

An Introduction to “Doing Biographical Research”

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Abstract: The purpose of this paper is twofold: It reconstructs the development of a joint endeavor of a group of social scientists trying to make their specific approaches of doing biographical research visible by focusing on one particular autobiographical narrative interview: the interview with a female labor migrant from Turkey living in a German city. The data were collected in a student research project of the late Christa HOFFMANN-RIEM. The product of this endeavor is this issue of “Forum: Qualitative Social Research”—a preliminary product since readers are invited to take part in the analysis of this interview, the transcription of which is made available to them (in the original German version and an English translation as well) and to offer their interpretations and critical comments in the future. In addition to this the reconstruction of this endeavor is placed in the context of research connected with Fritz SCHÜTZE, which initially led to the development of this type of interview—the narrative interview. Although today this type of interview is widely used it seems necessary to remind people of its history in order to avoid an undue and narrow focus on “method” or “technique.” The paper emphasizes the linkage of theoretical and methodological concerns at the beginning of this development, the decision to methodically utilize off-hand-narratives of self-lived experiences for sociological field research and the turn to autobiographical narrative interviews and their sequential analysis which

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proved to be fruitful with regard to the discovery of “structural processes” of the life course. Thus, this research tradition contributed to the emergence and the present shape of biographical research in the social sciences.

1. The Idea of this Project

During the last two decades interest in biographical research in the social sciences has continually grown (CHAMBERLAYNE, BORNAT, & WENGRAF, 2000). The study of life histories based upon different kinds of biographical materials—the “holistic” attempt to discover and to document how radical social changes have been experienced and understood by members of contemporary societies and how they have penetrated and shaped their life circumstances and life courses—has, of course, an important place in the history of sociology. Just think of the heyday of early Chicago sociology in the 1920s and 1930s when the unorthodox use of autobiographies, letters, diaries and other “personal documents” created an awareness of the complexities of modern life histories, milieus and social worlds. For many decades, however, such a project had fallen into disrepute, as it was viewed as diffuse and unscientific, as something belonging to an early age of the social sciences. This has drastically changed as can be seen in the attraction of the “biography sessions” during international sociological conferences.

Of course, there are many different sources for this upsurge of interest. One source is the widespread interest in biographical research among social scientists from the former Soviet Union and the Baltic States who are attempting to reconstruct how people make sense of what has been happening in the last decade and how they work to prevent their lives from falling apart. Many social scientists are attracted to this project because they assume that biographical research is especially promising with regard to bridging the traditional gap between “micro” and “macro.” It would be difficult here to go into detail about all the work and the methodological and substantive concerns which are subsumed under the wide label of “biographical research.” This research also tends to transcend the conventional disciplinary boundaries between sociologists, anthropologists, historians, psychologists, linguists¹, and social scientists working in the field of education studies, and it has important implications for the work of professionals with their clients, e.g., in social work². Biographical

¹ Cf. the work of Rita FRANCESCHINI on biographical processes of language acquisition and on language biographies which she refers to in her article in this issue. Bärbel TREICHEL, another contributor, has done research on language biographies in Wales.

² The idea is that professional practitioners who are expected to make sense of puzzling lives, to listen to difficult stories and to base far-reaching decisions on their inferences can become better listeners and more careful and responsible analysts by acquiring research skills

research is a good example of what the Gulbenkian Commission for the Restructuring of the Social Sciences (WALLERSTEIN et al., 1996) observed as a new and promising development which has surpassed traditional disciplinary boundaries.

Despite the fascination and the widespread use of biographical materials in the social sciences there is still a great deal of insecurity with regard to how researchers actually proceed when analyzing their data: texts which often appear chaotic and diffuse and are not as neatly packaged as supposedly unequivocal closed questionnaires. Such data cannot easily be “managed” by standardized analytical procedures which most social scientists had been trained in. It happens quite often that analytical operations used in working on biographical materials are hidden in publications and are just alluded to and subsumed under programmatic statements. Readers are at a loss for further specifications about how the authors really work on the data, how they gain substantive insights and arrive at theoretical conclusions. This is partly due to the conventional requirements of publications and to their closed format which prevent interested readers from overcoming their passivity and from entering into an exchange with the authors with regard to their actual practices of *doing* their work. It is also true that when researchers get together at conferences the regular format of such meetings (typically with enormous time pressure) usually encourages a summary presentation of “*findings*” whereas the process of “*finding the findings*” does not become visible. In such a situation there are risks of misunderstanding (APITZSCH & INOWLOCKI, 2000), misrepresentation and even self-misunderstanding: It would be much better if colleagues who do not share your presuppositions and routine ways of looking at things could help you—just by watching you at work, by wondering aloud and by asking supposedly naive questions—to look at yourself and to find out what you are really doing and what you are up to.

That was the reason why I suggested a change of the regular format of conference sessions and coordinated a “data session” or research workshop in the stream of the Research Committee “Biography and Society” at the 14th World Congress of Sociology in Montréal in July 1998: a session which we deliberately called “Doing Biographical Research.” The choice of the present participle was meant to stress that what is actually occurring in biographical research should become visible and should thereby become a matter of open discussion and self-reflection: i.e., of becoming aware of the specific presuppositions, blind spots and features of one’s own approach by seeing it in the light of the work of others. I got in touch with other researchers I knew who had a special interest in working on biographical materials and asked them quite generally to demonstrate how they approach and understand the same text which I sent to

in biographical analysis and ethnographic work. This assumption is shared by a growing number of social scientists working and teaching in professional schools (like schools of social work).

all of them: my English translation of the detailed transcription of an autobiographical narrative interview with a Turkish migrant woman, a “guest worker” (“Gastarbeiterin”), living in Germany; the interview had been conducted in German.³ (Those colleagues who were familiar with the German language also received the original transcription.) When I contacted these colleagues I did not have in mind that they had to be experts in the field of labor migration (some of them had worked in this area, others not), their interest in the text and their willingness to cooperate in this little project were sufficient. It appeared to me to be especially interesting that we had some variation in terms of closeness vs. distance to this particular subject-matter. This interview had been conducted in 1986—in a research project on the biographical experiences of migrant women in Germany which had been directed by the late Christa HOFFMANN-RIEM, a professor of sociology at the University of Hamburg, who closely collaborated in this research with students who attended her empirical seminar and participated in the fieldwork. She did not live long enough to finish this project, just smaller parts of her study could be published after her premature death in 1990, at the age of 52 (HOFFMANN-RIEM, 1994, pp.256-351).⁴ Thus, working on these data was also a way of continuing an important research project which had been cut short.

