxism and system theory was powerful so there was not very much place for interpretative sociology. There were just the circles in Constance, Frankfurt, and Bielefeld. Although Joachim Matthes had given lots of impulse, he retreated more and more from the new circles of qualitative research, and we, the young persons, got more and more into a relationship with the mentioned American researchers. And today, looking back, I would say a little bit that he naturally felt, although he never mentioned something about it, that we, who attended these conferences, would be the next generation, and he might have had some disappointment about the drifting away of the younger generation. Today, I know that we could have gone on with his idea on how to analyze ideologies, although it was not a manageable method during those days

and maybe we would be too enthusiastic, too rave regarding our heroes, like Goffman or Garfinkel, and he felt some bit put aside by us, but, alas, I did not realize it at all. And through Matthes I had learned about Schütz, Garfinkel, and Goffman. Through him I had gotten in contact with Thomas Luckmann and his main assistant, Richard Grathoff, with Ulrich Oevermann, and with Hansfried Kellner. All of this had been facilitated by Joachim Matthes, but maybe latter on it was quite difficult for him that I would so intensely stay in contact with all these people. Anyway, he stepped out some bit.

When Joachim Matthes left Bielefeld, he went to the University Erlangen-Nuremberg. The University of Bielefeld redefined the denomination of his position as “sociology of the social structure of the Federal Republic of Germany” – in a total contrast to what Matthes had taught and researched on. In some groups of the student body there was disappointment about it. What happened is that students made a go-in into the faculty meeting (perhaps some bit organized by Gerhard Riemann, I really cannot remember it), and the original decision of the faculty was cancelled. The denomination was redefined into a denomination for interpretative sociology, maybe the first professorial position in Germany with such a clear-cut denomination. Of course, there would be people like Luckmann and Kellner, but they would not have professorships defined in these clear-cut terms. Regarding the filling in of the position in Bielefeld: Oevermann got a call for this professorship in Bielefeld, but he had a call to Frankfurt University, too, and he preferred to stay in Frankfurt. And then, Richard

Grathoff got the call to Bielefeld, and he took it. So at least you can say that there was some impact of interpretative sociology on the Bielefeld students, and I had probably had some role in this development that students would get interested in interpretative sociology. Another circumstance might have been that Matthes tended to move into the direction of structural Marxism for a while in the middle of the 70s. So, still being a scientific assistant, I got into some university-political difficulties with him, focusing on a quarrel about the definition of one assistant position: either it was to go onto the side of structural Marxism, or onto the side of interpretative sociology, and finally I won. And then the students, and even these followers of structural Marxist sociology, started to think, “Oh, it must be something worthwhile in this type of ‘small-reach’ sociology because he can manage to do things like this. He must have gotten a special understanding of interaction and negotiations. This seems to be worthwhile, we must study that.” So, tiny things like that developed going into the direction of a more stable situation for interpretative sociology. (laughter)

### Working on the Idea of Autobiographical Narrative Interview and Its Analysis

F.S.: During those days in 1972 and 1973, the state or “land” of North Rhine – Westphalia – in the Federal Republic of West Germany decided to organize many mergers of local communities, if they were too small to provide for a good local self-administration and communal services and if these local communities were situated next to each other. Some are split into separate parts

again today, but there was this idea coming from some organizers in the government who thought that it would be a really rational measure to make the administration easier, cheaper, and more effective. [There is a similar conspicuously rational governmental idea of today: the “lighthouse” idea that you put lots of money in a few really famous German universities and not in others in order that you could emulate with Harvard and Oxford. However, by this measure the other German universities will sink down into mediocrity, and you lose the real quality of the German university system – that each university is principally on eye level with the others. There is some sort of notoriously rational engine of sick thinking within most of the German governments; one wave of sickness of thinking is coming after another.] And we had these phenomena of newly fused local communities in the vicinity of Bielefeld. I thought that doing research on the narratives of these mergers of communities would be one of the “simple,” down-to-earth phenomena I was talking about as desirable topics of research sometime before.<sup>21</sup> So I started in Schloss Holte – Stukenbrock where the communal politicians and ordinary citizens would especially harshly fight about the name of the fused community.<sup>22</sup> I thought, “Let’s tell them about it.” And I had very helpful students, who would be not so much younger than me, who helped me. And one of them was Gerhard (Riemann) who has become especially important for

<sup>21</sup> I wanted to concentrate on really simple stuff, and I had to develop the general idea of extempore storytelling as a means of getting to personal experiences.

<sup>22</sup> More detailed descriptions of this project, as well as work on the idea of autobiographical narrative interview is introduced by Gerhard Riemann in his text “A Joint Project Against the Backdrop of a Research Tradition: An Introduction to ‘Doing Biographical Research’” (2006).

me. And so we started to do this type of narrative interviewing on collective stories, and it worked beautifully. After a short while, after the interview with main protagonists mentioned in the newspapers like the two former mayors, other informants in Schloss Holte – Stukenbrock wanted to be interviewed, too, and we had the idea that we would interview all the people who would show up as *dramatis personae* within the recorded narratives about the merging of the community. A very interesting mechanisms of extempore storytelling showed up: it came out that when the informants had started to talk about the quarrel regarding the name of the community, they would – step by step – have to tell the *whole* story about the merger of the communities: not only about the *name* conflict. The informants were driven to tell about the decision who should be the new mayor of the fused community, who, from the group of former mayors (if there would be more than two communities to be merged) and administrators, would be left out from getting a position again, who would be – mostly without a clear-intention in the beginning or “willy-nilly” – withdrawn into criminal acts in the times of “interregnum” with lots of essential lack of legal and organizational regulations, and things like that. Later, we did this type of narrative interviewing in two other fused communities in other West German lands, too. One was consisting out of two former proud towns, one protestant and one catholic, and some additional villages. They are still together today, but in-between, it was quite difficult for them. And I can remember that I would go to the still cherished princess, the very offspring of the former reigning family in the protestant town. By

then, she was a member of the Social Democratic Party and its quite leftist youth movement. But, she was still a princess (a *Fürstin*), and she had lots of other titles. She wanted to be addressed as *Durchlaucht* (*your highness*), or something like that. Both of us, Gerhard and I, had lots of difficulties to address her like that because

**K.K.:** (laughter)

**F.S.:** it felt for us to be so odd. She was politically leftist (laughter), and she would meet us in her little castle. There is a central castle in that Protestant town, but some kilometers outside that town is another smaller castle constructed by one of the most remarkable German baroque architects. And when we put down the tape recorder on this table from the 18<sup>th</sup> century, it was something that did not belong there

**K.K.:** (laughter)

**F.S.:** but we could manage some bit. And on her lap sat her son, 3 years old, a beautiful boy, like his mother – an arrangement totally beautiful, like a classical art painting. At the very end she would show us a little around in her palace and we saw a painting of a young boy of the 18<sup>th</sup> century, looking like her boy. So, things like that happened, and Gerhard and I were also doing these types of interviews in a merged community in East Friesland, using the connections of my sister who is married to one of the big farmers there. There it was sometimes difficult to motivate the community politicians in the countryside to tell their personal story since they did not believe that we would under-

stand their Low German, the *Hanse* language, but finally we could manage there, too.

Then we had all these interviews of the 3 fusions and we realized, when looking at the text, that there would be a lot of biographical stuff in it, like that, that somebody got depressed, somebody was about to commit suicide, somebody would feel ashamed because he was postponed, some would get into these “willy-nilly” committed criminal acts, and all this was in it. We thought that we could follow up lots of interesting research questions in these interviews with these informants, but we did not know how to analyze these interviews in a transparent and systematic way, following the methodological rules of validity, reliability, and repeatability. We would have these collective phenomena in the interviews, on the one hand, and we would have chunks of autobiographical renderings in them, on the other, too, and we did not know how to put these two phenomena together in a basic-theoretically and methodically transparent and constructive way; we really did not know what to do with it. In between, dealing in quite practical ways with the recording and the analysis of actually ongoing conversation, I had learned to do an ethnographical type of conversation analysis or interaction analysis. This was done by using parts of the classical Californian conversation analysis, like that one of Harvey Sacks and Emanuel Schegloff. [Harvey Sacks we knew very well, as I told you, he came to our apartment several times, and we had a very good relationship.] But, other features of my type of ethnographic interaction analysis as developed together with my linguistic friend Werner Kallmeyer stemmed from

the ethnography of communication (John Gumperz and Dell Hymes) and partially from symbolic interactionism. This type of interaction analysis was quite practicable, and even today I use it basically unchanged. In the second half of the 70s, I started to be able to do this type of interaction analysis.<sup>23</sup> As I said, I did it quite a lot with my linguistic friend Werner Kallmeyer who even today is in a close relationship with me.<sup>24</sup> So, I started to use the methodology of our type of conversation analysis when looking at the narrative texts of the fusions of local communities. But, it still was very difficult to analyze these quite different types of empirical text materials – different from the transcripts of actually ongoing verbal interaction. I realized that the reason for this systematic difficulty was that the narratives of collective events were very much interwoven with very personal autobiographical chunks of text material: for example, one of the mayors told us that he suffered a lot by the fusion and got a heart infarction.

It was quite natural then that we, Gerhard and I, surmised that it would be enlightening first to study autobiographical narrative renderings properly in order to learn how to analyze these chunks of autobiographical narration in those narratives by community politicians about collective events in their local communities. And, I have to confess, that

<sup>23</sup> See: Kallmeyer, Werner, and Schütze (1976; 1977), Kallmeyer (1988).

<sup>24</sup> In the end of the 70s, Werner Kallmeyer became head of the Department *Sprache und Gesellschaft* of the Institut für Deutsche Sprache (IDS) in Mannheim. He was a director of the important sociolinguistic research project on the city language of Manheim. See: Kallmeyer (1994a; 1994b; 1994c; 1995). Three of his English language articles are: Kallmeyer and Keim (1996), Kallmeyer and Streeck (2001), Kallmeyer (2002).

we did not even know if this would be possible: just to let people tell their life histories in the extempore way, if they would really be able to do it, if this could be done by them in an ordered way. Lots of discussions were going on between, especially, Günther Robert, who very much helped in establishing the method of the narrative interview in biography research, too, and Gerhard Riemann and I. And then I said, "Let's do it and let's ask our friends, if they have friends who we do not know and who would have an interesting life history to tell." (laughter) And that we did, and we got quite a lot of very long extempore autobiographical statements, and it was a wonderful experience that people would tell us these personal stories – sometimes for even 6, 7, 8 hours. Sometimes, we were close to falling asleep since we became so tired. (laughter)

**K.K.:** (laughter)

**F.S.:** In former days, we did all interviews with two interviewers because we thought we should install some sort of collective situation to make sure for the interviewees, but for us themselves, too, that the interview work would be something done in society for society.

**K.K.:** The real interaction.

**F.S.:** Yes, the real interaction. And, in addition, in the beginning we felt we had to be very careful in listening. In the narrative interview, there is the rule that after the end of the main story line, you have to exhaust the additional narrative potential of the story to be told. And, in accomplishing this in your ongoing listening, you have to look

for narrative hints for additional experiential material. There are two types of them.<sup>25</sup> One type consists out of the "narrative spigots." These are freely given hints of the interviewee that she or he could – and would like – to tell more about if the interviewer would be interested in it. The second type of hints for additional narrative potential would be points of vagueness, hesitation, and discrepancies in the course of extempore narrative renderings of personal experiences. We surmised that at least some of them could be quite difficult to detect by the listener in the course of the ongoing main story line, and, in addition, it could be quite difficult to focus on them in the course of addition questioning. We were afraid that this very careful listening for detecting the additional narrative potential might not be possible when to do it just with one person as interviewer. And, of course, we liked to exchange our experiences as interviewers and listeners afterwards. Later on, we found out that interviewing with two persons is not necessary at all. The situation when there are two interviewers can be quite a good arrangement when there is some need (since the informant is in doubt about the value of such interviewing) to make sure that it is a social situation of cooperative work relevant for society and this work is addressed to the collectivity of the scientific audience. In addition, the two-interviewer arrangement can be very useful in case the expected life history and the topics connected with it could be very complex. [Two interviewers can be more on the alert.] Anyway, it was very important for Gerhard and I to do the first interviews in the

<sup>25</sup> See: Schütze (2008a:153-242, 243-298; 2008b:6-77, especially 16f).

style of narrative interviewing together in order to observe what was happening since we did not know if it would be possible at all to get possible informants to tell their life histories off-the-cuff. We did not know about all of this, and we had to observe each other even falling asleep (laughter), and then, of course, the other interviewer would be the person on the alert.

**K.K.:** (laughter)

**F.S.:** And afterwards, we talked about it, and, again, we learned a lot about it in our "afterword" conversations.

And then, I had these long autobiographical narratives and I started to use the general perspective of my and Werner Kallmeyer's type of conversation or interaction analysis on them. In doing lots of sequential analyses, I realized that there would be obvious repetitive expressive items (formulations of general predicates, short summary statements, and evaluations connected with them, and forms of certain disorders like background constructions) in all of these interview texts. I got started to ask myself how to put these repetitive phenomena to analytical use: what to do with all these supra-segmental markers, as I would call them today. Then, I realized that the repetitions of these expression items were linked to the expression of experiential content that would be in-between those markers. In addition, I still remembered my quite nice education in linguistics and I knew that those repeated expression devices would hint to, or depict, general features of experiential information within the topical field of the narrative – the general features of the expres-

sion of biographical experiences. Therefore, quite early I came up with the basic idea that the general features of various biographical process structures would be marked by these supra-segmental markers. This was the central discovery of regarding my type of biographical research.

The problem, which was still unsolved was; when one used these formal structures for one's analysis in doing what I then called "structural description" and "analytical abstraction," how could one put the research process and its results into a *readable* version of representing the research outcome? I was not the person who was able to develop this. It was Gerhard (Riemann) who did this with his doctoral dissertation.<sup>26</sup> He invented the readable form of our type of biography-analytical research with chapters on the structural descriptions and analytical abstractions of single interview as single cases, and chapters on their contrastive comparison and developing a substantive-theoretical model. He stressed very much the holistic overall gestalt of the structural description, and his doctoral dissertation remains to be one of the most elegantly readable pieces of qualitative research to date. The import on the holistic gestalts of the analyzed biographies as single cases is important up to today, but there is some deviation from the rule of quoting every line of the interview and giving an elaborate analytical description of every narrative unit of the main story line of the interview (or the interview at all) by separately written statements within the final book as a research report. Later, we found out that, although it is necessary to produce a structural

<sup>26</sup> See: Riemann (1987).

description of the *whole* autobiographical narrative interview to be analyzed with all its segmentations, it is, nevertheless, not necessary to put all the meticulous formulations of such structural descriptions into the final book as research report. You could just select some pieces of the structural description, which most conspicuously demonstrate the dominant biographical process structures. It is not necessary to formulate all of the segmentations of the whole autobiographical narrative and all the descriptions of the in-between inner-unit phenomena you have empirically found out about. You just select the most important pieces of the structural descriptions showing the biographical process structures, their dominant developments and their transformations, as well as the changes from one to the other and the oscillations between them. The other parts of the analysis of the interview as a single case you can present in a shortened way by using a somewhat more elaborated form of the depiction of the overall biographical structuring as a first step of analytical abstraction. This shortened way of presenting the analysis of biographical single cases was developed much later. But, Gerhard was the researcher who produced the first really readable text on a substantive topic of biography analysis on the empirical base of autobiographical narrative interviews, that is, on the topic of the life histories of psychiatric patients and of their losing the relationship to their own personal life history and life definition. During those days I was not able to find how to produce this type of readable presentation of the results of substantive sociological topics. In those early days, I was not able to do something that I have done only much lat-

er, for example, the analysis of the fate of small craftsman millers in the world of large marketing organizations of bakers<sup>27</sup> or the fate of an Apache Indian,<sup>28</sup> who would cross the cultural border between the Native-Indian and the American-White culture. The latter article, of course, is not the analysis of an autobiographical narrative interview, but of a written autobiographical statement. But basically, the analysis is similar. [I don't want to go into the intricate questions about the methods for the analysis of written autobiographical texts here, but it is an important question for the sociological biography research of the near future.]

Of course, biography analysis got further development. For example, the connection between certain types of supra-segmental markers and the four elementary forms of biographical process structures was established in two articles quite important for my scientific development.<sup>29</sup> And, although the phenomenon of background construction was something that I found out about very early – even in the times of reworking and amendment of my PhD dissertation – we, Gerhard, Thomas Reim, and I, realized only in the 80s that it is systematically linked with “disorderly” processes in social experience, and especially in biographical development. [These disorderly processes can be of the suffering trajectory type or of the creative metamorphosis type.] Only then we found out how to do the analysis of background constructions. Especially important for my own scientific development was the analysis of extempore stories I did with the

<sup>27</sup> See: Schütze (1991).

<sup>28</sup> See: Schütze (2012a). The review of the book, written by Katarzyna Waniek, is published in this volume of QSR.

<sup>29</sup> See: Schütze (1981; 1984).

book-length study letter *Das narrative Interview in Interaktionsfeldstudien*. It is a 250-page piece about the structure of extempore storytelling of personal experiences.<sup>30</sup> After that, I was quite sure that the collection and an analysis of extempore stories of personal experiences would be a manageable method and that it would be possible to analytically go into the intricacies of socio-biographical phenomena.<sup>31</sup>

### The Idea of Student Research Workshops

**F.S.:** And another thing which was important was that Werner Kallmeyer and I started to have some sort of early type interdisciplinary student research workshop in Bielefeld University. We started with that probably in 1974 and continued this up to 1979. We did this every Friday afternoon for roughly 4 hours, and the students endured this working on empirical text materials happily. In addition, we invited almost all the young experts in sociology, or linguistics or anthropology, who would do sociolinguistics and sociology of language in West Germany, and they really came to our workshop without any payment. Werner's and my workshop was principally open-ended, but normally, it started at 2 o'clock p.m. and would end about 5 to 6 o'clock, although it was official-

<sup>30</sup> See: Schütze (1987).