Of course, there are quite different types of data which are valuable materials for biographical research—depending on the particular analytical interests of the researcher. It would be a misunderstanding to reduce this type of research to the exclusive use of interviews. Very interesting qualitative projects utilize different types of data for the purpose of triangulating perspectives and in order to discover the relationships between biographies, milieus, and specific

³ I had originally translated this interview for Anselm STRAUSS who was interested in this text and wanted to analyze it himself; he refers to the narrative in his last basic theoretical book which appeared three years before his death (STRAUSS, 1993, p.67): “Recently, when looking at an interview from a study by the late Christa Hoffmann-Riem with a Turkish woman who had migrated to Germany at the age of seventeen, I could clearly see aggregate features and their consequences in her narrative. This immigrant rarely refers to any group membership other than her family back home, except for temporary groups of women living and working at the same factory. Most of the concepts that I formed when reading the narrative pertained to the woman’s individual experiences, some doubtless shared by other Turkish women in like circumstances. For instance, concepts of body failure seemed appropriate in some of her experiences, because the endless working hours of hard labor resulted in her failing energy and frequent and long bouts of illness. When coding such a narrative, one would also take note of her perpetual confusion in encountering the maze of governmental rules and regulations, beginning with her attempts to leave Turkey and including struggling with conditions stemming from ignorance or only partial knowledge of German regulations and laws. Throughout the narrative various in vivo concepts are scattered that relate to her conceptions about the impersonality of the Germans, to her sense of intense isolation, but nevertheless also to her motivations for staying in Germany as she becomes increasingly estranged from Turkey and her previously Turkish identity.”

⁴ After her death the transcriptions of these interviews were given to me by her husband, Professor Wolfgang HOFFMANN-RIEM. Christa HOFFMANN-RIEM was my sister.

social interactions. I deliberately chose this text for several reasons. One reason was I felt that this interview provided unique insights into the life history of a migrant woman, into peculiarities of the Turkish and German societal realities that she encounters and into processes of labor migration in general. I assumed—and I still do—that studying this text sensitizes social scientists to the heuristic possibilities of biographical research in gaining important insights into the workings of contemporary societies and the relationship between biographical and collective social processes.

The translation of the transcription was also made available to the large audience in this session in Montréal; before that it had been sent to colleagues who had expressed their interest in attending the session.⁵ Because of the openness and positive reaction of the colleagues partaking in the “data session” and because of the lively discussion with the audience, e.g., on the relationship and tensions between the use of ethnographic and cultural background knowledge and formal textual analyses of narratives, I felt encouraged to plan a publication on “*Doing Biographical Research*” based on the papers that had been presented in Montréal. Four of the papers that had been prepared for the congress—the papers by Kaja KAZMIERSKA, by Setsuo MIZUNO, Baerbel TREICHEL and Birgit SCHWELLING, and by Neval GUELTEKIN, Lena INOWLOCKI, and Helma LUTZ—were revised for this publication, two articles—those written by Rita FRANCESCHINI and Fritz SCHÜTZE—are not based on previous papers. It was important to me that the interview should also become part of this publication, so readers could become familiar with this text themselves and could have a base for a critical reading of the articles. In Montréal I was asked by colleagues in the audience if they could share this interview with their students and work with it. Of course I told them they could. And I would be happy if the text which appears in the Appendix (in German and English) will also be used by readers in order to engage (together with students and colleagues) in biographical research. I strongly advise readers to first get familiar with this interview before reading the analyses by the authors and the critical commentaries. In this way the discussion which started in Montréal will continue: Everyone who knows the text will somehow take part in an ongoing and egalitarian dialogue. *FQS* appears to be especially well suited to provide a space for an open continuation of such a dialogue.

Some readers may react critically to the fact that this interview was conducted in German. Is it not something very dubious to make a migrant worker talk about very personal matters of her life history in a language which is *not her own*? Is this not again an instance of an asymmetrical situation which is typical of the hierarchical relationships with which she is so familiar? Of course, the interview would have developed quite differently if it had been conducted in Turkish because of the mutual assumptions about the respective

⁵ All names of individuals and places had already been changed in the original transcription, so it is not possible to identify any persons.

background knowledge of the interaction partners. I would argue, however, that such a situation of having to explicate numerous things because they cannot be taken for granted by the interaction partners has many interesting features, too. Georg SIMMEL remarked on some qualities of such encounters in his famous “excursus on the stranger” which was first published in his “Sociology” in 1908 (SIMMEL, 1992, pp.764-771). But a much more important point is the following: The language which the interviewee is using in the interview is *her language*, too. I do not want to elaborate on this point since I assume that readers will have a chance to form their own opinion on this issue while reading this text. Finding this illuminating in this regard, I just want to quote a well-known writer and actress, Emine Sevgi ÖZDAMAR, who had originally gone to Germany as a “guest worker” (at the age of nineteen). When she was asked in an interview at Keele University in 1994 how she came to write in German (HORROCKS & KOLINSKY, 1996, p.47), she answered:

I was also attracted to German as a new language. You see, at that time, I often travelled back to Turkey by train, finding myself together with Greeks, Yugoslavs, Turks and Bulgarians, all migrant workers. Their common language was German. They would sing love songs and then try to translate them from their own language into German. They made mistakes, of course, but the German they spoke was devoid of clichés, and came out almost like poetry as they struggled to express the images of their mother tongues in this new language. And this, as I now realised, was the language of some five million *Gastarbeiter*. If I wanted to write a play about their experience, and I did, I knew it would have to be written in this new language.

A few words on the issue of translation: I know that there are legitimate questions as to the translatability of such an extempore interview presentation—questions which should be asked and will be asked. I am convinced though that it is possible and makes sense for researchers to focus on the English text, found in the Appendix in order to demonstrate how they tackle the problem of understanding the story which the woman is telling. Translating the interview from German into English meant trying to make this story available to a larger group of people participating in the discourse on biographical research, but, of course, such a translation into the most widely used language in the social sciences has its drawbacks, too. The translation itself is nothing more, but also nothing less, than a first attempt to understand the narrative and to put this down in writing. At one point I stopped my constant silent negotiating with myself and had the feeling that I had done a rather good job and could say “that’s it,” but I also knew that I would continue to make little changes here and there again if I decided to examine the text anew.

I had a foretaste of this when I was confronted with the textual analyses of a number of the contributors to this issue. The following is an example of the process of partially revising the translation. Rita FRANCESCHINI deals with a sequence (in the original transcript: page 32; line 1328 in the HTML version) in which the interviewee says, “Ich hab nich richtig deutsch gelernt ...” I dis-

covered that I no longer agreed with my first translation at this point and corrected it by writing down, “I didn’t learn German correctly,” but then I finally settled with “I didn’t learn German in the right way” (cf. line 748 in the English version). Maybe some of the readers who are familiar with German will disagree with me, but I think this translation makes sense if you take the context into account: The interviewee emphasizes that she acquired her German in *ways* which do not have anything to do with a formal school setting (“the right way”). “I didn’t learn German correctly” would have stressed the *outcome*: the non-achievement of a “correct” mastery of the language—and this is not what this sequence is about. “Ich hab nich richtig deutsch gelernt” can suggest both interpretations, and it is quite plausible that the interviewee *also* had in mind that she did not use German “correctly” (in a normative sense). But I did not find an English expression which could express this vagueness and so I chose the one interpretation which definitely fits into this context.

This is the translation of an interview with a woman who is talking about herself in a language which she acquired as an adult. I translated the text into a language which is not my first language either. This made me feel uneasy once in a while.⁶ But I also think that the translation does justice to the original transcription because the speaker, a 31 year old Turkish woman living in a German city, is extremely articulate in her use of the German language. There were no sequences during which I asked myself what she could have meant. There are some features of her oral presentation which show that she is not a native speaker,⁷ but this does not create any difficulties for communication between interviewee and interviewers and for understanding the transcription.

In translating the interview I did not engage in any systematic artificial exercise of reproducing or imitating her minor syntactical “mistakes”—“mistakes” in a normative sense. This would have been an impossible task because of the syntactical differences between German and English and such a forced imitation would have also created an uncomfortable distance between myself as a native speaker (constantly discovering, evaluating and mimicking minor “mistakes”) and the woman whose impressive story I wanted to make available to other social scientists who do not know German. But I also had to constantly negotiate with myself how I could do justice to her specific presentation and communicative style without polishing it for the sake of creating a “nice” sounding translation. In the end, I think I can live with the result.

If you turn to the interview in the Appendix you will notice that I did not correct the speaker’s mixing of tenses (where she “should” have stayed with

⁶ I had been exposed to American English when I lived with a family in Northern California for a year as a foreign exchange student when I was sixteen and seventeen. Later I spent a year as a research scholar at the University of California at San Francisco. But I still wondered again and again if what I wrote in translating might sound too clumsy.

⁷ Rita FRANCESCHINI refers to some features of the narrator’s German in her contribution to this issue.

one tense), and I did not get rid of self-interruptions, anacoluthons, new starts and hesitation markers (“eh”) in order to convey the qualities of spoken language and off-the-cuff story telling. Such features are typical of any oral extempore presentations and do not have anything to do with the fact that the speaker in this case uses a language which she had not acquired as a child. As will become apparent in several contributions to this issue (cf. e.g., the articles by TREICHEL & SCHWELLING and by SCHÜTZE), these features serve as important resources for textual analyses, and attempts at polishing would have destroyed the very base for doing such analyses. Of course, the placement of the hesitation markers cannot be exactly like in the German text because of the syntactical differences between German and English, but I paid attention to them nevertheless.

In some respects my translation simplified student researcher Heike KAHLERT’s German transcription: If you turn to the text in the Appendix you will find that I left out the interviewers’ frequent “uh”s and similar sounds. If an interviewer asks a question, makes a comment or joins in laughter, I *italicized* this (in a simple bracket). So a reader can distinguish between the narrator’s and the two interviewers’ statements quite easily. In the original transcription no distinction was made between the utterances of either interviewer. Sometimes I use a simple bracket because I added a word or a few words in order to make sure that the meaning gets communicated; a simple literal translation would not have made enough sense in such a context. Sometimes simple brackets indicate that I am not totally sure of the translation. I use double brackets for indicating paralinguistic phenomena in the interviewee’s speech (mostly laughter in her case) and for adding a short commentary on my translation occasionally. Short pauses are indicated by two dots, somewhat longer ones by three or four dots.

I translated the whole transcription of the interview: the main introductory narrative and the subsequent part of questions and answers. In editing the text I deviated from the common way of presenting transcriptions: I formed paragraphs for the sake of the readability of the text, i.e., I identified narrative segments, but I also made paragraphs which I do not regard as narrative segments in the strict sense of the term. Subsequently, when the interviewee is arguing with herself, I also tried to take the dynamics of her unfolding argumentation into account by constructing paragraphs.