<sup>31</sup> About the same time, Schütze wrote the long article in English: “Autobiographical Accounts of War Experiences. An outline for the analysis of topically focused autobiographical texts – using the example of the “Robert Rasmus” account in Studs Terkel's book “The Good War,”” showing the main steps of autobiographical narrative interview analysis. This text for a long time remained unpublished, though the manuscript was read by English speaking scholars. The text, slightly reworked and accompanied with contemporarily written foreword and postscript, is published in this volume of QSR for the first time.

ly 2 hours long. We would look at materials, and many of the empirical text materials were collected by our very interested students. So they would put tape recorders into their flats shared with other students and would, for example, record naturally occurring narratives of personal experiences. These naturally occurring narratives, in turn, were used to compare them with interview narratives in order to find out about possible essential changes caused by the professional action scheme of interviewing and the possibly changed (probably declined) capacity of interview narratives to express personal experiences. [Partly, it depends on the social arrangement of the interview situation; in case the arrangement is in accordance with the basic interaction postulates of cooperation in naturally occurring situations, the difference is not that big.]

So, this was some sort of pre-invention of a type of arrangement for a student research workshop in our home university in Bielefeld. At the same time, Ulrich Oevermann was developing something like that, too. This arrangement of research workshop was a social invention happening in several places at the same time. The second stage of my encounter with the (now full-sized) arrangement of an interactive research workshop was when I was in San Francisco 1978/1979, and saw how intensely and effectively Anselm (Strauss) would work with his research team on his research projects, especially on the research project on medical work in hospitals using sophisticated technologies.<sup>32</sup> Reporting about empirical material in a first stage, then analyzing this in a

<sup>32</sup> Strauss, Fagerhaugh, Suczek, and Winer (1985).

second, and thirdly, drawing some (tentatively) theoretical conclusions was always the sequential order of the research workshop arrangement. I learned quite a lot from this regular sequential order and the automatic guidance provided by that. I suggested that Anselm would tape the proceedings and let them to be transcribed in order to produce empirical instances for his rich book on research work and its steps and methods *Qualitative Analysis for Social Scientists* of the year 1987. I am still happy about my quite circumspect suggestion. Generally speaking, the student research workshops, as we practice them today, are a mixture of ideas stemming from our own Bielefeld experiences and Anselm's incentives. From Anselm I not only received new encouragement to arrange open research workshops, but, in addition, I learned quite a lot from him; what the role of the workshop moderator should be, how she or he would carefully listen to a narrative report about the collection of new data and to their description in the beginning of the research workshop, how she or he had to be carefully retrained in order not to overrun other participants with her or his interpretations and suggestions, as well as how the moderator could be most encouraging and propelling for the ongoing analysis of the reporting participant by putting in unexpected contrasts (from his personal experiences, too) and by incenting some sort of "ideational variation" (Edmund Husserl). In the 80s, we started to establish such research workshop arrangements together with our Polish colleagues and friends (who supposedly had already practiced something similar). By this new, joint step in "doing research together" the social arrangement of international

and trans-cultural student research workshops was born.<sup>33</sup>

**K.K.:** Were those seminars from the very beginning also devoted to students?

**F.S.:** It was always done for students in order that they would get first experiences in research steps and we, in reverse, would learn from their new materials brought in and from their fresh thinking about it. We always had this "research colleague" relationship to our students; they would be treated as members of our research community on equal footing with participating scientific assistants (and later, even with participating professors). I kept doing this up to the end of my work time as professor. Sometimes, you get into some difficulties with it, for sure, when you have participants who were not socialized into the habit of taking the perspectives of the other participants involved. And the workshop arrangement is not something that you could do with big masses of students. Treating the students as equals, when they would be willing to go through the narrow door of opening up for enlightenment through empirical materials and learning from co-researchers, was always our tradition that got started through these workshops with Werner Kallmeyer. I only rarely attended the PhD classes of Anselm, too; I always felt that he did basically the same as he did in his own research project groups. Gerhard (Riemann) attended Anselm's research classes much more regularly. He went to San Francisco 2 years later. Anselm did not have any research project by then and a respective research

<sup>33</sup> More details about cooperation with the Polish colleagues can be found in Andrzej Piotrowski's text published in this volume of QSR.

group. When I was in San Francisco, Anselm had his research project on the social organization of medical work (according to the application for financial support, especially with a focus on using medical technology), and the respective research group would meet regularly every week; so it was not necessary for me to attend all his teaching classes in addition. But, Gerhard did this, and he told me about it in detail. Of course, all these experiences of Gerhard and I had a strong influence of how our later student research workshops in Kassel would develop.

In Kassel, the student research workshop became really institutionalized as part of my ordinary university teaching. I do not know how Ulrich Oevermann established and developed his workshop arrangement, but I know that partially he did this out of the formal university teaching setting, too. Later on, Gabriele Rosenthal and Wolfram Fischer would do similar extra-mural research workshops in Berlin. In my case, the student research workshop was always a teaching institution within the regular university teaching. In the University of Kassel, from the very beginning, it was clear that the student research workshops would be a central and regular part of my teaching activity. Later on, in Magdeburg, the research workshop arrangement was difficult to get licensed within the faculty council, and I had to explain it again and again as not being the normal type of advanced seminar (which consists basically out of the presentations of the students and their being criticized by the seminar docent and the seminar group, whereas the student research workshop consists basically out of the cooperative analysis of the empirical

material brought in by a student who would not talk more than the other participants [mostly, even less in order to get the analytical perspectives of the co-participants]. The openness of the student research workshop (you as the moderator do not know in advance what will be the result of the session – a deadly sin in the context of pre-arranged and content-definite university lecturing and teaching), the treatment of the students as equals, and the larger time consumption (because of the emergent character of the research workshop you need more time than one needs for the normal type of seminar) are conventionally seen as irrational in terms of the features and criteria of highly advanced standard teaching. It is bespeaking that the research workshop arrangement as a regular form of teaching activity in the basic courses of the Bachelor study course of social sciences (with the exception that it is done in the cloak of an irregular "extra activity" by guests like me) was immediately put off after I had left Magdeburg University for retirement. [It might have to do with the tight time organization of study courses organized according to the Bologna regulations.] Anyway, I think it is an extremely important feature of a productive university setting to let students undergo open and cooperative research experiences. Such a social arrangement is not restricted to interpretative or qualitative sociology proper; it can be a productive arrangement within all types of social and cultural sciences. It is a very Humboldtian idea: that you would have a social arrangement for a joint research action schema that the students would freely embark on, that it is totally open regarding the results searched for, that the students as research partners would be principally equal to the docents,

although they are much more inexperienced, they have lots of fresh ideas, and that all the participants would work together cooperatively.

### One Year in the United States

**F.S.:** I have just mentioned that I spent some time in the United States and worked with Anselm Strauss. I did an application to the German Research Foundation (DFG) to go for a year to the United States to see Anselm (Strauss) and to see Aaron Cicourel. This was accepted by the DFG, so we did this with our whole family having three daughters by then (1978/1979). First, we went for half a year to Anselm Strauss, San Francisco. However, it turned out that it would be too difficult after the first half year to move from San Francisco to Aaron Cicourel in La Jolla. The children were at school, and it would be quite difficult to let them change schools again; so, we stayed in San Francisco. Aaron Cicourel was very fair, although slightly disappointed that I could not come for the second half of our Californian year to his teaching and research setting. However, I visited Aaron Cicourel for a shorter visit, too, and he was very hospitable and cooperative in his comments on my attempt to establish a biography analysis on the base of autobiographical narrative interviews. Thinking about the levels of abstractions in the course of autobiographical narrative rendering is very much indebted to his thinking on “higher predicates” and summary statements.

And then, of course, I started to cooperate with Anselm. He put a lot of time into people visiting

him, and his cooperation was not just with me, but with numerous others as, for example, with Hans-Georg Soeffner, Gerhard Riemann, and Wolfram Fischer, too. As you know, he had this bad heart condition, and he needed to do some bodily movement. He was happy to have people to talk to on sociological subjects when he would take walks through the Russian Hill district, where he was living with Fran, or through the Golden Gate Park that was easily reachable from the small Victorian house hosting the Institute for Social and Behavioral Sciences situated in the very vicinity of the compound of the University of California Medical Center. Anselm was extremely cooperative and intellectually helpful to me. So, with the background of *Mirrors and Masks* and *Time for Dying* in my mind, I would talk to him about phenomena of suffering with the autobiographical narrative texts and, of course especially on experiences of suffering connected with diseases. I knew Anselm’s trajectory concept and its constitutive features, and I told him that I had found these features in my autobiographical extempore narrative, too. We looked at interviews with very sick patients, some of them conducted by me in San Francisco; I had encountered these patients within the Cancer Clinic of the Moffitt Hospital of UCSF Medical Center, and we agreed that the forms of narration expressing suffering within in my long autobiographical accounts would basically express the elementary features of trajectories. So it was clear that I would name these phenomena of suffering that I saw expressed in the formal structures and abstract content forms of autobiographical narration “trajectory.”<sup>34</sup>

<sup>34</sup> For the concept of trajectory look at, e.g., Riemann and Schütze (1991), Schütze (2008a; 2008b).

Of course, the narrative structures expressing suffering and the corresponding biographical process structures could have been named differently. I guess that even today most of American social-science scholars do not understand that the term “trajectory” has a peculiar meaning in studies of professional medical and nursing work and in biography analysis, because in America, and in the English language in general, it is a common everyday word in the first instance. It then means something like “plan,” or “line,” or “curve,” or whatever, and they do not understand the deeper sense of it in terms of suffering. It is astonishing that even symbolic interactionists do not realize that trajectory, as understood by Anselm Strauss, means “suffering” and disorderly sequences of events like a “cumulative mess.” You can see this in *Time for Dying* and in some later books, like *Social Organization of Medical Work*. The features of trajectory as a peculiar class of social processes I had seen in my autobiographical-narrative interviews, although Anselm during those days (1978/1979) did not approach and envision those features in terms of biography, but in terms of work. However, even in transcripts of actually ongoing interaction you can find trajectory structures, if you are interested in looking at textual materials like that: the traps of misunderstanding, the deadlocks of verbal interaction, the adversely argumentative conflicts escalating turn by turn are verbal marks of trajectories of actually ongoing interaction processes in contrast to the ordering structures of social action.

But, in *Mirrors and Masks*, the book that states the final summary of his first phase of researching in the field of a sociological social psychology (and

this means to a certain degree: on biographical unfolding, too), Anselm was thinking much more on biographical phenomena of metamorphosis than on trajectories of suffering, although, I guess, he did not use this term “metamorphosis” in that book, but, instead, terms like “learning” and “development” that were very much influenced through his intricate studies of George Herbert Mead and following scholars (like his friend Lindesmith). But, it was Anselm who suggested to me the English term “metamorphosis” as appropriate translation for my German term *Wandlung* depicting the processes of creative inner changes in one’s biographical identity development. So, when I worked with Anselm in San Francisco from 1978-1979, my theory of the 4 elementary biographical process structures (biographical action scheme, trajectory, institutional expectation patterns, and metamorphosis) formally expressed by peculiar supra-segmental markers of autobiographical story telling crystallized, and Anselm would encourage that theory and could deal with it well. [Another series of conversations had been encouraging for me before: I had talked to Harvey Sacks when he used to visit us in the first half of the 70s. I can remember that he had a very interesting idea on how to analyze stories in sociological terms, especially addressed to the use of social categorization. He was convinced that it would be quite interesting to connect conversational analysis with the analysis of stories.]

**K.K.:** Could you comment on the origins of the term “biographical work” since, not only in my opinion, in Strauss’ works one may find lots of your ideas in this respect?

**F.S.:** I do not know where it comes from. It could be his idea or my idea, I do not know. (smiling) Lena (Inowlocki) keeps saying that *I* was the inventor of it, but I do not believe it. The term came out of our discussions, and it does not matter where it comes from. You have the use of this term very fruitfully in *Unending Work and Care*. We have a very good German translation of this book by Astrid Hildenbrand, one of the best translations I have ever seen. The book is quite prominent in German health research. First of all, it has to be established – this is very much Anselm-style and the style of the Chicago tradition of sociology in general – that biography is a social phenomenon; biographical processes are social phenomena. This I had discussed with Anselm very much when I had visited San Francisco in 1978/1979 for the first time. As I said, I was there for a whole year. Maybe in former days, before I came to Anselm for the first time, I might have thought that the activities of autobiographical storytelling as such would be just a medium for expressing a certain sort of social reality, but that social processes in their own right would be activated by them (for example, working through and fading out) – this insight came out of the discussions with Anselm. However, the more basic idea was that biographical process structures would be social phenomena depicted by narrative presentation activities. And then, after having realized that narratively expressed biographical process structures would be social processes in their own right, you could think about how the biographical identity subject would start to do work on it; this would be biographical work in a more specific way. So this was the second idea. [More generally, you could come to the conclusion that even autobiographical

storytelling as such would be biographical work already, since it orders the ocean of personal experiences.] The concept of biographical work is not sufficiently developed up to now. We, the biography researchers, must do lots of additional work on this very important list of phenomena. Nevertheless, to sum it up, in the last resort the term “biographical work,” of course, logically came out the taxonomy of work types and work steps as explicated and researched on in Anselm’s magnificent book *Social Organization of Medical Work* published in 1985. And then, Anselm and Juliet Corbin developed this idea and focused on it in the book *Unending Work and Care: Managing Chronic Illness at Home* published in 1988. I do not know who personally got the idea of biographical work first, but probably Anselm. (smiling)

**K.K.:** But, I think that you developed it, and actually, due to his death, he did not have a chance to work on it.

**F.S.:** Anselm always said to me: “Fritz, I do not have this feeling for language, I did not study linguistics, and you have to translate these socio-linguistic concepts for me.” He was very much in the sociology of work and social worlds, and, by then, he had all these young people around him who did work with him on social words and work. But almost nobody would take over this peculiar perspective on biographical processes. Admittedly, Juliet Corbin did this to a certain degree, but she went much more into the direction of methodology. Later, she did not develop the concept of biographical work further in a substantial way. Therefore we, on the old continent, have to do it;

I do not see anybody in America who does research on it today.

**K.K.:** That is what I wanted to ask you: How and why do you think that nowadays in America there is no systematic development of a methodological approach focused on biographical research? American sociology has such a rich potential for it stemming from the Chicago School of sociology.

**F.S.:** It is very difficult to say. It is such an off course for me that I never thought very much about it. There was this Chicago tradition of biographical research, and we still should work on this a lot. I still have a research paper about it in my drawer. We should do some additional analysis and assessment of what was done in terms of biography analysis in the Chicago sociology of the 20s and the 30s. It was much more sophisticated than we assume today. However, the Chicago sociologists did not work on great numbers, and in the end of the 30s, a conference of the “Social Science Research Council’s Committee on Appraisal of Research” took place on the use of biographical materials for studying crucial social problems. The Chicago sociologist, Herbert Blumer, as one of the path finders of a sociological social psychology, gave a very critical assessment of the lack of probative value and of representativity of the Chicago studies with biographical focus, especially, of the monumental study of William Thomas and Florian Znaniecki on the *Polish Peasant in Europe and America*.<sup>35</sup> Herbert Blumer was – I even met him as I also came across Everett Hughes – he was a very sober, science-minded, and fair critical mind, and so he

<sup>35</sup> See: Blumer (1939). See also: Angell (1945).

wrote this assessment of the Chicago style biography analysis in 1939, never published in a book later, and he – mostly correctly – pinpointed lots of shortcomings in Chicago style biography research. Some bit earlier, in the 20s, the Thurstone scale had been developed in psychology and sociology, and it was the opening door for quantitative research using big numbers of informants. American science politics recognized that this new type of quantitative social research would be very worthwhile for assessing the general condition of the society. And therefore, the classical style of Chicago sociology focusing on whole social cases in its integrity and dynamics of unfolding, as well as with its approach of analytically digging deep into the mechanisms of case unfolding would go down. Then, in the end of the 30s and in the 40s, Parsons’s style of sociology would develop with import on the system character of society. At least in the *reception* of Parsons’s approach the structure and system aspect of society and its institutional realms were underlined, although Parsons theory, in addition, was an eminent sociology of interaction; it is very much forgotten, for example, that the famous pattern variables are very much features and categories for research on (professional) action and interaction, and not so much features of any type of social system as a whole.

Then, there was a second offspring of interpretative sociology in the United States in Berkeley and other places in California and in Montana. Even then, the main figure for shaping the large sociology department in Berkeley in the 50s was Herbert Blumer. He did not push symbolic interactionism or phenomenological studies, so it was not some sort of new

establishment of symbolic interactionism, which would have possibly led to a recommencement of biographical research in the U.S. In the 60s, there was the republication of Clifford Shaw's *The Jack Roller* (from 1930) with a very informative new introduction by Howard Becker, but that's it. The person who was closest to the tradition of biographical research was, of course, Anselm Strauss. He would work together with his older colleague Lindesmith in George Herbert Mead's tradition, and they wrote *Social Psychology*, but it was more general theorizing stemming from a Meadean thinking, and not so much empirical research. In the style of Piaget, Anselm did the study on the concepts of money; he did this in the second part of the 40s, and at the very end of this social-psychological development *Mirrors and Masks* in 1959. But then, the Chicago Sociology research tradition of biography research was petering out; with the exception of Howard Becker's *Boys in White*, as far as it can be seen as a biographical career study of medical students, and *Unending Work and Care*, there is nothing that comes after *Mirrors and Masks* in the U.S. Anselm almost was to be thrown out from the University of Indiana; he did not get tenure. And then, at the end of the 50s, with the help of Everett Hughes, Anselm and a group of associated researchers (the sociologists Leonard Schatzman and Rue Bucher, the psychologist Danuta Ehrlich, as well as the psychiatrist Melvin Sabshin) did the study on *Psychiatric Ideologies and Institutions* published 1961. In this trailblazing research the concept of social world and the concept of work started to get prominent. When Anselm was invited by the dean of the School of Nursing in the University of California

at San Francisco,<sup>36</sup> Helen Nahm, to do research in the field of nursing work, he immediately started with these work studies. As I said, biographical research comes back some bit in *Unending Work and Care*, maybe even a little bit influenced by me. It might be, because he had all these discussions with me about the importance of what had been done in *Mirrors and Masks*. I cannot tell. But, there was never an attempt in American sociology to re-establish the tradition of Chicago style biographical research. The younger American sociologists never got this Chicago idea of a sociological type of social psychology, as Anselm Strauss would call it. It did not come to their attention that this could become an important tool for studying society again, as it had been in Chicago between the two World Wars. For Anselm himself, my type of approach to biography research was quite interesting. If he had lived longer, if we had had more time, we would probably have done something together on autobiographies of black people, at least we had planned to do so. Ordered by Anselm, I had already collected a whole bunch of published autobiographies by African Americans.