I have already alluded to some features of this interview. If you read the text and are not familiar with this type of interview you might wonder at the lack of further questions after the interviewers have initially asked their interviewee to tell her life history. Are there no questions until the end of her narrative? There are *no questions in between*—even though, in retrospect, the initial request of the interviewers does not appear favorable in terms of generating extempore narratives of whole life histories: Oftentimes formulations like “something about your life” and the listing of single items for a narrative presentation add

up to a difficult task for the story teller—there is too much to remember if she or he wants to be cooperative. But in this case—after a somewhat rough start—the interviewee really turns back to the memory of her childhood days and her story unfolds up to the present: until (and beyond) the narrative coda (on line 843 in the English version): “Well, that’s my story. That’s the whole life” (cf. lines 1497-1499 or page 36 in the PDF file for the German transcription).

2. A Line of Research: The Development of an Interest in the Methodical Usage of Narratives in Sociological Field Research, the Emergence of the Narrative Interview and its Impact on Biographical Research

Hülya, the 31 year old Turkish woman, told her life history to two students, Christa NOACK and Heike KAHLEERT, who participated in Professor HOFFMANN-RIEM’s empirical seminar. Hülya did so in the situation of a so-called narrative interview which she and the student researchers had arranged; the informal field notes of the students which you will find in the Appendix show how the interviewee and the interviewers cooperated in preparing the situation. Because of its widespread use in biographical research many social scientists tend to equate a “narrative interview” with an “autobiographical narrative interview,” i.e., an interview which explicitly focuses on the life history of the interviewee or at least parts of it. Such an assumption ignores the history of the method and the fact that narrative interviews have been used for handling quite different research problems in the study of social processes, e.g., the reconstruction of the experience of collective processes (see below) and the study of long-range work and interaction histories.⁸ In the following I would like to give a short overview over the development of this method and the theoretical and methodological concerns which have to be taken into account in this context, i.e., I will turn to the research project in which this kind of interviewing emerged and in which the term “narrative interview” was coined. I think it is important to keep in mind that theoretical and methodological concerns have been constantly intertwined in this kind of work.

⁸ Cf. my study on the counseling work of social workers (RIEMANN, 2000) which is partly based on spontaneous narratives of the professionals about the long-range history which they share with certain clients. The generative request was “Tell me your history *with ...*” instead of “Tell me a case *of ...*” in order to encourage a narrative which would reveal all kinds of personal involvement and inner states in the work with clients (and to avoid a polished expert presentation). These data are very rich and provide insights into the practitioners’ “arc of work” (STRAUSS, FAGERHAUGH, SUCZEK, & WIENER, 1985, pp.30-39) and the way they deal with basic recurring problems or even paradoxes in their work.

When trying to reconstruct this line of research one has to take into account the situation in West German sociology at the beginning of the 1970s: Many younger researchers and students had become critical of mainstream positivist research (under the influence of the “debate on positivism” and the student movement of 1968). Quite important in fostering such a mood were HABERMAS’ “Zur Logik der Sozialwissenschaften” (1970) for stressing the relevance of language in social research and for introducing German readers to American traditions of interpretive sociology on the one hand and CICOUREL’s critical explication of the common sense assumptions inherent in conducting social research (CICOUREL, 1964) on the other hand. But for quite some time this criticism had little or no influence on the development of new methods of empirical research.

A group which was especially interested in introducing interpretive approaches in the social sciences in Germany as well as in developing new qualitative research procedures was a team of younger sociologists at the University of Bielefeld. One of them was Fritz SCHÜTZE, at that time an assistant professor in the Department of Sociology. While working on his dissertation on “Sprache soziologisch gesehen” [“Language from a sociological perspective”] which was published in 1975, SCHÜTZE had developed a strong interest in different interpretive approaches in the social sciences like Symbolic Interactionism, Ethnomethodology, Ethnography of Communication and Cognitive Anthropology. He was also fascinated by certain social phenomena which were difficult to discover in conventional social research—phenomena like “invisible religion” (LUCKMANN, 1967) or “non-decisions,” a concept coined by BACHRACH and BARATZ (1970). He sensed that these methodological difficulties had something to do with the fact that sociological theorizing proceeded quite separately from a concrete observation of social phenomena and that there was something basically wrong with the relationship between sociological theory and sociological data, that there was “no transparent, controllable relationship of translation.”⁹ SCHÜTZE criticized the dominant arrangements for collecting data like standardized interviews for two reasons: “On the one hand the relationship between the interviewees’ utterances and their reality of action remained obscure, on the other hand there were no reliable rules for translating these utterances and verbal data into theory,” even if this did not appear as a problem in the usual coding procedures. SCHÜTZE formulated his problem in the following way: “Is it possible to get the problem of translating and the problem of relating utterances to real life circumstances”—something which he calls “pragmatische Brechung”—“under control by choosing forms of research communication which are as simple and ‘pure’ as possible?” That meant for him forms of research communication which would avoid the traps of standardized interviews, e.g., the uncontrollable and systematically irritating

⁹ These and the other quotes in this paragraph are taken from one of his unpublished memos. I cite and translate with his permission.

mix of different schemes of communication (narration, description, argumentation)—a difficulty which he would later call “Schemasalat” [“schema salad”] (1987, p.256). He argued that standardized interviews were experienced as something strange by the interviewees, as something which did not have anything to do with their everyday communication and forced them into a passive role.