In this sense, the interest for biographical research finally came back, at least in the work and planning of one of the important heirs of the Chicago tradition. However, even Anselm was more in the research fields of social worlds and professional and scientific work as you can see in research works of Adele Clarke, for example. In the American sociology, biographical processes and biographical developments did not become attractive again as social

<sup>36</sup> The University of California, San Francisco (UCSF) is a center of health sciences research, patient care, and education.

topics worthwhile as such to be empirically studied in sociology, although there had been this magnificent Chicago tradition. In addition, perhaps psychology, as a sister discipline of sociology, got too powerful; it seemed to claim this topic for itself, at least almost everybody assumed such a claim. Even today, if we start to talk about biography, everybody would assume it is a legitimate topic just for psychology. (smiling) [However, most, but not all, of the German psychologists are not interested in biography proper at all; this might be quite different in Anglo-Saxon countries.]

There are some traces of interests to study biography in the research work of the early Garfinkel. Of course, the magnificent study of Agnes in his book *Studies in Ethnomethodology* (1967) is, amongst other aspects, a biographical study in a certain sense; it is a study of biography meticulously done step by step in ethnographical style. These early movements towards biography could have been developed by Garfinkel into an explicit approach of doing biographical research, but it did not happen; he had to follow up other very important traces and developed his intriguing work study approach. Of course in social psychotherapy you have very interesting biography-oriented researchers in Germany, for example, Jörg Frommer, and in Scotland, for example, John McLeod. They go in the direction of biography research on the base of autobiographical statements quite a lot.<sup>37</sup> But, the Anglo-Saxon psychotherapy researchers would probably assume that they get their basic ideas from cognitive science; they normally would not look at continental European traditions.

<sup>37</sup> See, e.g., McLeod and Ballamoutsou (2000).

In Germany, there are traditions of artistic autobiography writing, like the *Anton Reiser* by Karl Philipp Moritz; he published this magnificent piece of autobiographical literature in the late years of the 18<sup>th</sup> century.<sup>38</sup> In addition, he was a specialist of Great Britain; he is most famous for his autobiographical narrative on his visit to Great Britain in, let's say, the 1780s.<sup>39</sup> And, of course, Goethe's *Aus meinem Leben. Dichtung und Wahrheit* (*From my Life: Poetry and Truth*). So, there is this German type of tradition of autobiographical writing, and then we have gotten Dilthey with his realistic hermeneutic analysis of biographical phenomena.<sup>40</sup> So the German tradition of biography research might come from this tradition of artistic literary work and from philosophical and pedagogical analytical description and reflection. In addition, there are students of Max Weber who did some sort of biographical studies, too, which did not become prominent, but at least there was this type of biography-centered research. And finally, even in German psychology there was a time, for example, Charlotte Bühler and Hildegard Hetzer, who would do biographical research, but after the Second World War it died down in main stream German language psychology. You may find lots of biography-centered contemporary studies in Polish, French, or German social-science literature, but, generally speaking, social scientists got used to the idea that everything that is important is written in the English language and you do not have to read French, Polish, or German. (laughter)

<sup>38</sup> The (disguised, since written in „he“ perspective) autobiography *Anton Reiser* was originally published in several partial publications within the time span 1785-1790.

<sup>39</sup> This journey took place in 1882.

<sup>40</sup> See: Dilthey (2002).

**K.K.:** I have one more question related to your stay in the United States: you have collected different materials there. Some we used in our Polish-German or tri-national workshops; they were still typed on a typewriter. Did you have then any systematic project to work on or did you just gather different kinds of interviews?

**F.S.:** I collected some empirical material in the field of hospital treatment, like the autobiographical narrative interview with Mrs. Jackson<sup>41</sup> and some other interviews. In these treatment settings I collected even some recordings of actual ongoing communication. In addition, there is a small corpus on Swiss and French watchmakers. Then there are quite a lot of interviews with American soldiers. There are some interviews with German immigrants to the United States, too. For example, I conducted a long autobiographical-narrative interview with a very close friend of Gerhard who is my friend to some degree, too; he fled from the GDR under very dramatic circumstances and lived in San Francisco illegally since he did not get the U.S. immigration ticket as a refugee from communist dictatorship while he had the West German citizenship, what he did not want, but what was ascribed to him automatically. This interview is more than 100 pages long; it is basically spoken in German, but when he comes in his narrative to his imprisonment, since he was expected to get extradited from the United States, he turns to the English language. We have 3 or 4 of these interviews with German immigrants, too.

**K.K.:** And have you written anything about these watchmakers?

<sup>41</sup> The case of Mrs. Jackson is described, e.g., in the core paper on trajectory of suffering: Riemann and Schütze (1991).

**F.S.:** No, I use it sometimes, but I did not write about it in a focused way.

**K.K.:** Is this material transcribed?

**F.S.:** Some bit is transcribed, but many of the interviews are still on tapes. I bought a special computer program to transform the typewritten interviews into a digital version. Right now I do not have the time to install this program, but I plan to do it. You know, I plan to write a book on biographical processes and biographical work, and for this purpose, I will need these materials. I am very much concerned about the richness of these interview materials. There is another corpus of autobiographical-narrative interviews with Welsh people, which I have not used very much yet. I used it several times in workshops, but I haven't published specifically about it. It is in my thinking, all of this material will be used in the book on biography. Of course, I have to admit an overflow of empirical materials and therefore, I cannot use everything in a very careful way, and some I have to leave out totally. However, this is the situation you have to live in as a qualitative sociologist.

### The Impact of the World War II

**F.S.:** I felt entitled to do real sociological study after I had established the basic theory of biographical processes and the method of analyzing the autobiographical narrative interviews. I started to do research on World War II. When I started to do this, I had no idea that it would have much to do with my own biography, although I had already learned that I could not escape from my existence of be-

ing German. As I told you, I got lots of information from my parents about World War II. I got information that both of them were some bit connected to the Nazi movement. My father was for a while in the student movement of the Nazis, later on he went out of it, but did not separate from it by intention. My mother was not very political, but my grandfather, as a director of a public school, was in the NS party. So both parents were some bit connected to the Nazi culture, whereas Evi's parents were totally against the Nazis. However, both of my parents had told me about the crimes of the Nazi times, and that they hated what Nazi Germany had done. They would bring Jews and other people who had been maltreated by the Nazis in concentration camps to our home. So I had learned about that. At school, Jewish people would come to class and tell us about their suffering in concentration camps, and we had seen all these documentary films and fictitious art films like *Die Brücke* (The Bridge). So I knew all of these crimes of the German nation in Nazi times, but I would see myself more like an earthling. This would stay that way up to the time when I came to America the first time, where I realized that when I even opened my mouth, everybody would know that I was a German who probably had parents who had been in the Nazi movement, and so on.

When I was in San Francisco the first time, I wanted to do some empirical research with Anselm; therefore, as I have told you, I did some interviewing in the community of French and Swiss watchmakers. And I interviewed one of them in his shop in the basement of one of the skyscrapers of San Francisco. When I started my auto-biographical in-

terviewing, he was working on his watches, and at the same time he would tell me his life history. Suddenly, a middle-aged guy came in and said: "Oh you have this extraordinary typical accent! Last week I had to produce a TV show, and then I would have really needed you. In my arranged scenery a young SS officer was playing the piano most elegantly. I would have needed your voice for him." I could manage to let the watchmaker tell his life history, too, amidst the life history of the French-Swiss watchmaker Alain. It came out of this "embedded" autobiographical interview with the TV-man yearning for my thick German accent that he was the son of a Finnish Jew, and his parents had managed to escape from Finland and the Nazi reign. At the very end of his interview, which took more than an hour, he said to me, "Oh Fritz, I tell you, your voice is so soft that you could even be a Swede." (laughter)

**K.K.:** (laughter)

**F.S.:** Later on I understood that when somebody asked me: "Are you from Sweden?" she or he, and everybody else standing by, would immediately know that I am from Germany, but they did not want to make me feel ashamed. So I realized I could not escape my "Germanhood." Perhaps this episode was the first impulse for choosing the research topic of the life histories of persons having been young adults in World War II. Another reason was that both of my parents were dead already; I could not talk with them anymore about their personal experiences in Nazi times. Those might have been the 2 reasons for choosing the war topic, probably it: my conspicuous Germanhood I was not able

to escape from, and that I could not talk with my parents about the Nazi times anymore. Anselm was interested in the war topic, too, but it was not he who suggested to me that I should choose the war topic as a research subject; it was I myself who did this. And then phenomena like fading out of awareness<sup>42</sup> would come up in the interviews. After my change to Kassel University, a lot of this interviewing with persons having been young adults in World War II was done. I had lots of students doing autobiographical narrative interviews on persons having been young adults in World War II, and I did lots of interviews myself.

It was very interesting for Evi, too, because as I said already, her life was very much impacted by World War II. She is some bit older than I, and she can remember the war time as a child very intensely. Her father fell at the Eastern front in summer 1943 when the German army started the last massive attack on the Russian troops at the Kuban arc. Since he was a communist, he was put in the first line of attackers, and he was killed immediately. And Evi's mother criticized Adolf Hitler openly when all the neighbors of their flat in Münster would sit together in the big bomb shelter and were shivering from the heavy bombing. She was imprisoned for a while for *Wehrkraftzersetzung* (undermining the energy for war fighting), but she was soon released because there were some nice women connected to Nazi husbands who managed to get her out. She was a remarkably capable dressmaker, and those women needed a good dressmaker. (laughter)

<sup>42</sup> The phenomenon of fading out of awareness is analyzed, e.g., in Schütze (1992a; 1992b).

Evi's mother got very sick immediately after the end of the war. During the war, because of the heavy bombing of Münster, she and her three daughters were evacuated out of the city of Münster into the rural area. It therefore happened that Evi grew up on a farm, and her second mother tongue was the Munsterlandish, Low-German dialect, which is the German variety most similar to the English language. Evi had to help her mother for 12 years in the household and as a nurse; she could hardly go to school, and I do not know how teachers let her do this: to stay at home and to be the nurse attendant for her mother (what she very much liked to do, but, on the other hand, she loved to go to school and to learn there). So, both of us experienced a very chaotic school education. But, Evi's experiences were more unhappy than mine. Nevertheless, both of us were seriously affected by the war through the fate of our parents. [My mother died very early because of the impact of the war, too.] So, for Evi, it was important, too, that I would deal with the experiences of young adults in World War II and in the Nazi time in general. I never could manage getting stories from convinced Nazis what other researcher accomplished to do. However, today I think it is not that important for me. Now, I try to get at least some informants who would have been closely connected with the DDR state and with Stasi.

**K.K.:** When you were working on the topic of the war, did you conduct interviews in Germany?

**F.S.:** Yes, quite a lot and in America, too. And I am still concerned that there are these beautiful American interviews which have not been used up to now

very much. For example, there is the interview with John Hampshire<sup>43</sup> who is a very famous construction engineer. It is always on my mind. Interviews like that had an important impact on my thinking.

### Relations to Poland

**F.S.:** I still have guilt feelings, I was invited by Antonina Kłoskowska to come to Poland, I guess to Warszawa or to Poznan, I cannot remember exactly.<sup>44</sup> And I did not follow this invitation since I felt Richard (Grathoff), my longstanding friend and organizational partner, would be the *spiritus rector* of this invitation and through this – I was afraid – I would be drawn into new overwhelming responsibilities (which, of course, as I knew later, was not at all the intention of Antonina Kłoskowska, who did not know me at all).

I have to explain this. I had been in this long-standing working cooperation with Richard Grathoff, which I liked very much and from which I had profited a lot in my personal and career development. However, I had one big problem with it: Richard tended to make me organize difficult and time-consuming organizational projects. [This had been some bit similar in my very fruitful relationship to Joachim Matthes.] He was very much concentrated on his very important and world-changing liaison work between various scientific cultures in the social sciences. As I have told you, for German interpretative social sciences, he opened

<sup>43</sup> It is one of the interviews analyzed during Polish-German or tri-national student workshops.

<sup>44</sup> The interviewee was unable to remember the conference he was invited to. Most probably, it was the "Florian Znaniecki Symposium on the Centenary of His Death" in Poznan in December 1982.

the windows to the world; especially import was that through his lovable personality and through his so very warm-hearted attitude to relate to others he laid the ground for many others to establish biographically important new relationships to new significant others they had never dreamt about. But, in my organizational cooperation with him, in which, I have to admit, I sometimes acted in a much too detailed way, the organizational work with and for Richard became too time- and energy-consuming for me. Therefore, I finally felt I had to protect myself against it.

For example, according to Richard's wish – we both worked in the Sociolinguistics Committee of the International Sociological Association, I organized the sociolinguistics section of the World Congress of Sociology in Mexico City in 1982. When I arrived in Mexico City, I was totally exhausted from my preparation of the vastly numerous meetings in the Sociolinguistics section with roughly 500 group sessions. I was the guy who had to organize them in terms of topical differentiation, chairmanship, time schedule, and place. Most of the participants, the majority being linguists and anthropologists, I did not know, and – alas! – I was not able to speak Spanish. Luckily, I got decisive help from Mexican sociolinguists and anthropologists, otherwise I would not have been at all able to prepare the sociolinguistics section of the Sociological World Congress. 2 of my decisive Mexican supporters, Teresa Sierra and Rainer Enrique Hamel, became my personal friends even until today (again, the impact of Richard's liaison work, although in this case unintentionally and indirectly!). In the middle of the World Congress

I got very sick, and I even had to stay longer because I was not able to travel by plane. Through the help of my new Mexican friends the sociolinguistics section of the World Congress of Sociology went very well, but it had been extremely strenuous for me. And Richard was the powerful person who had made me do it – as he had always expected from me to help in other things, too, for example, to write up part of the application to the German Sociological Association in order that we would establish the German Section of Sociology of Language, as I have mentioned before. As I said, he was extremely important for establishing the foundations of interpretative or qualitative sociology in Germany. As I told you, he brought all the American stars, like Strauss, Goffman, Garfinkel, Cicourel, Sacks, Schegloff, Gumperz, and others to Germany and enabled the 3 pivotal conferences in Bielefeld, Gottlieben, and Constance. He was the engine of all this establishing and foundation work. In addition, without him, I would have never met Anselm and Fran Strauss, my dear friends. In addition, I am very grateful to him for lots of personal stimulation and encouragement. However, he was so powerful in his relating to me, that I decided to be careful when he would attempt to engage me again within new activities.

Richard had asked me to invite Marek (Czyżewski) for a conference on “mountain interpretative sociology” I wanted to organize in Kassel University – that strand of interpretative sociology in Montana and Colorado, which was called “Ethnoinquiry” (Edward Rose, Rolf Kjolseth, Charly Kaplan, and others). So I invited Marek for this conference in 1981. And I did this because I understood it would

be good for Marek to get contacts to Western sociologists. However, in-between was this so very exhausting organizational work for and in Mexico City, and I had decided not to get into new projects of liaison work suggested by Richard. I knew that Richard had done tremendous work to help Polish sociology and social philosophy in the times of the Solidarity Movement and in the following period of Marshal Law. He had developed all these new beautiful contacts to Poland. Now, there would be this conference organized by the eminent Antonina Kłoskowska where I, too, was invited. Since I got this invitation through the mediation of Richard Grathoff, at least I had to surmise this, and probably I factually knew this – I was afraid that I was expected to embark on a new very energy-consuming project of discovery, exploration, and establishing new relationships. I was still so exhausted and even sick, and I thought it would be too much. I had to concentrate on my new professorship of qualitative social research, the first position of that kind in West Germany, which I had, by necessity, neglected some bit in favor of the urgent Mexico organization. I remember that later, for another conference in Lodz, Rolf Kjolseth came to our house in Wattenbach near Kassel. He attempted to take me with him to a second Polish conference in Lodz, to which I had been invited, too, and I said, “No, I cannot do this.” Again, I surmised that Richard would be the spiritus rector of my invitation. And I declined again, since I was still afraid of new industrious work coming up.<sup>45</sup> However,

<sup>45</sup> It was the conference in Lodz in 1983, “Approaches to the Study of Face-to-Face Interaction,” organized by Richard and Marek Czyżewski. Harry Hermanns, Hans-Georg Soeffner, Jörg Bergmann, and Rolf Kjolseth also were active participants. Publication of the conference proceedings: Bokszański and Czyżewski (1987).

that is a wonderfully ironic turn of events, when in 1984 I worked in Princeton in the Institute of Advanced Studies for a whole year, I got an invitation to a new Poznan conference in 1985,<sup>46</sup> and then I was in the wrong assumption that this invitation was not triggered by Richard, and so I finally said, “Okay, I can go to it.”

**K.K.:** (laughter)

**F.S.:** So I went to this conference, and I was in Poland then for the first time. Of course, finally I found out that even this invitation to Poland was triggered by Richard. (laughter) And, again, I have to be very grateful to him for this.

**K.K.:** (laughter)

**F.S.:** And at the Poznan conference I met Marek (Czyżewski) for the second time and Krzysztof Konecki (who Marek introduced to me) for the first time. I did not meet Andrzej (Piotrowski) by then. I saw Andrzej for the first time when I came to our first Polish-German research workshop to Lodz in 1986. In Lodz we, the Polish colleagues and Gerhard and I, started immediately with doing our new style of bi-national workshop using the format of a *binationale Forschungswerkstatt* – a bi-national research workshop with students. I cannot remember who had the idea to work in

<sup>46</sup> It was the conference organized by University of Poznan where many prominent scholars using the biographical methods were present. The book published in 1990 on the proceedings of this conference was entitled *Metoda biograficzna w socjologii*. This volume contained Polish translations of articles on biographical method. Up to today it has remained one of the core publications on biographical research in Polish social science literature. It also contains a shortened version of Schütze’s article “Pressure and Guilt” (1992a; 1992b).

this research workshop format. Probably, it was Marek since I would not have dared to suggest something like that for a socio-cultural university context totally unknown to me. But, it was a seminal idea, something like a boundary object in the sense of the ethnography of science, the research on the social worlds, and work activities conducted by the Garfinkel-type of work studies, Anselm-Strauss- and Adele-Clarke-type of social world studies, and the studies of the French actor-network theory.<sup>47</sup> And later on (from 1996 on) this developed into three-national workshops with Polish, Welsh, and German students and do-cent.<sup>48</sup> Today, this is what I love most in being a university teacher. For example, the last student workshop that we had in Lodz in May (2013)<sup>49</sup> made possible the most fruitful processes of mutual learning (including the participating professors) we could imagine: people from two nations with different cultures were in it, we had to deal with the shortcomings of our lingua franca, we had to take into account the different national perspectives peculiarly shaped by different historical experiences and traditions, we had to overcome the astonishment and even embarrassment about systematic differences in the assessment of a certain interview passage, we had to find out the reasons for having these different attitudes for

<sup>47</sup> For these new encounters between symbolic interactionism, ethnomethodological work studies and Latour-type actor-network theory see, e.g., a collection of papers (translated into the German language) from these fruitfully interacting approaches: Mondada and Schütze (2004).

<sup>48</sup> For the underlying idea of these three-national student research workshops cf. part 11 in Schütze (2005). See also: Inowl-ocki, Riemann, and Schütze (2010).

<sup>49</sup> Schütze alludes to our last student research workshop organized in Lodz. The frame of the workshop was typical to all the student workshops. The empirical material came from our ongoing Polish-German project, see footnote 56.

interpretation, et cetera. I feel especially intrigued through my various biographical experiences of having been some sort of mediation worker, in-between worker, liaison worker. [Of course, I see the parallel to Richard.]

Looking back, I am totally sorry that I did not meet Antonina Kłoskowska. She is one of the most important sociologists of the second half of the 20<sup>th</sup> century. I did not realize that she even quoted me. It never came to my mind that I would be important enough for her to be quoted by her. Today, I would love to work together with her. Nevertheless, the work and friendship relationship to the Lodz colleagues – first, Andrzej (Piotrowski), Marek (Czyżewski), Zbigniew Bokszański, and you, still being a PhD student (Kaja Kaźmierska), and later, in addition, Agnieszka Golczyńska-Grondas, Katarzyna Waniek, and others – has been extremely enriching for my biographical development. It is not only enlightening to realize the systematically different perspectives due to different historical experiences and cultural background, we also learned about our common ground; an ultra-stable trust relationship developed.

Looking back, I also feel responsible to mention the influence of my former teacher and chief Joachim Matthes regarding an earlier very positive attitude to Polish sociology. When I was a young student of sociology in 1964, we had in the Sozialforschungsstelle Dortmund of the University of Münster on display a sociological journal by the name *Polish Sociological Bulletin*.<sup>50</sup> Matthes

<sup>50</sup> The English-language quarterly *The Polish Sociological Bulletin* was first published in 1961 by the Polish Sociological As-

sociation. In 1993, the title was changed into *Polish Sociological Review*.  
 used this journal in his teaching courses on the sociology of language and the sociology of religion. Matthes was very interested in the sociology behind the Iron Curtain, since he grew up in Magdeburg and had been imprisoned by the GDR state (or the Soviet forces?) for almost 1 year in the early 50s or the end of the 40s, since he had brought Western newspapers, journals, and books to East Germany. His conspicuous interest in Eastern European sociology might have stemmed from this quite cruel experience. Anyway, I do not know if you can remember that Jan Szczepański was on the editorial board of the *Polish Sociological Bulletin*, and in the teaching courses of Joachim Matthes we had to read articles of Szczepański and Leszek Kołakowski from this journal. [Actually, they were the first sociological texts I had to read in the English language.] These articles were quite impressive. In addition, Matthes visited Poland several times. [He was one of the liaison persons for the foundation of the Polish-German schoolbook commission.] In winter 1968, he went with me for a month to Czechoslovakia, that is, immediately after the Russians had invaded Czechoslovakia. It was an awful, but very deep and interesting experience for me. And suddenly, Matthes cut all these relationships to Poland, Czechoslovakia, and other countries behind the Iron Curtain, I really do not know why, he never talked to me about it carefully. I guess – I have only a very vague remembrance – he was criticized by the Polish state or by some Polish officials to be a CIA agent.

sociation. In 1993, the title was changed into *Polish Sociological Review*.

**K.K.:** Really? (laughter)

**F.S.:** He was very furious about it, and after that he cut all his professional relationships with sociology behind the Iron Curtain. [In this vacuum Richard stepped in and established all the new pivotal relationships between Western interpretative sociologists and Polish ones.] I had always kept in my mind the idea that we, younger German sociologist, would have to start an intensive relationship with Polish sociology because I had heard from Matthes that it was one of the most important sociologies all over the world. Very early on I had heard about Znaniecki from Matthes, for example. And in terms of politics, the relationship to Poland was important for me, too. Evi and I had belonged to the many thousands of people who supported the election campaign of the German Social Democratic Party by visiting the voters and optimizing the election campaign by means of the statistical analysis of the socio-structural character of the living quarters in West German cities. [The question was: who would be still undecided and how they could be won for the social-democratic vote.] Of course, the victor of the Social Democratic Party had much to do with the improvement of the relationship to Poland (the acceptance of the border between Poland and Germany at the rivers Odra and Neisse, Brandt's kneeling down in Warszawa, etc.) So, the relationship to Polish sociology and to the "Polish nation" (if this doesn't sound too melodramatic) was extremely important for me in biographical terms. The only reason I had not started it earlier, was because I thought: "Richard is controlling it, and it's so dangerous for me

**K.K.:** (laughter)

**F.S.:** to be burnt out by this liaison work." So, I did not do it. At the very end, it appears that Richard was responsible for my "finally coming to Poland," and I am very grateful for it now.

## Biographical Analysis – Present Perspective

**K.K.:** You have mentioned that the Foucault type of discourse analysis can be dangerous for biography analysis. Could you explain what you mean by this statement?

**F.S.:** I know about the work and different approaches of discourse analysis, at least since the early 80s, because I was tangentially involved in Mexican research on Otomi Indians. I had to write one of the assessments of the sociolinguistic PhD dissertation of Rainer Enrique Hamel on the bilingual situation of Otomi Indians in the Valle del Mezquital,<sup>51</sup> and one of my PhD students, Michael Appel,<sup>52</sup> did his PhD research on the biographical processes of Otomi Indians – as some sort of counterpart to the language and discourse studies of the Mexican colleagues on the Otomi in the Valle del Mezquital. Teresa Sierra, the other important researcher on the Otomi, wrote her PhD dissertation under the supervision of a French sociolinguist or anthropologist who had connections to Foucault.<sup>53</sup> Therefore, it was a no-brainer for me

<sup>51</sup> See: Hamel (1988 [PhD dissertation on Frankfurt University 1986]). A later important research piece on the discourse and the biographical impact of bilingual situations is: Treichel (2004).

<sup>52</sup> See: Appel (2001).

<sup>53</sup> See: Sierra (1986).

that Rainer, and especially Teresa would use ideas of Foucault's concept of discourse when they were studying the language use and discourse of Otomi Indians. For example, Teresa and Rainer had recorded the speeches of the government representatives when they would address the village community meetings. Of course, the very use of the Spanish language and the manner, in which the state representatives would address the Otomi audiences and give their speeches – none of the officials would use the Otomi language, the everyday vernacular in the villages; most of the village inhabitants had difficulties using the Spanish language fluently – would hint to the power position of the state representatives and to the automatic and effective use of power: the phenomenon which Foucault had addressed so clearly. The research among the Otomi also explicated how the Otomi Indians would overcome the difficulties of being in a discouraging situation of lack of power, when they were barely able to speak the Spanish language. For example, the Otomi women drew on powerful symbolisms – forcing the government men with all their power to be polite and helpful. So I knew that a Foucault-type discourse analysis would be important.

But, during those days in the 80s, there were no rules how discourse analysis could, or even should, proceed by clear-cut methods. In addition, there were several quite different understandings of discourse as a social phenomenon, from casual conversation up to parliamentary debates and/or argumentative exchanges of articles – as those between Sartre and Camus about the historical role of communism and the avant-garde role of

the Soviet Union and its terror. Corollary, discourse could mean just some sort of liberal type of conversation analysis, it could be public opinion analysis, it could be the re-analysis of literary discourse, et cetera; the question of method was totally unclear. In the case of Teresa and Rainer, they would proceed by ethnographic observation and ethnographic description and by the sociolinguistic analysis – a combination of micro-ethnographic conversation analysis and ethnography of speaking – analysis of the transcribed recordings of the public meetings in which power persons would act as local leaders administrators, community mayors, state and federal government administrators, et cetera.

Some bit later I had to do my own work of discourse analysis, and this was on the public discourses about the West-German student revolution in 1968. With two co-workers, I collected newspaper editorials about certain events in the course of the student rebellion. For example, one of these central events was the occupations of the university rector's office building of Freie Universität Berlin. The conservative elite newspaper *Frankfurter Allgemeine Zeitung* wrote about the young guys who stood up and entered the rector's building. In his editorial, the conservative journalistic commentator attempted to "understand" the life situation of the student protestors – that they would have some sort of prolonged adolescent moratorium, and this would bring them to crazy ideas. Of course, his activities – the journalist tacitly insinuated that without being outspoken on the following topic – were adolescent and premature and therefore, society was not really called upon for re-thinking the

wrong developments and the Nazi background in the universities, and in society at large. I undertook the analysis of this article, and of course, I had analyzed lots of others, too. For these analyses of editorials and other articles, I developed some sort of mixture of conversation analysis of my style (focused on the action schemes in verbal interaction), text sort analysis and arc-of-work analysis in the style of Anselm Strauss. This was my type of discourse analysis or action-text analysis addressed to text manifestations of public discourse in order to find out what are the action schemes of the journalists (as actors of and within public discourse) in those textual manifestations (in editorial) and what are their appeals to underlying, "hidden," shared assumptions of society. So, together with the two students, I wrote a 30-page article, and it was published in an important book by the psychoanalyst, Professor Marianne Leuzinger-Bohleber, the director of the Sigmund Freud Institute in Frankfurt and, in addition, a professor in Kassel, a very knowledgeable researcher. The book was on adolescence. However, our article was never discussed at all. It was as if nobody had written it, and, factually, nobody looked at it.<sup>54</sup> This article, which I am still proud of, was quite easy to write for me because, as I told you, I still had some unconscious or subliminal rest of training in me from those days when I had been the sub-assistant of Joachim Matthes and when he had put me and others (from the Catholic Theological Faculty of Münster University) on the task to analyze sermons of catholic bishops. As I told you, during those early days, we did not have clear methodological rules how to accomplish such an analysis, but in 1964,

<sup>54</sup> See: Schütze, Lützen, and Schulmeyer-Herbold (1993).

Matthes did this "somehow" – as a "natural gift" – and extremely successfully in his habilitation thesis on the politico-societal discourses discussing the ideas and blueprints for the vast and complex compound of law regulations for social services decided on in the West-German parliament in 1961. In 1964, Matthes opened up a broad road to discourse analysis as we understand it today, although the methodological concepts were not stated explicitly. Again, this study was never looked at a second time for its achievements in the field of sociology of knowledge, for its analysis of the role of public and hidden discourses in it, and for the question how to manage such a complex discourse analysis. Matthes's 1964 study is very rich, and one day, I would like to demonstrate this in a separate article. Joachim Matthes himself had probably not realized that he had started to unravel such a very important methodological and basic theoretical thread; perhaps in the 70s, he was drawn too much into the direction of structuralistic Marxist sociology in order to realize his own achievement; in addition, his young co-workers – me included – have unintentionally discouraged him to unravel his so very beautiful thread further, since we were so very much impressed by the new developments in the U.S. interpretative sociology. It is a tricky story. However, the teachings of Matthes, the careful reading of his early publications, and the practice of text analysis in scrutinizing the sermons of Catholic bishops were the reasons that it was so easy for me to develop my style of action-text analysis (or discourse analysis).

Later on, René Sternberg, who went for part of his studies to Lodz and learned some Polish, did

a Master thesis here, in Magdeburg, on the EU debate about the weighing of votes for the EU parliament in the advent of the Lisbon conference and the Lisbon Treaties. René had collected important newspaper articles published in Poland and Germany during the Kaczyński days as representative pieces of the fiercely argumentative discussion going on in Polish and German newspapers. He used my method of action-text analysis. René Sternberg's research work developed into an elegant, very cute thesis, and I wrote a quite meticulous assessment of it. And thirdly, one of my daughters, my youngest daughter who is a very good social worker, she did as her diploma thesis in her study course of social pedagogy at the University of Bremen the analysis of the introduction of the topic of Viagra into the German print media, and she used the action-text analysis, too. Thus, at least there are some uses of this method in my personal surrounding. In addition, you know yourself as our consortium leader that we proposed this method in the (finally unsuccessful) applications for two research projects we submitted to the Research Committee of the European Commission in the 7<sup>th</sup> Framework Program.<sup>55</sup> There connected action-text analysis with biography analysis: it allowed the analyzing of the impact of public discourses on autobiographical narratives and life histories and, reversely, the impact of biographical concepts and biographical work on public discourses.

As you know, Foucault never developed a strict method for discourse analysis, but he "practically" did very intriguing discourse analyses, and he

<sup>55</sup> In 2011 and 2013, our international consortium submitted two projects to the 7<sup>th</sup> Frame Program of European Commission.

developed essential basic-theoretical concepts for all sorts of public discourses. Generally speaking, discourse analysis is an important branch of qualitative social science research, both in terms of basic theorizing and methodic approaches. It is not in conflict at all with biography analysis. As you know yourself, when we started to do the FP7 application, it fitted very well: you can analyze discourse in biographies, especially, you can look at the function and impact of discourse in biographical work. And reversely, you can analyze the assumptions regarding prototypical biographies in textual manifestations of public discourse as, for example, in editorials. [And sometimes you can even follow up how a new discourse item emerged through biographical work in a life history – as, for example, the discourse on a school education that takes into account, and fits with, life history and biographical development as in the case of Moritz's *Anton Reiser*.] Half a year ago, in January 2013, we, Martina (Schiebel), Anja (Schröder-Wildhagen), Bärbel (Treichel), Carsten (Detka), Gerhard (Riemann), and I, prepared a bunch of short papers for a shortened research workshop "Triangulation von Biographie und Diskursmaterial: Eine exemplarische Analyse" for a conference in Kassel on biography analysis and discourse analysis. It was the joint annual conferences of the two sections of Biography Research and of the Sociology of Knowledge of the German Sociological Association. Our group used the interview with a woman who also wrote a book about her unsuccessful flight from the GDR. Both in the book and in the interview that impressive lady tells how the idea of the flight developed, how it was planned, how she was captured, how she was imprisoned, how

she was "bought free" by the West German government, and how she (successfully) attempted to bring together her family in West Germany again. In the interview, the autobiographical narrative has a much broader scope, Anja (Schröder-Wildhagen) had conducted the beautiful interview in the context of our joint Polish-German research project. In addition, Martina Schiebel had selected several articles of quality newspapers and journals about the "being free" activities of the West German government in the historical span of more than 20 years in order to represent the development and change of public discourse about this touchy issue. We proceeded with the shortened research workshop (presenting the results of our various lines of research that addressed the different materials and peculiar features of them, to each other and connecting them analytically) very well for 2 or 3 hours, and then we realized, and this was totally unexpected for me, that there was a very critical attitude towards what we did. Since we were seen as biography researchers, nobody ever realized that we had also done our type of discourse analysis or action-text analysis from time to time. It was as if we would not be entitled to do discourse analysis, too. On top of this, it seemed to be illegitimate that we had even attempted to put the two strands of biography analysis and discourse analysis together. Finally, what was especially missing was the melody of Foucault. Of course, I have to introduce to the picture Marek Czyżewski who did his habilitation in Magdeburg with lots of discourse analyses in it, and I learned a lot from it. In addition, in later times, Marek dealt with Foucault's writing quite a lot, too. After that experience at the Kassel con-

ference on biography and discourse, I talked to Marek, since I thought I should consult with him, and he said: "Yes, there is a tendency to see biography research and discourse analysis in some sort of competition or even conflict regarding the position of the leading paradigm in the interpretative or reconstructive social sciences. It might be an important research-political question, which I am not so much interested in. However, I think that in terms of basic theorizing and in terms of basic methodological stances, the potential conflict between the approaches for biography analysis and the approaches for discourse analysis should not happen at all."

The last thing regarding discourse: through a gift of Marek I came across the book *Past Imperfect: French Intellectuals, 1944–1956* by Tony Judt. Judt shows that the 12 years from the liberation of France from Nazi Germany's occupation up to the invasion of the Soviet Union into Hungary, the years from 1944 to 1956, were the "long decade" of very intensive discourse going on in France about the role and legitimacy of the show trials under Soviet control, about the purges (in the Soviet Union and in the satellite countries), about the role of the communist party in the history of mankind, et cetera. Although Judt does not put it in these terms, by looking at these discourses, very much controlled by Jean Paul Sartre, Simone de Beauvoir, by Maurice Merleau-Ponty, on the one hand, and Albert Camus and Francois Mauriac, on the other, we can see that there was no progress propelled by the intensive discourse at all in terms of enlightenment or a surplus of common understanding. No new aspects of observation and assess-

ment would show up, no new insight into all these difficult questions regarding purification (from the Vichy mental demons), the legitimate practice of revenge, the position of legitimate violence in history, the rational development of history, the avant-garde role of the Soviet Union and the communist parties in the course of a progressive history of mankind, et cetera. Judt showed that nothing enlightening came out of these intensive discourse activities. The conclusion is that not all public discourses *by necessity* will elicit, set free and harness a productive epistemic power; not any discourse will add knowledge, such an outcome is not automatic. However, as children of the French enlightenment period, which was so productive in terms of new styles of thinking, sociologists tend to think that way. We should ask: What are the constitutive conditions for epistemic productivity of public discourses and what are the barriers for it? [Some parameters could be found, for example, in features of the structure of social arenas for discourse, in features of the openness or closeness of a historical situation in terms of biographical orientation to the future, in the strength of habits of taking into regard universal moral interaction postulates, et cetera.]