In this context SCHÜTZE developed a strong interest in exploring the *methodical* use of extempore narratives—something which ordinary members of society are skillful at—in order to pursue research problems which had remained a puzzle for mainstream sociological research and in order to uncover domains of social reality which one could not grasp with standardized interviewing. This interest differed from the traditional use of and reliance on off-the-cuff story telling in interpretive sociology: Of course, Chicago sociologists and later generations of Symbolic Interactionists and ethnographically minded social scientists in general had routinely elicited extempore narratives from their informants and had made use of these stories in their research accounts, but most of them had done so in a more or less intuitive way paying no systematic attention to their formal features. Unlike Symbolic Interactionists, ethnomethodological conversational analysts (like Harvey SACKS) had turned to the study of conversational organization and issues such as how a speaker gets the floor to tell a story (SACKS, 1972). But of course they had no interest in using such insights into formal features to develop new research methods for tackling sociological problems which appeared “traditional” from their perspective. SCHÜTZE’s interest in narratives meant merging different interpretive traditions: While he stayed interested in substantive matters—i.e., in *what* people had to tell—, he was also interested in formal features of their narratives—in *how* they told their story. The idea was that (a) by systematically focusing on the *how* one could arrive at a deeper understanding of the *what*: the long-range experiences of the narrators and the social processes which they had been involved in and that (b) by doing so one could make one’s own forms of understanding explicit and intersubjectively controllable. I think that such a creative merging of different traditions was easier in a setting which did not have much to do with the animosities and misunderstandings which had developed among American representatives of the different interpretive approaches at that time. There was no need to take sides. One was free to appreciate the specific contributions and analytical resources of these traditions to discover what they still had in common and to make use of it (Arbeitsgruppe Bielefelder Soziologen 1973).

Against this backdrop SCHÜTZE began with a research project on community power structures in which narrative interviews were used for the first time. Since another student, Christine BRUEHNE, and I were drawn into this project as his research assistants, I still have quite vivid memories of the development of this style of collecting data. What became known as “narrative interview”

(SCHÜTZE 1977) is the product of a process of preparing, doing and thinking aloud about many interviews and thereby observing ourselves, gaining insights into certain mechanisms of this particular type of communication and gradually refining interview strategies. The development and refinement of this type of interviewing was not an arm-chair invention.

The research focused on communities—villages and small towns—that were in a prolonged state of crisis due to large scale bureaucratically enforced regional reforms which were devised in the state capitals. Villages and towns which had been independent for centuries were forced to merge with neighboring villages or towns. Often traditional rival villages and towns which had been part of other territorial units with their distinct history and their distinct characteristics, such as religious affiliation, lost their collective identity or were at least threatened with losing it. Fritz SCHÜTZE's idea was that such phases of turmoil provided a special opportunity to uncover political processes which are usually kept from outsiders and to study how macro conditions which were outside of the sphere of influence of the local politicians had their impact on their work and their lives, i.e., how local politicians made strategic use of such changes or “drowned” (so to speak) and got marginalized in the political arena. It seemed the best way to learn something about these processes was to let the actors in this drama tell about their experiences without unnecessary outside interference.

The idea was to use a sufficiently innocent topic as a question for generating narratives: the story of the “fight” for the new name of the new community and how our interaction partners, local politicians, had been involved in these processes as members of different factions themselves. (We conducted these interviews in two newly formed villages and one newly formed town.) When choosing this topic we were well aware that this appeared amusing and even trivial for outsiders—and especially “serious” sociologists—but it was a serious matter for the local people and their representatives. At the same time it was something which narrators could talk about with some distance: These were battles of the recent past. SCHÜTZE's assumption was that the local politicians whom we interviewed would tell about the development of these events, but would also feel that it was necessary to explicate their involvement in other “backstage” political processes which they would usually keep under information control in ordinary conversations or in situations of standardized interviews; trying to omit these backstage processes from their presentation would be difficult, there would be a “lack of plausibility.” This assumption was correct and was affirmed again and again in the course of our field research; we gained extremely detailed insights into the collective and personal social processes in these arenas.¹⁰

¹⁰ I will not refer to the substantive findings in my introduction, but see, e.g., SCHÜTZE, 1982.

I want to mention how we proceeded when doing these narrative interviews. As I noted above, the term “narrative interview” was coined within this research project (SCHÜTZE 1977) and the format of the interview has basically stayed the same since then (cf. the interview with Hülya in the Appendix), even when the fields of application have widened and many other substantive topics and concerns have emerged due to the methodological insights into the analytical usability of such data. This form of interviewing is firmly grounded in members’ everyday competencies to narrate their own experiences,¹¹ but of course it is also a deviation from situations of everyday story telling, because it has particular features of a research procedure. Furthermore, since it is a professional project problems and paradoxes arise (as in all types of professional work) which have to be handled in a sensible and responsible manner.

At this point it is possible to mention a few aspects which turned out to be important in the course of this field research on community power. I prefer to formulate them in a general way and in the present tense since they are still relevant in studies which are based on this procedure:¹²

- 1) It is necessary that a *sufficient trust relationship* develops between researcher(s) and informant before and during the interview. This also involves, as we discovered, a narrative presentation of self of the researcher(s): it is necessary that a researcher refers to the process in which the idea of asking the informant for his or her own story (about X) emerged in the first place. It is also important that the prospective informant develops some interest in co-operating in this project, i.e., that it makes sense and appears worthwhile. The researcher has to inform the interviewee about features of this particular interview format (cf. 3. and 4.) and has to find out if the interaction partner goes along with her or his suggestions. And of course the interviewee has to be

¹¹ This is a basic assumption which has been found valid again and again: that people basically share the ability to narrate their experiences. Against this backdrop it is especially interesting to study extempore presentations which reveal a partial loss of a narrative relationship to one’s own biography. Cf. my study on the life histories of mental patients which was based on autobiographical narrative interviews (RIEMANN, 1987): Sometimes biographical experiences had become so complicated and diffuse and had led to contradictory or idiosyncratic self-theories that another scheme of communication—in this case the scheme of argumentation—became dominant.