So when I was asked to prepare something for this conference in Kassel on biography and discourse, I realized that I always had the inclination not to treat discourses as processes and mechanisms that would be automatically enlightening in society and history. In addition, I realized that I always had assumed that discourses would not be the most relevant phenomena for ordinary “humble” members of society and even for well-in-

formed citizens in the understanding of Alfred Schütz. Instead, I had always been more interested in the “lower” layers of everyday knowledge, biography, and other more tacit phenomena down to earth (like, for example, elementary schemes of orientation). On the other hand, I also knew that public discourse is shaped by societal power and can produce additional societal power by itself, although this power is not always enlightening or productive in terms of epistemic procedures. [You know, I took part in, and observed all these long discussion processes during the student revolution. Often, the debates were characterized by an interactive escalation mechanism like this: you would take a certain stance and somebody else would find out that even a more radical stance could be taken, thus, a competition got started on who would be the most prolific announcer of radical statements. As soon as the unfolding of such an escalation process got started, discourse in debate sessions of the student revolution had almost nothing anymore to do with questions of real circumstances.] Discourse is not automatically productive in terms of new insights. It can be to the contrary, especially by its embeddedness in power contexts in the sense of Foucault. We should be concerned about the double face of discourse, we should study it closely. Therefore, I decided to take part in the Kassel conference, and even to attempt to establish a potent working group in order to study the complex relationship between biography and discourse. However, I do not think that discourse analysis can be equivalent with the whole of research activities in the realm of sociology of knowledge. Discourse plays an important role in the architecture of layers of societal knowl-

edge. But, there are, too, the powerful elementary phenomena of societal knowledge, which Alfred Schütz, Peter Berger, and Thomas Luckmann were researching.

**K.K.:** What phenomena would you consider to be developed or worked on? You have already mentioned the phenomenon of the fading out of awareness that you should work on, and we talked about biographical work. Are there any other phenomena?

**F.S.:** I will just say what I would like to work on: one central research focus is on the various relationships between biographical identity and collective phenomena. Of course, you can find this topic in the work of Anselm Strauss. In *Continual Permutation of Action*, his last book, you can find it to a certain degree. When I discussed the results of our recent workshop in Lodz with my German colleagues, I said that I had not expected that our small joint research project about the impact of real or state socialism on the life histories of longstanding inhabitants of Poland and East Germany would have such an important basic theoretical impact on our thinking.<sup>56</sup> We have to find out how the collective phenomena are interiorized into, and built in, biographical identity development and biographical work and how, in reverse, they are conditioned by biographical processes. The latter means understanding how these collective phenomena are a little

<sup>56</sup> The mentioned project is: “The People’s Republic of Poland and the German Democratic Republic in memory and biographical experiences of people born between 1945-55. Sociological comparison based on biographical comparison.” The project is conducted by the Department of Sociology of Culture at the University of Lodz in cooperation with a working group in the Institute of Sociology of the University of Magdeburg. It is funded by the Polish-German Science Foundation, Frankfurt/Oder (2012-2014).

rooted in biographical development; without these biographical developments they would not function at all. So even in collective phenomena there is to be observed the imprint of biographical processes and structures, and this insight we have to follow up, too. In addition, there is the phenomenon of collective mental space<sup>57</sup> in contrast to phenomena of collective identities. Collective mental space (for example, in terms of doing continual comparison of life situations and scanning the landscape of opportunities) has an important impact on life history and even on biographical identity unfolding, although, in itself, it is not shaped by an own identity architecture. I gave a long talk last summer in a symposium of the University of Mainz organized by Detlef Garz. It is mostly in German what I have done up to now. But, I would like to do something in English, too. I would like to follow up these questions regarding the relationships between biographical identity and collective phenomena basically in terms of biographical work. How collective phenomena occur in processes of biographical work – basically, this will be my approach.

The second phenomenon, which I think is very important, is the style of autobiographical rendering, the question of different styles and repertoires. This is a much more sociolinguistic question. Perhaps an “orderly” sociologist would not assume that this is sociology at all. But it is very important. My text on biography analysis published in *European Studies on Inequalities and Social Cohesion*,<sup>58</sup> I am

<sup>57</sup> For the concept of mental space see: Schütze and Schröder-Wildhagen (2012) and Schütze et al. (2012a).

<sup>58</sup> See: Schütze (2008a; 2008b). The long article has been translated into the Polish language: Schütze (2012b). See also: Scheme 1 “Cognitive Figures of Autobiographical Extempore Storytelling” in the appendix of the “Rasmus” article by Fritz Schütze in this volume of QSR.

very grateful to Agnieszka Golczyńska-Grondas that it was published, for me, it is the most important statement on biography analysis I have formulated up to now – you will find the topic of the various relationships between the narrator, on the one hand, and the trajectory incumbent and the event carrier in these stories to be told, on the other. It could be a naive relationship, it could be an ironic relationship, or it could be a defensive or legitimizing relationship. Such basic relationships influence the style of autobiographical narration. These complex coincidences should be studied very much.

And then, a third topic of research will be all these phenomena of expression of “unorderly” features and processes of social reality expressed by autobiographical extempore narration. Background constructions are forms of textual expressions of some of these socio-biographical phenomena. They are connected to the fading out of awareness or even to repression. I did not work on these mental and social phenomena very much during the last years. I realized in writing my *Pressure and Guilt* paper that this topic is quite tricky and therefore, I abandoned it for a while, but now I should return to it. Of course, the central textual phenomena for indicating fading out and repression are background constructions. But, it has to be realized that there are social mechanisms of fading out, too, as discussed in my paper on collective trajectory and collective metamorphosis, and they are expressed by other textual features – even by non-thematization. They can be empirically addressed and analyzed only by contextual hints, but how to find them at all? Now, I am most involved within the analysis of the expression of

collective phenomena in autobiographical storytelling. Perhaps from further insights into it will also be gained some advancement of knowledge regarding collective fading out.

**K.K.:** As you wrote: when we do biography analysis, we can choose between two different perspectives – one more macro-sociological and the other more psycho-sociological. From what you have just mentioned, I understand that presently you are more focused on the more macro-sociological perspective?

**F.S.:** Yes, for a while, because in the European identity project and in the Polish-German project, we were forced to deal with these collective phenomena, and we still are very much focused on them. There might be a fourth subject that is important, at least for me. This is how to deal with *written* autobiographies. So I went into attempts to re-analyze some of the documentaries of the Chicago tradition or to look at other autobiographical documentaries. My paper on the Apache Indian is very much in this direction. And I have a long manuscript on the *Anton Reiser*. I never published it. Of course, in extempore storytelling, we have all these nice formal and symptomatic (non-intentional) features of “textual disorder” (like background constructions and split codas) to look at in order to find out about disorders of factual biographical and social experiences. One question, for example, is: Can we find forms of background constructions in literary autobiographies? Sometimes we do, but they are disguised, they are polished or embellished by textual re-formulation. But we do not have para-linguistic features and other symptom-

atic phenomena in published autobiographies, like hesitation phenomena. I addressed these questions in a paper for the annual conference of the DGS Section of Biography research in Hamburg in December 2011.<sup>59</sup> In this paper, I went back to the questions on the use and text (or experiential) validity of written autobiographical documentaries. If you look closely into my analysis of the written autobiographical statement of the Apache Indian Don Decker, you will find traces of that hidden methodology how to utilize written autobiographical statements as data for the analysis of socio-biographical processes (in this case, of cultural and ethnic hybridity).

**K.K.:** I want to ask you about some ethical issues: when we interview a person and transcribe the narrative it is a rule to anonymize it. However, sometimes this is very difficult to do, especially in these days when you have the Internet and you can google a person. Another difficulty is related to the fact that sometimes you cannot cover everything because then you lose the context. So my question is how to deal with it? And the second part of this dilemma, at least for me, is that we receive the life story, and the narrator usually does not know how we will analyze it. Thus, to some extent the interviewee is not aware that he/she would tell us more than he/she intends to tell. How to deal with this problem?

<sup>59</sup> Jahrestagung der Sektion Biographieforschung in der Deutschen Gesellschaft für Soziologie (DGS): Medialisierungsformen des (Auto-)Biographischen und ihre Kommunikationskontexte. December 2 to December 4, 2011. Universität Hamburg. Fakultät für Wirtschafts- und Sozialwissenschaften. The topic of my lecture delivered to this conference: “Autobiographical Documents in der Chicago-Soziologie zwischen wissenschaftlicher Falldokumentation und medialer Darstellungsform: Authentizität und Ironie.”

**F.S.:** The basic experience I have is as follows: interviewees, who have told their life history in an extempore way, normally assess afterward that autobiographical narration gives some order to their life, or at least it adds to it. Autobiographical storytelling as such is some sort of elementary biographical work they are doing, and normally, it has a productive impact on their life situation and their relating towards themselves. I cannot remember any case in which autobiographical narrative interviewing would distort the relationship of a person towards her- or himself or would make life more difficult for her or him, even if her or his life history or part of it had been very difficult. Normally, autobiographical storytelling is something that is very good for the narrator. I do not go that far as Gabriele (Rosenthal) did for a while – and maybe some of the “narrative” psychotherapists would do – in believing that autobiographical storytelling would have the function of some sort of social therapy or psychotherapy in itself. Thus, I do not feel any type of guilt in the conduction of autobiographical narrative interviewing. It is a strange phenomenon that today you have lots of students who tend to think that doing autobiographical narrative interviewing is something that is unethical in itself, that going to a person and letting her tell her or his life history is something that destroys the private sphere of that person. We have to think about where this belief and this guilt feeling come from: perhaps it is the life situation of tglobal society and information society, in which every piece, every feature of your life can be published on the Internet and Facebook, or whatever. We could see in the European

INVITE<sup>60</sup> project on biographical counseling in life situations of vocational rehabilitation, where Agnieszka (Golczyńska-Grondas) was in, that we would have lots of difficulties in Great Britain even to get to informants for narrative interviewing. For example, professional institutions for social services and for health care would not be allowed to give us hints regarding people worthwhile to talk to. It was really difficult: lots of discussion with ethics committees and with the British Psychological Association. I think that informants should decide for themselves if they like to give an autobiographical narrative interview or not. Normally they would do, and if it turns out to be too difficult for them, they realize this in advance. At the latest, they realize it at the very beginning of the interview situation, and then they say, “No, I cannot do it.” This had happened sometimes to me as an interviewer. For example, I wanted to do some research on the veterans of the Vietnam War. So I started to do some interviews in that year when there was this big earthquake in San Francisco. I drove a long way to a veterans’ hospital in Northern California, I went into the hospital and into the room of the person who had initially agreed to the interview – I had phoned him before – and he said: “Now I have realized that it is too difficult for me. I have a bad heart condition, let us not do it.” Something like that happened a few times to me as apresumptive interviewer, but I claim that people would normally know what is going to happen.

<sup>60</sup> The research project “INVITE. New Ways of Biographical Counselling in Vocational Rehabilitative Training” was conducted in the years 2003-2006 under the Leonardo da Vinci program. Its aim was to design *Educational Curriculum* on biographic method application in professional counseling and social work. The main coordinator of the project was the University in Magdeburg, Germany. The team carrying out the project consisted of various university scientists from Austria, Finland, Germany, Poland, Great Britain, and Italy.

Towards the second issue of the upcoming analysis I will do and the interviewee not knowing about its direction: I let the informants realize that I would treat their autobiographical material in a fair way by using Alfred Schütz’s adequacy postulate – I normally do the analysis in a way that I could show it to the interviewee and make sure that she or he would be treated with respect by my analysis and not harmed by it. I do not expect that the interviewee would accept the outcome of my analysis, and she or he should never be forced to say “yes” to it. But, at least she or he should understand how I have arrived at my argumentation for analysis, my general type of analysis, and my conclusions from it. It is not necessary that one agrees with its results but with the general road I took for the analysis: that one understands how I undertook the analysis. However, normally the person who gave you a gift of a narrative of her or his life history would never be interested in looking at the written analysis. It extremely rarely happens that an interviewee wants to see the written outcome of the analysis afterwards. On the other hand, interviewees love to listen to the taped interview. They are very much intrigued in looking at the written interview, although they are sometimes also irritated by the technicalities of transcription and by the fact that it is not transformed into an “orderly” written language.

We have this book on Felix.<sup>61</sup> It is on the empirical base of an interview I did with a good friend. Some day he came to our house because his girlfriend had left him, and he did not know what to do with his life situation and his biographical fu-

<sup>61</sup> See: Koller and Kokemohr (1994).

ture. I said: “You know, you assume that I know a lot about you; however, this is not true and therefore, please tell me your life history.” We conducted some sort of long autobiographical interview. After that, we had the second session, after he and I had listened to the interview. It should be on finding out about his difficulties and about his present potential for development; it should be some sort of “counseling together.” The result from this joint counseling endeavor was the plan for a university study of constructing engineering. He had successfully studied mathematics and physics in a big university of North Rhine – Westphalia up to the half-way examination he had retreated with some other students of that university into the Eastern Hesse woods near the Iron Curtain, since this under-populated stripe of West Germany seemed for him a road “back to the roots and to concrete life circumstances.” He was (and still is) a magnificent mathematician, but this capacity became some “black hole” or object of addiction for him. For example, he wanted to *see* mathematical structures *mentally*, like the Platonian ideas, and this going the whole way back on the road of the incremental abstraction processes in the mental history of European natural sciences was very strenuous for him; it turned out to be too strenuous for him to quit the utilization of any sort of mathematical algorithms that must not be understood and mentally seen in the form of Platonian ideas. On the other hand, he was, and still is, very interested in concrete material and its practical shaping. Anyway, it turned out that studying and becoming a construction engineer was a very good suggestion. After he had lived in a moratorium of late adolescence for several years, he officially cancelled his stud-

ies of physics after the sixth-semester in-between examination, and he started his study course of construction engineering. Today, he is one of the most famous construction engineers dealing with the repair and protection of medieval buildings, like palaces and churches. Some time ago in Magdeburg, he took me on one of the towers of the cathedral pointing out to me all the mistakes they made in the 13<sup>th</sup> century, and he explained to me how to work on these medieval construction mistakes today. In addition, some day when his mother and his father met me, his mother approached me: “How nice. You are the guy who rescued my son from a successful occupational life...!” This assessment, of course, is not totally true, but nevertheless it pinpoints to the power of autobiographical-narrative interviewing in situations of the need for biographical counseling.

Four years after my interview with him, all of a sudden, Felix brought the interview tape to me and said – we had never talked about something like that at all – that I could do research on it. It was rained through after having been forgotten under the rotten roof of a half-ruined old farmer’s house my friend Felix was living in; it was a miracle that we could reproduce it electronically. I never had thought that his story might be used for scientific reasons. But then, I thought it might be of interest as empirical material for educational research. In the pedagogical seminar of the University of Hamburg a quite stable research workshop group was active that the educational science researchers, Rainer Kokemohr, Winfried Marotzi, and Walter Bauer, had with us, the people from Kassel (Gerhard, Harry Hermanns, and I), and with some participants

from Lodz. I asked my friend, the construction engineer, if I could use the tape and he agreed. So we did transcribe the very long interview (as far as I can remember 102 pages of 60 lines each), and then it was made the empirical material for a special workshop. I do not agree with all the post-modern interpretations of the Felix interview in this book; my disagreement is certainly not the case with the interpretation of Andrzej (Piotrowski), Marek (Czyżewski), and Alicja (Rokuszewska-Pawełek).<sup>62</sup> Remarkably, Felix never asked me about what we had done with his interview. He even knows that there is the book, but he never asked me about it. And I did not give it to him out of my own incentive, since he needed to do his own biographical work in the very course of his autobiographical storytelling and in the course of his thinking about it immediately after the interview. Admittedly, he expected me to counsel him immediately after the interview, and this is what I did, but he never needed to read the written analyses. Of course I wrote my own analysis<sup>63</sup> taking into account seriously that whenever Felix would read it, he could at least accept the way how I would have come to my conclusions (although I did not expect that he would accept all of them in content). I guess the fact that most of the former informants never ask for the outcomes of our analyses is the normal thing to occur. Informants prefer to draw their own conclusions from their intensive experiences of telling their life history.

So, for me, the informants are co-workers, they give to us the gift of their life story, and often it is

<sup>62</sup> See: Piotrowski, Czyżewski, and Rokuszewska-Pawełek (1994).

<sup>63</sup> See: Schütze (1994).

very enriching as it is in the case of the life story of Zofia Pająk.<sup>64</sup> You know, this interview conducted by Anja (Schröder-Wildhagen) is very moving for me. It gives social scientists the chance to let us know much more about the general relations between personal identity unfolding and collective phenomena. Insights into these general relational processes and mechanisms are good for even *other* informants in case they would like go get some biographical counsel (what Zofia Pająk certainly does not need at all, but her personal experiences can teach others who are in need of biographical counsel). Of course, it should be made sure that nobody can realize from the analysis and/or the documentation of the interview which person's life history it is. Nevertheless, the masking of the identity of the informant might sometimes be a big technical problem. On the other hand, some persons would not care to get identified, some would even find it helpful (for example, for showing one's own personal example in educational situations), but we certainly are not allowed to reckon on this. Since we would like to draw general conclusions, even if we dig very deeply into single cases, masking should be the strict rule of documenting and analyzing the interview. Otherwise we would drift into the wrong directions of documenting and/or writing biographies of remarkable or even "famous" persons.<sup>65</sup> In the case of Zofia Pająk, the

<sup>64</sup> The interview with Zofia Pająk – a representative of a civil society organization for crossing the national-cultural fences, for reconciliation after World War II, and for mutual national understanding was conducted by Anja Schröder-Wildhagen for the FP7 research project "EuroIdentity." The case has been analyzed in Schütze et al. (2012b).

<sup>65</sup> This is not the case in the artistic books with documentary life-history interviews published by Studs Terkel since he stresses the documentation of the life histories of "humble," ordinary persons. See: Schütze's "Rasmus" article in this volume of QSR.

analysis of her autobiographical rendering shows her beautiful work in theorizing, teaching, social arranging, and establishing good social milieus, she learned this from her own personal experiences under dramatic biographical circumstances, and she did this for the deep mutual understanding between Polish, German, and third-European-country adolescents. So in several cases it should not be a disaster and not too tormenting if the real identity of the informant could be retrieved later. But, we generally should to be painstakingly careful in our masking work in order not to let happen the retrieval of the personal identity of the informant.

Now, I have to come to the third ethical question. I cannot give any general statement about the question of anonymization. I think it is important to do the anonymization in order to protect the informant who opened up by very personal information. As I mentioned already, there are some people who would be interested in letting themselves be known under their full authentic name. Then, the interview acquires a slightly different quality: to a certain degree it loses its deep focus on one's personal biography proper; instead, it tends into the direction of a memoir as a description of interesting encounters, social milieus, social worlds, and episodes of a general humanistic quality. Such a tendency I do not like very much; I even experienced the power of an overwhelming tendency into the memoir direction caused by non-anonymization in this very interview by you with me. But, this changed quality we cannot hinder. Anyway, one cannot give general rules for anonymization. If I take my study on the millers: in this study, I got into some difficulties with the masking and ano-

nymization work because the small country-side handcraft mills (with big water wheels) draw a lot of curiosity and interest from regional and local museums on them. These museums would like to know where these handcraft millers would have the mill. Quite often, they wrote to me or phoned me up inquired where these mills would be situated. I had put one or two "misleading" pseudo-traces into the documentation and the analysis of the interview, and the museums never found out where this peculiar miller would be situated. Naturally, I explained to the museum people why I could not support their totally understandable attempt to localize the mill and the miller. Museums always feel responsible for the very opposite to anonymization: they feel responsible for identification and authentication. I explained to them that the intention of sociological biography study would be in-debts analysis and generalization, and both intentions normally would require anonymization. Otherwise the miller with his complicated life history would be changed and degraded into an object for display in a museum showcase. The museum professionals did understand this, and took off their inquiry. However, the masking is more difficult in other situations. I have just returned from the defense of PhD thesis of one of my PhD students who was part of a German Research Foundation project on high-career couples. Alas! Such career couples can be quite famous, and right now I do not know how to mask some of them, it will be published in our book series ZBBS, but we have to find out how to do the masking. And, of course, if you, for example, change a protestant into a catholic or a male into a female or reverse

**K.K.:** (laughter)

**F.S.:** (laughter) Yes, that really happened, some young researchers did this. (laughter) This we obviously cannot do, since it changes empirical data (most of the social context) that are pivotal as cognitive resources for the analysis. To sum it up: the anonymization work is difficult, but in most cases, if you work very carefully, you can manage. Perhaps I am one of the German professors with the largest number of supervisions of PhD qualitative research projects. We never caused any difficulties to anybody in our field of research. So it is quite often an exaggeration if the anonymization task is stated as a big or even insurmountable problem. You must and can be very imaginable in putting small misleading pseudo-traces into your empirical documentation (which should not change the matrix of empirical data), and then it normally works fine.

**K.K.:** Just to sum up, could you mention the research projects you have been involved in?

**F.S.:** The first inquiry – not a clear-cut research project – that I should mention was that Matthes assigned me to the task of analyzing, together with a group of Catholic theologians, written texts which had some official *gestus* and importance, like sermons. As I told you, during those days we would not come up with a straightforward method for the analysis of written texts. Nevertheless, at least it sensitized me for the analysis of official ceremonial texts in terms of the hidden verbal actions embedded in them. Since Matthes put me in front of this task, he also brought me to my second

dissertation theme on the relationship between language and action. The first had been the topic of invisible religion. I realized that both phenomena – the sermons and the language manifestations of invisible religion – could not be analyzed without the understanding of the basic relationship between language and action. From these insights it seemed to be possible for me to develop interpretative or qualitative methods for the strict analysis of the manifestation of social phenomena: for example, power- and interest-related actions and ideologies, on the one hand, and elementary levels of biographically relevant levels of knowledge and beliefs (as pinpointed to by Thomas Luckmann in his concept of invisible religion), on the other. The second inquiry was that when I was writing my PhD dissertation, I was thinking how to put my basic-theoretical insights into the relationship between social phenomena as actions and frames of social knowledge, on the one hand, and written or spoken language manifestations to empirical work, on the other. I came up with two central topics or ideas mixing basic theoretical questions with methodical strategies.

The first idea for a more concrete inquiry was the fusion of collective identities in the form of the merger of local communities. For this concrete research field (with others together, especially Gerhard Riemann), I developed the data collection method of the narrative interview. [In the beginning, it was not a method of analysis, but it would be expected to become an instrument for analysis.] In the course of attempting to do the first analyses of the narrative interviews on mergers of local communities, I realized – first seen as a difficulty and not a po-

tential for new insights – the biographical imprint in it. I realized that I should look more closely how verbal manifestations of biographical processes would be present within the verbal presentations of collective processes (regarding all the tasks and difficulties of community merger) and therefore, I had to develop the method of the *autobiographical* narrative interviewing and the analysis of the narrative texts, that is, of extempore autobiographical narratives as produced in these interviews. Again, I did this together with others; I would like to mention here Gerhard Riemann, Günther Robert, Ralf Bohnsack, and later on, Thomas Reim. Of course, the fusion of local communities was my first glimpse towards the question of collective phenomena in everyday life and in biographies of members of society and local communities. However, during those early days, I did not realize that it would later become such an important research topic for me. When I spent a year in America, I also realized that I would be an “inescapable” German. I observed that everybody in America was a bit shocked when they heard my German voice. Of course later, in my first visits to Poland, I observed this, too, but you, my emerging Polish friends, like Fran and Anselm Strauss, dealt with it sensitively. According to those personal experiences, it became important for me to do analytically dwell on collective phenomena and the involvement of biography in it, for instance, the article “Pressure and Guilt”<sup>66</sup> is some example of this type of inquiry.

The other topic of inquiry that stems from my PhD thesis is communication under constraint. “Communication under constraint” means that you as

<sup>66</sup> See footnote 42.

a “victim” have to undergo situations of enforced communication: you would not like to communicate at all, but you are institutionally and organizationally enforced to do so. Something like that happens in courtroom interaction or in situations of inquiry into your political biography, when you apply for a position in public administration, in a public school, or in a university institution. [This type of inquiry took place in the 70s up to 90s in West Germany, in case you had been a member of a leftist student organization or in a leftist – “communist” – party, especially those ones connected to the GDR; if you did not pass this inquiry, you would get banned from any type of position in public institutions – of state or communal administration, of schools, of universities, etc.] In many of these communications under constraint, you as a “victim” would be asked to tell your life history in order to demonstrate your loyalty to the state, your authenticity as a regular, law-abiding citizen, your religious attitude against war service, et cetera, and this expected personal communication would take part under conditions under which it would be virtually impossible or totally insensible – in terms of social arrangement controlled by the master of these institutional proceedings – to really tell your life history.

I studied the phenomenon of communication under constraint mainly on the empirical base of recordings and transcriptions of courtroom proceedings for getting acceptance as a conscientious objector.<sup>67</sup> The first instance of decision about an application for the recognition as conscientious objector was a hearing in an administration of-

<sup>67</sup> See: Schütze (1978).

face where the “victim” or plaintiff (technically seen, the presumptuous conscientious objector is not a defendant, but a plaintiff), on the base of his written application, would repeat: “No, out of my unbearable anguish of conscience I am not able to go to the army!” and in front of the administrator he would have to corroborate again and again that he would permanently live under an inner condition of anguish of conscience, and that religious or moral obligation would forbid him to go to war service. In addition, the victim or plaintiff would react to the administrator’s argumentative refutations of his own corroborations and explanations. After that, in case of the negative first decision of the administrator, there would be a second instance of a panel of 3 administrators, some bit higher up within the administrative body, and in case one’s application would not be accepted by the second administrative proceedings, the victim or plaintiff could appeal to the administration court, and in this administrative court an allegedly elaborate “search” for the stated inner event of a conscientious impasse of the plaintiff would be conducted again. This would be mainly done by means of his autobiographical storytelling forced upon him, and then there would be statements by the plaintiff and refutations by the judge, and the final decision would be taken. The central task of the proceedings for the recognition or refutation of conscientious objection is the proof of the plaintiff that such an inner event of moral or religious impasse in terms of conscience had really taken place, and a successful proof of such a proposition would in fact presuppose free autobiographical narration of the plaintiff. To state one of the most important results of my research: such a free autobiographi-

cal narration and other provisions of natural communication for the free expressions of inner events of conscience are situationally not possible in those administrative and legal settings because of the institutional necessity for communication under constraint. When in Munich I had presented my study to an audience of roughly 100 professors of law, they would state at the very end: “Look, these administrative court proceedings for the recognition of conscientious objection are no real legal procedures. They are something extraordinary, an oddity enforced on us, we willy-nilly have to enact them. However, in reality, they are not proceedings of real law at all.” (laughing)

My article was a 100-page long statement with some comments of legal experts put together in a small book, and my research was methodically based on Werner Kallmeyer’s and my peculiar type of action-oriented conversation analysis. My student co-workers and I were again and again thrown out from the courtroom when the judges realized that I would carry on me one or two recording machines Sony TC 55. It was a very heavy, but high-quality forerunner of the later walkman-type audio cassette recorder. And with this Sony TC 55 it always happened that the red button you had pressed down for starting the recording, would spring off at the end of the cassette tape (i.e., after 45 minutes); alas!, the red button that was jumping up made a lot of noise. Thus, the judges realized our hidden recording and – having the power of the room police – threw us out. I wrote lots of applications to the Ministers of Law in several West German states. But, they did not allow me to do official recordings, although courtroom

proceedings are public events open to the eyes and ears of critical observers. I was counseled by a very helpful professor of civic and administrative law, and he said to me: “Do not put these transcripts into the official report of your project, you may get into legal difficulties.” I meticulously worked on transcriptions of the mentioned recordings of courtroom proceeding, but I could not document them for the scientific community, and accordingly, some linguists would criticize me: “He claims the existence of proceedings without any empirical base, it is not a clear-cut empirical analysis.” I was very disappointed by all these difficulties and therefore, I left this field of research. Just 10 years later, the president of the high court in Dusseldorf asked his co-workers: “Where is Schütze gone to?” He wanted to open every courtroom proceedings for my research, but I had to answer to his generous offer: “I cannot do it anymore, now, I am involved within totally different things.”

When I first started to think about empirical research at all, I also had the intention to study the proceedings of labor administration, especially vocational counseling in job centers. In the end of the 60s and in the beginning of the 70s, I tried hard to get a license to observe and record actually ongoing counseling proceedings in job center offices. Several applications to job centers and to the Bundesanstalt für Arbeit (Federal Institution for Work) were declined. And for 40 years up to the times of our “INVITE” project of 2005 and 2006 on biographical counseling in life situations of occupational/vocational rehabilitation, the job centers and institutions for vocational counseling could always manage not to get researched on. Only one

time I partially succeeded because I had one student in Magdeburg working as some sort of substitute worker in a West German job center, and she did open recordings of ongoing vocational counseling; the clients and her boss knew and accepted this. But, even in her case, it was not eventually allowed to officially utilize these transcriptions of vocational counseling in her very cute master thesis. The local labor administration offices, as we know from numerous autobiographical narrative interviews, are one of the most pivotal institutions for biographical planning, vocational decision making, and future management. But, they keep on being able to disguise their big influence on life, including their professional shortcomings.

When in Kassel I got my first professorship in qualitative analysis, the social work department there offered a study course in supervision (including Balint groups). To put it short: experienced professionals (as, for example, social workers, teachers, catholic priests, and protestant ministers, as well as medical doctors) would return to university for a second study in order to conduct a reflected analysis of the ways they do their professional work and of the systematic difficulties involved in it (e.g., dealing with burn-out). I took part in the discourses of the often convened docent panels of this study course on how to analytically look at the action schemes of professional work and what are the essential features of professions, how to analytically and practically deal with the systematic misunderstandings involved within the interaction with clients, how to analytically focus on and practically handle paradoxes of professional action (e.g., on the one hand, pedagogically instructing

the client how to handle certain tasks and, on the other, paralyzing the action competence of the said client by this very instruction at the same time), how to analyze and practically find out about mistakes at work, and how to develop a culture of professional critique and self-critique, et cetera. Especially because of the topics of systematic mistakes at work and of arenas and cultures of their professional critique, the study course on supervision and the accompanying discourse arena of the docents group was very intriguing for me. I was not a psychologist, psychoanalyst, social worker, or supervisor like the other docents and therefore, for them, I was not an insider, but after a while, they accepted my interactionistic-sociological perspective. I was so intrigued that I started to do research on supervision. I even wrote an application to the German Research Foundation, but it was not accepted. Nevertheless, I did my research on supervision as a set of knowledge-generating procedures and as the central place for the “meta-reflexion” of the features and systematic problems of professional work. I got lots of tape recordings of supervision sessions from master supervisors, so I could systematically compare quite a variety of types and styles of supervision. This was written down in some shorter and longer articles on supervision, profession, professional work, professional typification, and especially paradoxes of professional work.<sup>68</sup> In addition, there existed a research project mainly done by supervision students and research assistants, here, I have to name especially Monika Müller and Arnold Otten.

In my research on supervision, I developed the

<sup>68</sup> See, e.g., Schütze (2000; 2002).

idea of knowledge-generating procedures in professional work, and in scientific work, too, which is very important for me today. Certain types of supervision and Balint groups are prototypes of peculiar social arrangements for the establishment and the conduct of knowledge generation procedures – especially, the “oblique” ones using several levels of observation and reflection – including the observation and reflection on one’s own attitudes as professional worker and the involved tendencies for systematic mistakes. With the help of my type of conversation or interaction analysis, I study how these knowledge generating procedures would function, what would be their essential epistemic principles and mechanisms, how their free functioning could be impaired or even hindered – for example, through communication under constraint – and what would be “milieu work” for the establishment and cultivation of social arrangements and social conditions for their support. Right now, together with Carsten Detka, Susanne Kuczyk, and Bärbel Treichel, I conduct research on the pedagogic dimensions of medical work, funded by the German Research Foundation. Central questions of this research project are: how medical doctors generate their knowledge about the life situation of their patients and what is the quality of this knowledge, how they would typify the patients on the base of this knowledge in their encounters with them, especially on their hospital rounds,<sup>69</sup> how they would involve their patients into knowledge-generating procedures through counseling processes, and how they would talk to the patients in the context of these counseling sessions using

<sup>69</sup> See: Schütze (2013).

strategies of teaching, instruction, biographical counseling, supervision-type of impulses for self-search, self-observation, and self-reflection, et cetera. It is obvious that my own involvement into this research project was immensely facilitated by my having been sensitized through my original inquiries into supervision.

I was also interested in the improvement of the case studies in social work. When, for example, the client of social work is an alcohol addict: how, on the empirical base of autobiographical-narrative interviewing, could you, as a professional social worker or therapist for alcohol addicts, find out about the life history and identity development of your client, how could you find out about his involvement within a disastrous biographical trajectory (becoming unemployed, losing one’s partner, etc.), how you could find out from the client’s autobiographical extempore narrative whether alcohol addiction occurred first in the client’s life history or the disastrous biographical trajectory of becoming unemployed and losing one’s bourgeois existence, et cetera. The methodological question is: how can such a biographical inquiry of a professional social worker or social therapist be assisted by qualitative, reconstructive research methods? My long-standing friends and colleagues, Thomas Reim and Gerhard Riemann, are best on the answers to these questions.<sup>70</sup> We always believed that our type of interactionistic qualitative research methods could be really worthwhile for social work case analysis. We thought that on the base of such in-depth case analyses, professional counseling in social work, social therapy, and oth-

<sup>70</sup> See: Reim (1996) and Riemann (2000).

er professions would be much more understanding, circumspect, and far-sighted. Then, the unplanned, “naturally occurring” biographical counseling encounter with my friend Felix happened, which I have told you about. And this, in conjunction with circumspect suggestions and a basic text of Jürgen Voigt, provided the central idea for our European combined research and (meta)counseling project “INVITE” on biographical counseling in situations of vocational rehabilitation. I will not tell the details of this project, but there was systematic training of social workers and psychologists in the professional work fields of vocational/occupation rehabilitation as members of the “INVITE” project for fulfilling the task of conducting autobiographical-narrative interviews with clients and analyzing them. I think the “INVITE” project, circumspectly organized by my former scientific assistant Peter Straus,<sup>71</sup> demonstrated well that qualitative-reconstructive case analysis, by means of biography research, can be very helpful within the processes of professional counseling support for clients in difficult life situations. As I said already, the application for this counseling and research project was not my idea alone; instead, some of my former social work students and now co-researchers would keep saying that the biography analysis and other forms of qualitative case analysis can be fruitfully used in the arcs of work of psycho-social counseling, social work help, and socio-therapy. In addition, as I mentioned already, we had the cooperation of Jürgen Voigt, who was the first researcher in a research project on medical care and counseling for diabetes patients and later in another one on acupuncture (financed by the big

<sup>71</sup> See: Betts et al. (2008).