¹² It would be a misunderstanding to assume that a “technique” developed once and for all that is now “fixed” in time. Social scientists working with narrative interviews have their own research experiences which they reflect upon and which influence their personal style of conducting field research. I would just like to mention one feature which has changed from “the early days” of this style of research: In the community power study there were usually two researchers present in an interview situation which at that time proved to be helpful for reflecting on what had happened in the interaction with the particular interviewee and for developing and revising research strategies. This still occurs (cf. the interview with Hülya), but I assume that in many or even most research projects based on narrative interviews only one researcher is present in an interview situation.

assured that her or his information will be treated in a strictly confidential manner.

- 2) *A generative question* has to be formulated in such a way that it can elicit an extempore narrative of the interviewee's involvement in a complex of events and experiences that were relevant for her or him, in contrast to eliciting descriptive or argumentational presentations (like "accounts" in the sense of SCOTT and LYMAN (1968), which primarily aim at saving one's face as self-justifications, excuses or "sad tales" as GOFFMAN (1968, p.141) referred to them.¹³ It is important that the topic lends itself to a narrative presentation, a presentation which should not be prepared prior to the interview because the analytically relevant features of off-the-cuff story telling would get lost. Of course, it is necessary that there is a sufficiently marked topic: it is a common misunderstanding that narrative interviewing means letting the interviewee tell about *anything* of her or his own choosing, taking into account the general interest of the research which she or he had been informed about. And there should be a mutual understanding that the researcher does not know very much about the interviewee's relevant experiences and the events which are of interest in this context, otherwise there would be no point in telling the story.
- 3) After the informant has ratified the scheme of narration, i.e., has agreed to tell her or his story, *the main narrative unfolds without any interruptions* by the researcher—except when she or he is getting lost and does not know what the narrator is talking about. If the researcher wants to have more specific information at certain points, but is still able to follow the line of presentation, she or he should write down short notes for later queries (cf. 4.). Of course, a researcher-interviewer is constantly interacting with the narrator by showing interest in the unfolding narrative ("uhm," laughter, means of non-verbal communication), but as long as the listener does not get lost there should be no interference—no queries or commentaries from the "outside" which provoke a deviation from or even disruption of the story line. The idea is that the interviewee can really be guided by her

¹³ Inexperienced interviewers sometimes make the mistake of asking their initial (supposedly "narrative") question in a way that is systematically irritating for the interaction partner because of conflicting tendencies as far as schemes of communication are concerned ("schema salad"), i.e., the initial question is unintentionally both directed at generating a narrative and generating argumentation. In this case it is difficult for the interviewee to develop a sufficient orientation for the communicative task at hand even if she or he is quite willing to cooperate. In contrast to narrative interviews other research procedures systematically center on other schemes of communication: group discussions make use of the scheme of argumentation and ethnographic interviews as described by SPRADLEY (1979)—e.g., interviews on the features of knowledge systems of social worlds and milieus—make use of the scheme of description. Cf. the distinction between different schemes of communication in KALLMEYER and SCHÜTZE (1977).

or his recollections, i.e., the narrative interview can be used to reproduce the inner form of the sedimentation of experiences in which the interviewee was involved, in which she/he acted and/or suffered (SCHÜTZE 1983, 1987). The narrative unfolds until a coda, which is always clearly marked. Sometimes inexperienced interviewers find it difficult to remain silent before the narrator has arrived at the coda, but they soon learn that it makes much sense to refrain from asking questions; the disruption of a story line would create many difficulties for the sequential textual analysis later on.

- 4) After the coda of the main narrative there is an extended *phase of questions and answers*: in the beginning, narrative questions aim at exhausting the additional narrative potential that had “flashed up” in the main narrative (in hints referring to further narrative possibilities and in noticeable “gaps”). Concerning such questions it is possible to distinguish between “immanent” questions which are based on the main narrative and “exmanent” questions which introduce “new material.” And afterwards it is possible to ask questions eliciting descriptions (e.g., on recurring situations and routines, milieus and social worlds) and theoretical-argumentational statements (retrospective evaluations and reviews, reflections on what one would do differently today, what the events reveal about one’s self etc.). It is important that such argumentational questions are not asked in the beginning of this phase since this would make it difficult for interviewees to resume narrating.

I want to return to the study on community power for a moment. In that phase we had not yet devised strategies of sequential textual analysis—this was something that slowly emerged over a number of years up to the early 1980s (cf. SCHÜTZE 1983). Nevertheless it was already quite impressive how certain formal features of the narratives could be related to specific analytical dimensions (SCHÜTZE 1976). For example, when I think of our interest in discovering the interest constellations of the politicians whom we interviewed, there were certain features of their narratives which became relevant in this regard: a conspicuous vagueness when recollecting certain events and experiences; the use of devices of turn taking (“What do you think about this issue?” as a question posed to the interviewers); generalizing and abstract commentaries when approaching matters which had to do with being involved in illegitimate “back-stage” affairs. It was also possible to link other analytical dimensions and intentional or symptomatic textual indicators which I will not discuss here. In any case, it was quite helpful to have the possibility to make cross-comparisons of the different relevant perspectives in the respective local communities. In this way it was much easier to assess the meaning of certain formal features which appeared again and again in specific contexts of remembering.