German health insurance company AOK and both directed by me). Jürgen knew different European and national support programs, and he proposed that we should apply for the “INVITE” project in the framework of the EU Leonardo da Vinci program. In the truly multi-national “INVITE” project, we compared the potential for using biography analysis in counseling processes for vocational rehabilitation in several European countries (Poland, Wales, Finland, Italy, Austria, and Germany), and we studied essential features of arrangements and steps of the arc of work in biographical counseling for situations of vocational rehabilitation. It is very valuable that Peter Straus and others put together a CD of the “lectures” in the project and that Agnieszka (Golczyńska-Grondas) undertook the very circumspect editing work of publishing the research and counseling procedures and the results of the cooperation of professionals and scientists of the “INVITE” project in two volumes.<sup>72</sup>

So, this was my description of the stream of inquiry towards the features and difficulties of professional work and case analysis in my life as sociological researcher. For me, the historical background of case analysis is not only the Chicago tradition with documentary publications, like *The Jack Roller*, edited and analyzed by Clifford Shaw, or the *Urke-Nachalnik*, edited and analyzed by Stanisław Kowalski.<sup>73</sup> There was also the eminent social worker, Mary Richmond, with two books on case analysis, one is *Social Diagnosis* from 1917, the other is even more important – *What is Social Case Work* from 1922. She

<sup>72</sup> Research Project “INVITE. New Ways of Biographical Counselling in Vocational Rehabilitative Training.” See: Golczyńska-Grondas, (ed.). (2008).

<sup>73</sup> See: Kowalski (1933) and Shaw (1966).

established the first and, nevertheless, astoundingly elaborated version of the arc of work of case analysis and case work. Unfortunately, today, it is not taken seriously enough both by social workers and by sociologists. However, Mary Richmond laid the foundations for a “sociology” of single case analysis, and the unique single cases Mary Richmond took into regard can be individual cases or collective cases. Mary Richmond’s work can sensitize sociologists towards the analysis of single cases of all kinds and lines of counseling empirically based on them, it advises sociologists how to put their analytical and theoretical apparatus to practical use. This is a question not very much tackled in the sociology of today, since sociology lost its relationship to professional work when having split from social work in the 20s. Here, I see an interesting connection line between social-work type, interactionistic-ethnographic type, and liberal-positivistic type of praxis-oriented case studies by Mary Richmond, by Kai Erikson (in his Buffalo Creek Flood study as empirical base of a law suit), and by Hans L. Zetterberg (in his study of a Mid-Western museum and its being counseled by him regarding the question of how to improve its attendance) and the respective counseling processes on their empirical base.<sup>74</sup> I had originally mentioned Mary Richmond’s achievement for the establishment of a complex method of sociological and social-work case analysis in an article on social work as a humble profession in 1992.<sup>75</sup> Then, I wrote a long article, together with Gerhard, on the intricacies of her case analysis and case work method in 2011.<sup>76</sup>

<sup>74</sup> See: Zetterberg (1962) and Erikson (1976).

<sup>75</sup> See: Schütze (1992c).

<sup>76</sup> See: Riemann and Schütze (2011).

Of course, what is most important in my research work during the last 15 years is dealing with collective phenomena: in my case, this line of research comes out of both strands of my early inquiry how to use the linguistic expression of social phenomena. My study of collective phenomena and their connection with biography was firstly focused on the war topic. Soon, there were intensive discussions with my Polish colleagues and friends: I thought that we, Germans, would have to deal with the question of a collective feeling of German guilt, and actually, the interviews with Germans having been young adults in World War II clearly showed that in this generation, there existed the empirical fact of a biographical feeling of guilt and responsibility. Later on, I added research on social and inner-psychic mechanisms of fading out (which can be empirically studied with our qualitative and reconstructive methods of biography analysis, contrary to repression in the psychoanalytical sense). You, in Poland, accomplished much more than me, and you concentrated on many aspects of suffering in various sorts of war predicament.<sup>77</sup>

My second research topic of collective phenomena was Welsh collective identity. This was not a circumspect search for a new interesting research topic. It came out of my concern as a university teacher that my new students in Magdeburg, I had changed from Kassel to Magdeburg in 1993, would be so extremely shy in encounters with the “outer world.” Thus, I surmised I would have to use

<sup>77</sup> Schütze alludes here to the research project “Biografia i tożsamość narodowa” (Biography and National Identity) conducted in the Department of Sociology of Culture in 1992-1994, based on autobiographical narrative interviews focused on experiences of World War II (Czyżewski, Piotrowski, and Rokuszewska-Pawełek 1996).

the social arrangement of ethnographic excursion in order to take them out into the world. [Strangely enough, the culture of ethnographic excursions was not institutionalized in West German sociology teaching, except, of course, in departments of “Sociology of Development,” “Latin American” institutes, etc., as far as I got insight into various West German sociological institutes.] I wanted to find for my shy students the strangest cultural context in Europe, in which the English language would be used. And I thought this context could be Wales. And, at first nicely supported by the Magdeburg English Department, this worked fine. We met in Bangor the very supportive, very understanding docents, John Borland and Aled Griffiths. They built bridges into the Welsh culture and society for us, and the students were enchanted by the Welsh social surrounding, and soon they started to interact with this new world for them. As you probably know, from the very beginning we took students from Lodz with us on those excursions in order that the students from three cultures and historical contexts would be very deeply involved in comparisons. For example, the German students were some bit concerned about Welsh nationalism since they had learnt that in European history nationalism became very dangerous. But, the Polish students would step in and would ask: isn’t it the nationalism of a small country and nation? How can it become dangerous? This small society fights for the endurance of its historical, linguistic, and cultural traditions. We, in Poland, know what this means. Please think twice.

Out of these very lively encounters came the idea – very much prodded by John Borland and Aled

Griffiths – to establish a regular format of a tri-national research workshop for students taking place one or two times a year, with changing the locations for the proceedings each time. And for roughly 10 years this became an intriguing offer for our students and for ourselves – the docents from the 3 countries would always be involved in the research work, too – to transgress the cultural borders in doing intensive research work. As you know, we developed five workshop topics: biography analysis, analysis of Welsh culture, analysis of European identity work, analysis of professional work, and case analysis. In all these courses, there were short introductory instructions on the pertinent qualitative research methods, but the real learning of the methods and of their basic-theoretical grounding took place “on the job.” I got lots of help from three students from Magdeburg, later becoming my research or teaching assistants: Michaela Froberg, Carsten Detka, Nick Thräne, and later Anja Schröder-Wildhagen. Later, students of Bamberg and Belfast would join in, too. The regular docents would be: Andrzej (Piotrowski), Marek (Czyżewski), you yourself, Aled Griffiths, John Borland, Graham Day, Howard Davis, Bärbel Treichel, Anja Schröder-Wildhagen, Gerhard Riemann, and I. The tri-national workshops suggested a lot of professional research work. The remarkable habilitation thesis of Bärbel Treichel on suffering when being positioned between two languages came out of it.<sup>78</sup> Our EuroIdentity project took shape with 3 applications to the European research committee; the last application chaired by Robert Miller from Belfast and I<sup>79</sup> was finally successful. Indeed, even our ongoing Polish-German research project

<sup>78</sup> See: Treichel (2004).

<sup>79</sup> See: Miller and Schütze (2011).

on the biographical impact of real or state socialism is an intellectual offspring or our tri-national workshops. Basically, I dare to say: if you look at all this theorizing about the relationships between individual identity development, on the one hand, and social collectivities, on the other, the basic theorizing about it did not develop very much beyond the level of George Herbert Mead and Emile Durkheim during the last 100 years. Of course, there are some exceptions, like Erik Erikson, Kai Erikson, Daniel Bertaux,<sup>80</sup> Antonina Kłoskowska, and Peter Alheit,<sup>81</sup> and the research work in Lodz, your own book on biography and memory included. We can say, taking those new embarkments into account, that there is something new on the way; we sail over a vast ocean to undiscovered lands.

### Instead of Concluding Remarks...

**F.S.:** In giving this interview, it was very important for me to let you know how biography research would become so meaningful for me in very personal terms, although in former days I did not think about it. I never thought that I would study biography as such. The topic was forced on me by others research questions I could not handle well. I do not think that in terms of finished studies I have accomplished anything. On the other hand, I tend to think that in terms of the development of my sort of micro-ethnographic conversation or interaction analysis, of my version of action-text analysis for written documents, and of the method of autobiographical-narrative interviews, I achieved something. It is important for me that I was able to teach some of my

<sup>80</sup> See: Bertaux (2006).

<sup>81</sup> See: Alheit et al. (1999).

students how to utilize these methods for the follow-up of interesting substantive research questions. Especially the tri-national research workshops were a most intensive biographical experience for me as university teacher, although I probably nerved every participant with my so clumsy English.

In my story presented to you, I also wanted to show why it has happened that I became sensible for my perennial “deeper” topics of inquiry. In a certain way, they are the offspring of my complicated biographical experiences: what I have experienced with my father being in the beginning a stranger after his coming back from his long prisoner of war term, and what I encountered during my illness. We did not develop this topic in length in our conversation, but in Germany, I am perhaps more known for my analyses of the professional work than for the autobiographical narrative interview. I did 5 or 6 long articles on the professions, their work, as well as the paradoxes and systematic mistakes of professional work. Naturally, it seems to me, that the topic of professional work has something to do with my quite complicated experiences with medical doctors. Understandingly, I was systematically doubtful towards some of the medical doctors treating me for my bone marrow infection, I did not trust them at all.

Then, a professor of orthopedics in Münster, Oscar Hepp, who helped the Contergan-afflicted children a lot, established a trust relationship with me. He explained to me that every new outbreak of the inflammation would be a chance for his treatment becoming more focused and effective. Such a new move of inflammation I would know much earlier than every-

body else. He was right in his assumption: normally, I would attempt to hide even from my mother that I got fever again. I was able to disguise the fever for a certain while. I would not talk about it and would keep on doing what I wanted to do for a month or so. And only finally, when I broke down, then the new outbreak would be known to my mother and, through her, to him, too. He advised me: “Fritz, if you realize that something new is happening with your leg, please come in immediately and show me what it is.” You know, during those days nothing of all these wonderful picture-giving facilities, like tomography or magnetic resonance, would be around. Therefore, he needed me as a co-worker, and he announced to me: “If you do this, you will probably have 8 or 10 operations or something like that during the next years, but when you reach 18, and your body will not grow anymore, the disease will be gone since we could weaken its stance in your body through exact attacks on the seats and hiding places of the germs in your leg bone. We have this color fluid for marking the inflamed bone. You show me where the inflammation in the bone is, since you feel it. We will inject the marking color fluid exactly in that area of the bone you will have shown to us, and then, after a while, we will see the inflamed areas of the bone since they are now color-marked. And then, we can exactly remove just the inflamed bone and keep the healthy areas.” Oskar Hepp was right, the disease was gone when I was 18 years old. Today, I should be a totally sick person with lots of heart problems, with kidney problems, and what else I do not know (smiling) because it is the most severe infection of the human body by *Staphylococcus aureus*, and I am totally healthy today, everything is okay. It is almost like a miracle. Oskar Hepp did it by his

counseling. Through his sensitive counseling of a 12-year-old boy he taught me a focused type of cooperation as patient, which turned out to be pivotal for success in treatment.

Our medical sociological research project on the pedagogical dimensions of the work of medical doctors, in which I am still deeply involved, certainly comes out of this intensive experience as a child. And more broadly seen, it is the same with my long-standing interest for the professions. I do not have this sort of critical-shouting attitude towards the professions. I admire the professions as one of the most beautiful research objects of sociology, especially in terms of societal evolution, as Parsons has done it, but, on the other hand, I am much more interested, as Everett Hughes was, in the frailty of the professions and their mistakes at work. For me, this is the most interesting phenomenon in sociology – the topic of mistakes, how mistakes are happening. And this comes from my experiences as a child in hospital. I do not want to conjure that you, as a younger social scientist, should permanently reflect on your biographical experiences as potential for creative topics of research. You cannot develop master plans towards biographically intriguing, and therefore creative, research projects. But, you should have a “listening” attitude towards your biographical experiences. And in cases you really hear something in your biographical experiences, you might consider searching for a reason of that call, and, possibly, you will find a new topic to be followed up.

**K.K.:** And perhaps this is one of the reasons you had such a good relationship with Anselm Strauss who also had health problems?

**F.S.:** Yeah, sure, we both knew what chronic illness is, and exactly in this sense he accepted me as a knowledgeable person. (laughter) Yes, and I could tell lots of nice stories of our walks of our *spazieren gehen*, we loved to do this. Anselm always wanted to study bakery shops, he wanted to find out about the different quality of the bakery products of the various nations present in multi-cultural San Francisco. It was a sociological puzzle for him that French cuisine is very fine, but, as he claimed, the French culture was not able to bring forth a social world of good cookies. When Evi and I visited him with our 3 daughters, he would like to take us on a *Spaziergang* to the various national bakery sub-cultures. We would check the cookies in different quarters of the city with different bakery subcultures, we would visit the Italian bakeries, the Polish bakeries, the French bakeries, the Russian bakeries, the German bakeries, the Mexican bakeries in order to find out what would be the best cookies. Our small assessment panel found out that the best cookies were always the Italian ones. Since San Francisco is not Chicago, there was not so much of a Polish population around. Therefore, the Polish question had to be kept out of the assessment procedure. Of course, German and Russian cookies are very similar. Our daughters kept saying that the Russian ones would be the second best. However, since Anselm had had this very nice Jewish German grandmother from Hanau, he would put the German cookies in the second position. Astonishingly, as he wondered, the Mexican cookies had to be put in the fourth position, and my family members agreed unanimously. On the ladder of assessment nothing else would show up for a long time, and only then others would come, including the French ones. Anselm did not understand this embarrassing outcome of the assessment procedure: where would be

the social world explanation for such an unexpected French “falling through?” In addition, he could not explain why all these lesser cultivated national social worlds of cuisine could manage to produce good cookies, why even the Mexican cuisine would be able to produce good cookies, even if, throughout its history, Mexico had been invaded by the U.S.-American forces more than 150 times. By the way, Anselm was the only U.S.-American I met who knew this high number of the U.S. attacks on Mexican territory exactly. (laughter)

**K.K.:** And what about the American cookies?

**F.S.:** He did not like them so much.

**K.K.:** But why not the French ones? I am astonished.

## References

- Albert, Hans. 1965. “Die Einheit der Sozialwissenschaften.” Pp. 53-70 in *Logik der Sozialwissenschaften*, edited by E. Topitsch. Cologne: Kiepenheuer & Witsch.
- Alheit, Peter et al. 1999. *Gebrochene Modernisierung – Der langsame Wandel proletarischer Milieus. Eine empirische Vergleichsstudie ost- und westdeutscher Arbeitermilieus in den 1950er Jahren*. Bremen: Donat-Verlag.
- Angell, Robert. 1945. “A Critical Review of the Development of the Personal Document Method in Sociology.” Pp. 177-232 in *The Use of Personal Documents in History, Anthropology and Sociology*, edited by L. Gottschalk, C. Kluckhohn, R. Angell. New York: Social Science Research Council.

**F.S.:** Yes, French cookies are not very good. Ask my daughters, they will tell you. (laughter)

**K.K.:** But, you know, it is not an objective sociological study.

**F.S.:** You know, it was a panel of counselors with *Urteilkraft* (faculty of judgment). It was a group assessment, and I really think that French cookies are not that good, and in fact, the Mexicans are really good. (laughter) We had established a whole list of graded national cultures of cookies. And Anselm never understood the serendipity pattern of the French and Mexican cookies. (laughter)

**K.K.:** Thank you very much for this extremely interesting and inspiring talk.

- Appel, Michael. 2001. *Indianische Lokalkultur und gesellschaftlicher Wandel in Mexiko. Autobiographisch-narrative Interview mit Kulturmittlern der Otomi*. Opladen: Leske and Budrich.

- Bertaux, Daniel. 2006. *Le Récit de Vie*. 2<sup>nd</sup> ed. Paris: Armand Colin.

- Betts, Sandra et al. 2008. “Biographical Counseling: An Introduction.” Pp. 5-58 in *European Studies on Inequalities and Social Cohesion*, edited by A. Golczyńska-Grondas. Lodz: Wydawnictwo UŁ.

- Blumer, Herbert. 1939. *An Appraisal of Thomas' and Znaniecki's 'The Polish Peasant in Europe and America' (Critiques of Research in the Social Sciences I)*. New York: Social Science Research Council.

- Bohnsack, Ralf. 1973. *Handlungskompetenz und Jugendkriminalität*. Berlin: Luchterhand.
- Bohnsack, Ralf. 2003. *Rekonstruktive Sozialforschung. Einführung in qualitative Methoden*. 5<sup>th</sup> ed. Opladen: Leske and Budrich.
- Bokszański, Zbigniew and Marek Czyżewski, (eds.). 1987. *Folia Sociologica 13: Approaches To The Study Of Face-To-Face Interaction*. Lodz: Wydawnictwo Uniwersytetu Łódzkiego.
- Czyżewski, Marek. 2005. *Öffentliche Kommunikation und Rechtsextremismus*. Lodz: Wydawnictwo Uniwersytetu Łódzkiego.
- Czyżewski, Marek, Andrzej Piotrowski, and Alicja Rokuszewska-Pawełek. 1996. *Biografia a tożsamość narodowa*. Lodz: Wydawnictwo Uniwersytetu Łódzkiego.
- Dilthey, Wilhelm. 2002. *The Formation of the Historical World in the Human Sciences. Selected Works, Vol III*. Princeton, Oxford: Princeton University Press.
- Erikson, Kai T. 1976. *Everything in its Path. Destruction of Community in the Buffalo Creek Flood*. New York: Simon & Schuster.
- Garfinkel, Harold. 1967. *Studies in Ethnomethodology*. Englewood Cliffs: NJ: Prentice-Hall.
- Golczyńska-Grondas, Agnieszka, (ed.). 2008. *European Studies on Inequalities and Social Cohesion*, 1/2 and 3/4. Lodz: Łódź University Press.
- Grathoff, Richard H. 1970. *The Structure of Social Inconsistencies*. The Hauge: Martinus Nijhoff.
- Gumperz, John J. 1994. "Sprachliche Variabilität in interaktionsanalytischer Perspektive." Pp. 611-639 in *Kommunikation in der Stadt. Teil 1: Exemplarische Analysen des Sprachverhaltens in Mannheim*, edited by K. Werner. Berlin, New York: Walter de Gruyter.
- Habermas, Jürgen. 1970. *Zur Logik der Sozialwissenschaften*. Frankfurt: Suhrkamp.
- Hamel, Rainer E. 1988. *Sprachkonflikt und Sprachverdrängung in der verbalen Interaktion. Die zweisprachige Kommunikationspraxis der Otomi-Indianer in Mexico*. Bern: Peter Lang.
- Hildenbrand, Bruno. 1983. *Alltag und Krankheit. Ethnographie einer Familie*. Stuttgart: Klett-Cotta.
- Hughes, Everett C. 1972. "The Linguistic Division of Labor in Industrial and Urban Societies." Pp. 296-309 in *Advances in the Sociology of Language, Vol. II: Selected Studies and Applications*, edited by J. A. Fishman. The Hague, Paris: Mouton.
- Kallmeyer, Werner. 1988. "Konversationsanalytische Beschreibung." Pp. 1095-1108 in *Sociolinguistics/Soziolinguistik. Ein internationales Handbuch zur Wissenschaft von Sprache und Gesellschaft*, edited by U. Ammon, N. Dittmar, K. L. Mattheier. Berlin, New York: Walter de Gruyter.
- Kallmeyer, Werner, (ed.). 1994. *Kommunikation in der Stadt. Teil 1: Exemplarische Analysen des Sprachverhaltens in Mannheim*. Berlin, New York: Walter de Gruyter.
- Kallmeyer, Werner, (ed.). 1995. *Kommunikation in der Stadt. Teil 2: Ethnographien von Mannheimer Stadtteilen*. Berlin, New York: Walter de Gruyter.
- Kallmeyer, Werner. 2002. "Verbal practices of perspective grounding." Pp. 113-141 in *Perspective and Perspectivation in Discourse*, edited by C. F. Graumann, W. Kallmeyer. Amsterdam, Philadelphia: John Benjamins Publishing.
- Kallmeyer, Werner and Fritz Schütze. 1976. "Konversationsanalyse." *Studium der Linguistik* 1(1):1-28.
- Kallmeyer, Werner and Fritz Schütze. 1977. "Zur Konstitution von Kommunikationsschemata. Dargestellt am Beispiel von Erzählungen und Beschreibungen." Pp. 159-274 in *Gesprächsanalysen*, edited by D. Wegner. Hamburg: Buske.
- Kallmeyer, Werner and Inken Keim, (eds.). 1995. *Kommunikation in der Stadt. Teil 3: Kommunikative Stilistik einer sozialen Welt „Kleiner Leute“ in der Mannheimer Innenstadt*. Berlin, New York: Walter de Gruyter.
- Kallmeyer, Werner and Johannes Schwitalla, (eds.). 1995. *Kommunikation in der Stadt. Teil 4: Kommunikative Stilistik von zwei Sozialwelten in Mannheim-Vogelstang*. Berlin, New York: Walter de Gruyter.
- Kallmeyer, Werner and Inken Keim. 1996. "Divergent perspectives and social style in conflict talk." *Folia Linguistica* 30:271-298.
- Kallmeyer, Werner and Jürgen Streeck. 2001. "Interaction by inscription." *Journal of Pragmatics* 33:465-490.
- Kallmeyer, Werner and Inken Keim. 2003. "Linguistic variation and the construction of social identity in a German-Turkish Setting. A case study of an immigrant youth group in Mannheim, Germany." Pp. 29-46 in *Discourse Constructions and Youth Identities*, edited by J. Androutsopoulos, A. Georgakopoulou. Amsterdam, Philadelphia: John Benjamins Publishing.
- Koller, Hans-Christoph and Rainer Kokemohr, (eds.). 1994. *Biographie als Text. Zur biographischen Artikulation problematischer Bildungsprozesse*. Weinheim: Deutscher Studienverlag.
- Luckmann, Thomas. 1967. *Invisible Religion*. New York: Macmillan.
- Matthes, Joachim. 1964. *Gesellschaftspolitische Konzeptionen im Sozialhilferecht. Zur soziologischen Kritik der neuen deutschen Sozialhilfegesetzgebung 1961*. Stuttgart: Enke.
- McLeod, John and Sophia Balamoutsou. 2000. "Narrative Processes in the Assimilation of a Problematic Experience: Qualitative Analysis of a Single Case." *ZBBS* 1(2):283-202.
- Miller, Robert and Fritz Schütze. 2011. "The Evolution of European Identity: Using Biographical Methods to study the Development of European Identity." *Przegląd Socjologiczny* 60(1):9-40.
- Miller, Robert and Graham Gray. 2012. *The Evolution of European Identities. Biographical Approaches*. Houndmills, Basingstoke, Hampshire: Palgrave Macmillan.
- Piotrowski, Andrzej, Marek Czyżewski, and Alicja Rokuszewska-Pawełek. 1994. "Deciphering 'Felix.'" Pp. 61-78 in *Biographie als Text. Zur biographischen Artikulation problematischer Bildungsprozesse*, edited by H. C. Koller, K. Rainer. Weinheim: Deutscher Studienverlag.
- Reim, Thomas. 1996. "Die Weiterbildung zum Sozialtherapeutenberuf. Bedeutsamkeit und Folgen für Biographie, professionelle Identität und Berufspraxis. Eine empirische Untersuchung von Professionalisierungstendenzen auf der Basis narrativ-autobiographischer Interviews." PhD dissertation, Fachbereich Sozialwesen der Universität Gesamthochschule Kassel, Kassel.
- Riemann, Gerhard. 1987. *Das Fremdwerden der eigenen Biographie. Narrative Interviews mit psychiatrischen Patienten*. Munich: Fink. ["Biographieverläufe psychiatrischer Patienten aus soziologischer Sicht: eine empirische Untersuchung." PhD dissertation, Universität Kassel (1983)].
- Riemann, Gerhard. 2000. *Die Arbeit in der sozialpädagogischen Familienberatung. Interaktionsprozesse in einem Handlungsfeld sozialer Arbeit*. Weinheim, Munich: Juventa.
- Riemann, Gerhard. 2003. "A Joint Project Against the Backdrop of a Research Tradition: An Introduction to 'Doing Biographical Research.'" *Forum Qualitative Social Research* 4(3). Retrieved January 15, 2004 (<http://www.qualitative-research.net/index.php/fqs/article/view/666/1441>).
- Riemann, Gerhard and Fritz Schütze. 2011. "Die soziologische Komplexität der Fallanalyse von Mary Richmond." Pp. 131-201 in *Forschungstraditionen der Sozialen Arbeit*, edited by K. Bromberg, W. Hoff, I. Miethe. Opladen: Budrich.
- Schelsky, Helmut. 1959. *Ortsbestimmung der deutschen Soziologie*. Düsseldorf: Diederichs.
- Schütze, Fritz. 1975a. *Sprache soziologisch gesehen. Vol. 1, Strategien sprachbezogenen Denkens innerhalb und im Umkreis der Soziologie*. Munich: Fink.
- Schütze, Fritz. 1975b. *Sprache soziologisch gesehen. Vol. 2, Sprache als Indikator für egalitäre und nicht-egalitäre Sozialbeziehungen*. Munich: Fink.
- Schütze, Fritz. 1978. "Strategische Interaktion im Verwaltungsgericht – eine soziolinguistische Analyse zum Kommunikationsverlauf im Verfahren zur Anerkennung als Wehrdienstverweigerer." Pp. 67-156 in *Schriften der Vereinigung für Rechtssoziologie, Vol 2: Interaktion vor Gericht*, edited by W. Hassemer, W. Hoffmann-Riem, M. Weis. Baden-Baden: Nomos.
- Schütze, Fritz. 1981. "Prozessstrukturen des Lebensablaufs." Pp. 67-156 in *Biographie in handlungswissenschaftlicher Perspektive*, edited by J. Matthes, A. Pfeifenberger, M. Stosberg. Nuremberg: Verlag der Nürnberger Forschungsvereinigung.

Schütze, Fritz. 1984. "Kognitive Figuren des autobiographischen Stegreiferzählens." Pp. 78-117 in *Biographie und soziale Wirklichkeit. Neue Beiträge und Forschungsperspektiven*, edited by M. Kohli et al. Stuttgart: Metzler.

Schütze, Fritz. 1987. *Das narrative Interview in Interaktionsfeldstudien, Kurseinheit 1: Merkmale von Alltagserzählungen und was wir mit ihrer Hilfe erkennen können*. The Hagen: Fernuniversität.

Schütze, Fritz. 1991. "Biographieanalyse eines Müllerlebens – Innovationsbereitschaft als Familientradition und Lebensführungshabitus: Wie die Müllerfamilie Berger die Krisen des Mühlensterbens um die Jahrhundertwende und in den Fünfziger Jahren überwunden hat." Pp. 206-227 in *Wasser- und Windmühlen in Kurhessen und Waldeck-Pyrmont*, edited by H. Scholz. Kaufungen: Axel Eibel.

Schütze, Fritz. 1992a. "Pressure and Guilt: War Experiences of a Young German Soldier and their Biographical Implications (Part I)." *International Sociology* 7(2):187-208.

Schütze, Fritz. 1992b. "Pressure and Guilt: War Experiences of a Young German Soldier and Their Biographical Implications (Part II)." *International Sociology* 7(3):347-367.

Schütze, Fritz. 1992c. "Sozialarbeit als 'bescheidene' Profession." Pp. 132-170 in *Erziehen als Profession. Zur Logik professionellen Handelns in pädagogischen Feldern*, edited by B. Dewe, W. Ferchhoff, F. Radtke. Opladen: Leske and Budrich.

Schütze, Fritz. 1994. "Das Paradoxe in 'Felix' Leben als Ausdruck eines 'wilden' Wandlungsprozesses." Pp. 13-60 in *Biographie als Text*, edited by H. Koller, R. Kokemohr. Weinheim: Deutscher Studien Verlag.

Schütze, Fritz. 2000. "Schwierigkeiten bei der Arbeit und Paradoxien des professionellen Handelns. Ein grundlagentheoretischer Aufriß." *Zeitschrift für qualitative Bildungs-, Beratungs- und Sozialforschung* 1(1):49-96.

Schütze, Fritz. 2005. "Eine sehr persönlich generalisierte Sicht auf qualitative Forschung." *Zeitschrift für qualitative Bildungs-, Beratungs- und Sozialforschung* 6(2):211-248.

Schütze, Fritz. 2008a. "Biography Analysis on the Empirical Base of Autobiographical Narratives: How to Analyse

Autobiographical Narrative Interviews." Pp. 153- 242 in *European Studies on Inequalities and Social Cohesion 1/2 (part I)*, edited by A. Golczyńska-Grondas. ([http://www.profit.uni.lodz.pl/pub/dok/6ca34cbaf07ece58cbd1b4f24371c8c8/European\\_Studies\\_2008\\_vol\\_1.pdf](http://www.profit.uni.lodz.pl/pub/dok/6ca34cbaf07ece58cbd1b4f24371c8c8/European_Studies_2008_vol_1.pdf)).

Schütze, Fritz. 2008b. "Biography Analysis on the Empirical Base of Autobiographical Narratives: How to Analyse Autobiographical Narrative Interviews." Pp. 6-77 in *European Studies on Inequalities and Social Cohesion 3/4 (part II)*, edited by A. Golczyńska-Gronda. ([http://www.profit.uni.lodz.pl/pub/dok/6ca34cbaf07ece58cbd1b4f24371c8c8/European\\_Studies\\_2008\\_vol\\_2.pdf](http://www.profit.uni.lodz.pl/pub/dok/6ca34cbaf07ece58cbd1b4f24371c8c8/European_Studies_2008_vol_2.pdf)).

Schütze, Fritz. 2009. "Gegen die Reifizierung des Gesellschaftlichen und die Emigration der Soziologie aus der Gesellschaft: Zum Tod von Joachim Matthes." *Biographieforschung Sektion der Deutschen Gesellschaft für Soziologie* 56:18-31. ([http://www.sociologie.de/fileadmin/user\\_upload/Sektion\\_Biographieforschung/Rundbrief\\_56.pdf](http://www.sociologie.de/fileadmin/user_upload/Sektion_Biographieforschung/Rundbrief_56.pdf)).

Schütze, Fritz. 2012a. "Don Decker's Apache Odyssey." Pp. 159-242 in *Approaches to Autobiography, Narrative and the Developing Self*, edited by G. Bartelt, B. Treichel. Berlin: Frank & Timme.

Schütze, Fritz. 2012b. "Analiza biograficzna ugruntowana empirycznie w autobiograficznym wywiadzie narracyjnym. Jak analizować autobiograficzne wywiady narracyjne." Pp. 141-278 in *Metoda biograficzna w socjologii. Antologia tekstów*, edited by K. Kaźmierska. Cracow: Nomos.

Schütze, Fritz. 2013. "Alltägliche Kategorisierungs-, Typisierungs- und Klassifikationstätigkeit der Ärzte als abgekürzte professionelle Erkenntnis- und Vermittlungszuwendung." Pp. 227-290 in *Der soziale Körper*, edited by H. Herzberg, A. Seltrecht. Opladen: Budrich.

Schütze, Fritz. 2014. "Autobiographical Accounts of War Experiences. An Outline for The Analysis of Topically Focused Autobiographical Texts – Using the Example of the "Robert Rasmus" Account in Studs Terkel's Book, "The Good War."" *Qualitative Sociology Review* 10(1):224-283.

Schütze, Fritz and Gerhard Riemann. 1991. "'Trajectory' as a Basic Concept for Analyzing Suffering and Disorderly Social Processes." Pp. 333-357 in *Social Organization and*

*Social Structure. Essays in Honour of Anselm Strauss*, edited by D. Maines. New York: Aldine de Gruyter.

Schütze, Fritz and Lorenza Mondada. 2004. "Soziale Interaktion und die Herstellung von Wissenschaft: Einführung in den Themenschwerpunkt." *Zeitschrift für qualitative Bildungs-, Beratungs- und Sozialforschung* 5(2):139-154.

Schütze, Fritz and Anja Schröder-Wildhagen. 2012. "European Mental Space and its Biographical Relevance." Pp. 255-278 in *The Evolution of European Identities. Biographical Approaches*, edited by R. Miller, G. Day. Houndmills: Palgrave MacMillan.

Schütze, Fritz, Claudia Lützen, and Ulrike Schulmeyer-Herbold. 1993. "Unterschiede in der Berichterstattung der FR und der FAZ zu studentischen Anliegen 1967/68 und 1989/90 – Eine qualitative Auswertung." Pp. 300-341 in *Phantasie und Realität in der Spätadoleszenz. Gesellschaftliche Veränderungen und Entwicklungsprozesse bei Studierenden*, edited by M. Leuzinger-Bohleber, E. Mahler. Opladen: Westdeutscher Verlag.

Schütze, Fritz, Lena Inowlocki, and Gerhard Riemann. 2010. "Das forschende Lernen in der Biographieforschung – europäische Erfahrungen." *Zeitschrift für qualitative Forschung* 11(2):183-195.

Schütze, Fritz et al. 1973. "Grundlagentheoretische Voraussetzungen methodisch kontrollierten Fremdverstehens." Pp. 433-495 in *Arbeitsgruppe Bielefelder Soziologen, Alltagswissen, Interaktion und gesellschaftliche Wirklichkeit, Vol. 2: Ethnotheorie und Ethnographie des Sprechens*. Reinbek: Rowohlt.

Schütze, Fritz et al. 2012a. Discoverers in European Mental Space: The Biographical Experiences of Participants in European Civil Society Organizations." Pp. 150-169 in *The Evolution of European Identities. Biographical Approaches*, edited by R. Miller, G. Day. Houndmills: Palgrave MacMillan.

Kaźmierska, Kaja. 2014. "An Interview with Professor Fritz Schütze: Biography and Contribution to Interpretative Sociology." *Qualitative Sociology Review* 10(1):284-359. Retrieved Month, Year ([http://www.qualitativesociologyreview.org/ENG/archive\\_eng.php](http://www.qualitativesociologyreview.org/ENG/archive_eng.php)).

Schütze, Fritz et al. 2012b. "Policy Suggestions Regarding Support of the Work of European Civil Society Organizations." *Przegląd Socjologiczny* 61(4):9-76.

Shaw, Clifford. 1966. *The Jack-Roller. A Delinquent Boy's Own Story. With a new introduction by Howard S. Becker*. Chicago, London: University of Chicago Press.

Sierra, Maria T. 1986. "Pratiques discursives et relations de pouvoir chez les paysans Otomi de la Valée de Mesquital." PhD dissertation, Université de Paris VIII, Paris.

Strauss, Anselm L. 1987. *Qualitative Analysis for Social Scientists*. Cambridge: Cambridge University Press.

Strauss, Anselm L. 1993. *Continual Permutation of Action*. New York: Transaction Publishers.

Strauss, Anselm L. and Juliet Corbin. 1988. *Unending work and care: managing chronic illness at home*. San Francisco: Jossey-Bass.

Strauss, Anselm et al. 1985. *Social Organization of Medical Work*. Chicago, London: University of Chicago Press.

Topitsch, Ernst. 1965. "Sprachlogische Probleme der sozialwissenschaftlichen Theoriebildung." In *Logik der Sozialwissenschaften*, edited by E. Topitsch. Cologne: Kiepenheuer & Witsch.

Treichel, Bäbel. 2004. *Identitätsarbeit, Sprachbiographien und Mehrsprachigkeit. Autobiographisch-narrative Interviews mit Walisern zur sprachlichen Figuration von Identität und Gesellschaft*. Frankfurt: Peter Lang.

Urke-Nachalnik. 1933. *Życiorys własny przestępcy*, edited by S. Kowalski. Poznan: Patronat.

Włodarek, Jan and Marek Ziółkowski. 1990. *Metoda biograficzna w socjologii*. Warszawa: PWN.

Zetterberg, Hans L. 1962. *Social Theory and Social Practice*. New York: The Bedminster Press.