When thinking about our research experiences it became clear that the notion of a narrator who is fully in control of her or his narrative presentation was

inadequate. This notion has been prevalent in a good deal of symbolic interactionist research and in the literature which mainly discusses narratives under the aspect of a strategic presentation of face and purely interest-bound reconstructions. What became important for further research was SCHÜTZE's discovery of certain constraints of extempore story telling. These constraints can be detected in everyday story telling as well as in narrative interviews (as a form of research communication): the constraints to condense, the constraint to go into details, and the constraint to close the textual forms [*“Gestaltschließungszwang”*]. As SCHÜTZE put it (I assume some of the dynamics of these constraints becomes apparent when looking at the interview with Hülya):

The narrative constraint to condense entails the narrator's being driven to tell only what is relevant in terms of central “knots” or the overall happenings in the story to be told. Single events and situations have to be evaluated and weighed permanently in terms of the announced overall thematic meaning and moral of the story to be told. The narrative constraint to go into details has the following effect: if the narrator has told event A, then she/he has to go on and has to tell also event B related to event A as the next link in the chain of experienced events—these events are concatenated formally in temporal succession, causality, finality, etc. In cases of implausibility of the envisaged narrative proceeding from event A to event B, there has to be a ‘background search’, a checking of the details of the supposed link between events A and B. The narrative constraint to close the form [Gestalten] has the following impact: the narrator is driven to finish the depiction of an experiential pattern (such as an episode in the unfolding of events, an interaction situation, a chapter in one's life history, etc.). In extempore storytelling there is always an undecided competition between these three narrative constraints.¹⁴

After the study on community power SCHÜTZE engaged in some basic theoretical research—quite often in close collaboration with a linguist, Werner KALLMEYER¹⁵—on the structure of different schemes of communication (narration, argumentation, description) and the sequential order of action schemes; this work led to a deeper understanding of what goes on in narrative interviews and in other spheres of institutional communication (cf. KALLMEYER & SCHÜTZE, 1977; SCHÜTZE, 1978). At the same time he and quite a few other social scientists began doing narrative interviews in other fields of application outside the arena of community politics, e.g., status passages.

Something which turned out to provide an important impetus for biographical research in the long run—in theoretical as well as in methodological re-

¹⁴ The notion of narrative constraints was explicated in several publications by Fritz SCHÜTZE (e.g., 1977, 1982, 1987). At this point I quote (with his permission) from an unpublished paper (SCHÜTZE, 1991).

¹⁵ KALLMEYER's sociolinguistic work which he has engaged in together with his collaborators at the Institute of German Language in Mannheim has attracted much interest, e. g., their detailed ethnographic study of urban communication in different social worlds of the city of Mannheim (cf. KALLMEYER & KEIM, 1988; KALLMEYER, 1994).

spects—was SCHÜTZE’s decision to conduct autobiographical narrative interviews, i.e., interviews in which people were asked to tell their life history as such, not just certain status passages. The background for this decision was the irritating observation that the interviews which we had conducted in the study on community power always had a very important autobiographical component even though the focus had been the narrators’ participation and entanglement in collective social processes; the biographical processes which became visible in such texts were hard to grasp analytically. Of course, literary autobiographical texts have been a well known and celebrated genre. An early example of this genre in Germany was Karl Philipp MORITZ’ s “Anton Reiser” which was originally published in 1785 (cf. MORITZ 1977). At the time of SCHÜTZE’s decision it was far from certain if it was feasible to just ask ordinary people to narrate their life history even if such interview situations had been carefully arranged and motivated. We did not yet know if such a general topic could organize oral extempore narratives, but it became obvious very quickly that it “worked.”

In order to understand these autobiographical narrative interviews, SCHÜTZE engaged in a formal sequential analysis; he used the analytical resources of conversational analysis, and by doing sequential and comparative analyses it became possible to identify specific textual forms of representing biographical structures. He identified recurrent and regular forms which are related to specific modes of biographical experiencing, “structural processes” of the life course (SCHÜTZE, 1981): namely (a) different kinds of biographical action schemes, (b) institutional patterns of the life course (such as phenomena of life and family cycles, career patterns, etc.), (c) metamorphoses (like the emergence and gradual—often unexpected and surprising—development of creativity, e.g. in artists’ biographies), and (d) biographical trajectories (SCHÜTZE 1992, 1995; RIEMANN & SCHÜTZE, 1991). Trajectories in this sense are biographical processes of long-term suffering and cumulative disorder, of being overwhelmed by outside forces which lead to a successive loss of control over one’s life circumstances. In working on many different autobiographical narratives analytical strategies emerged which proved to be heuristically fruitful and economical for doing studies in diverse substantive fields.¹⁶

¹⁶ I do not want to explicate these research procedures at this point, but just want to mention that they consist of a combination of the steps of a sequential single case analysis (text sort differentiation, structural description and analytical abstraction), contrastive comparison, generating theoretical models, confronting the theoretical models with new empirical data and their further differentiation and densification (cf. SCHÜTZE, 1983) and as an example of a monograph in which these procedures were used: RIEMANN, 1987). The element of generating and elaborating theoretical models via contrastive comparisons owes much to GLASER’s and STRAUSS’ methodology of developing “Grounded Theory” (1967, pp.101-115). Another approach to narrative analysis (FISCHER-ROSENTHAL & ROSENTHAL, 1997) is partly based on these procedures, but also utilizes other analytical resources like OEVERMANN’s “Objective Hermeneutics” (see OEVERMANN et al., 1979).

The concept of “trajectory” which has been very prominent in the writings of Anselm STRAUSS (cf. GLASER & STRAUSS, 1968) was further refined in this work on autobiographical narrative interviews in quite different fields of interest. The textual analysis of trajectory processes led to the discovery of phenomena which had been overlooked in sociological research which always had difficulties with conceptualizing suffering. This is also true of much of the theoretical work in the tradition of symbolic interactionism with its emphasis on “deviant careers” and intentional acting even in hopeless situations—“ways of making out” as GOFFMAN called it (1968, pp.157-280).

It is important to keep in mind that using the concept of trajectory in this sense does not entail forcing an “outside” category upon the data. Trajectory processes have to be discovered in working on the narrative data, by taking into account intentional and symptomatic indicators like: (a) suprasegmental framing devices which announce a radical shift in experiencing one’s life, (b) references to conditional relevances (“this forced me to do ...”), (c) early indirect hints that something difficult is coming up, (d) commentaries that show how one calmed oneself despite ominous signs that things were getting worse, (e) extended sequences of arguing with oneself (cf. RIEMANN 1987), e.g., in an extended pre-coda phase of a narrative, background constructions as self-correcting devices to repair a disorder in the narrative.

Thus far I have tried to provide some background to the emergence of the narrative interview as a research procedure, on its underlying assumptions and on its impact on biographical research. My intention was to put into perspective the interview situation and the data which all contributors to this issue of “Forum: Qualitative Social Research” focus on. The student researchers who contacted a Turkish migrant woman, Hülya, who was to become their hospitable host, had become familiar with this procedure of collecting data and had in mind to do an autobiographical narrative interview with her. That is what they did. By the time they did this the narrative interview had been around for a number of years and had already been used in quite a few studies in the field of biographical analysis.

3. Concluding Remarks

The interview with Hülya is one of 48 autobiographical interviews with migrant women (most of them Turks) which Christa HOFFMANN-RIEM and her students had collected. She did not live long enough to finish her research, but one thing which she did do was to write a letter to the women who had been interviewed informing them in German and Turkish on what she had found in her first analyses and in a comparative perspective, so they could relate their own specific experiences to those of other women who had also come to Germany as female “guest workers” (HOFFMANN-RIEM, 1994, pp.277-299).

Some of the themes which are prominent in Hülya's narrative also appear salient in this letter: e.g., the degrading experience of undergoing the selection procedures and health checks in Turkey; the shared feeling of being deceived when the women were confronted with the inhumane working conditions in Germany; the frustrating knowledge that it would be impossible (because of the collective positive image of Germany) to reveal to their families back home what was really happening to them; the onset of diffuse chronic illness which was often hard to define; the gradual calling into question of the matter-of-factness of traditional gender roles; the discovery of new strengths; etc.¹⁷

HOFFMANN-RIEM's study focused on a topic which had been neglected in the social science literature on migration, namely, the biographical experiences of female "guest workers." During the 1960s and early 1970s (until the recruitment stop in 1973) large groups of contract laborers had arrived in West Germany from Southern Europe and Turkey. While many of them returned, many others stayed and were joined by their families. Today about two million Turks are living in Germany (WHITE, 1997)—by now members of three generations.

I am convinced that the interview which is available in the Appendix is a valuable resource for learning something about the experiences and perspectives of people who are still marginalized in the political discourse and whose "essential" strangeness is often subtly affirmed in the mainstream social science literature, as INOWLOCKI (1998) points out in her critical discussion of notions of "Islamic fundamentalism" and irreconcilable cultural differences between "us" and "them." While Germany has been a de facto immigrant receiving society for a long time—think of the immigration of French Huguenots to Prussia¹⁸ and Hesse in the 17th century and of the large groups of Poles settling in the Ruhr area in the beginning of the 20th century—this is still far

¹⁷ This letter was typical for her style of relating to her "research subjects" and for the practical implications of her research. She wanted her research to contribute to practical enlightenment and to conditions in which their voices are heard. This is also obvious in the political consequences of her study on social processes in adoptive families (HOFFMANN-RIEM, 1990). This monograph (an English translation of a German monograph which had been published six years before) is the first study which introduces English speaking readers to the use of narrative interviews in the social sciences. It does not contain single case analyses, it is basically a comparative analysis (in the style of Anselm STRAUSS who also wrote the foreword) of many narrative interviews with adoptive parents in order to reconstruct the sequence of the social processes in becoming an adoptive family. The study contributed to strengthening self-help groups of adoptive families and adoptees and it helped to prevent the destruction of adoption files in the city of Hamburg. This destruction would have made it impossible for adoptees to learn about their roots and to contact their biological mothers with their consent. The study also had consequences for preparing a ruling of the German Federal Constitutional Court in 1989 which affirmed the right of knowledge about one's own descent (HOFFMANN-RIEM, 1994). At the time of her death she and her co-workers were engaged in studies on the biographical consequences of reproductive technologies and prenatal diagnostic procedures.

¹⁸ Around 1700 every third inhabitant of Berlin was a Huguenot.

from being taken for granted. Recently the idea of a German “Leitkultur” (“defining culture”) has been propagated by leading Christian Democrats. This is a concept which reveals the many reservations against people who have come “here” and are not like “us.” At the same time other politicians of the same party express the opinion that Germany needs over 600,000 immigrants per year in order to cope with long-range problems which are related to its particular demographic development. The emotionally charged and long-lasting struggle over the requirement and the details of an immigration law and the widespread existence of restrictive and harsh policies against political asylum seekers indicate strong tendencies in German society to view immigration as undermining social stability and as a threat to the national “we”-ness.

As I mentioned at the beginning, I suggest that you turn to Hülya’s story first and then to the articles which were written for this issue. You thereby become part of the group of people trying to understand the narrative and the experiences which it reveals. You will have a base for reading the analyses, for discovering how the authors proceeded and for finding their specific approach convincing or not. I have tried to make the point that this publication on “*Doing Biographical Research*” is not about technical matters in a narrow sense, but about ways of understanding the “other.” These ways of understanding—and maybe misunderstanding—are open to debate, a debate among colleagues. This issue is only a starting point insofar as the authors make their own reading of the text—Hülya’s narrative—visible and therefore open to criticism. All readers are invited to contribute their own pieces of analysis of Hülya’s narrative in the future or to comment on the papers they read in this issue by offering their own interpretations of the text. Such a continuation of the work which has started in this issue may avoid the pitfalls of very general and repetitive debates about (supposed) positions and premises of biographical research which have often lost their empirical grounding and are prone to rhetorical idling.

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