When "We Ourselves" Become Our Own Field of Research[1]

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Abstract: In our current research project, we do cultural research on cultural research. In doing so we apply cultural research-instruments to ourselves and thereby constitute ourselves as a field of research. We describe some of the incidents that can occur in such an ambivalent situation, focussing on the following (which appears to us as the most significant output of our work in this special context of research so far): This kind of research will always be a sensitive one when dealing with biographical material. Since one has to deal with humans, "naturally" relationships develop along the way. Neither the social status of the persons explored, nor closeness or distance to them should affect the results or the degree of responsibility felt towards to them. The representations resulting from this research ought to be salient to all participants, including the researcher and the researched. Research is an interaction—this assertion implies for us that we do not want to study sciences for the purpose of science studies, even if we (would) like to put our work into these discourses. It is important to us to transmit our research into our everyday academic life, put our findings into action—in teamwork, in teaching, in organisation and communication of research. These interactions also have a reflexive effect on our research project —they draw our attention to certain topics, help us set priorities and make us aware of taboos.

Table of Contents

1. How Does It Come to Pass?

To legitimise your research by quoting the authorities of the field is essential, especially if you are not an authority yourself. We will start with Pierre BOURDIEU: a trained philosopher himself, he described "typical" philosophic ways of working and approach using the picture of a military strategist's vantage point on top of a hill, where the philosopher would stand like a general, observing and analysing people from a distance (1993b, p.41). Likewise, other scholars of

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1 In a first feedback to our article one of the editors commented that as not (yet) established researchers we do not (yet) belong to the field which we research. However, irrespective of our position in the scientific hierarchy: contents and practices (e.g. forms of recruiting a new generation of academics) of every scientific community are influenced not only by established persons (e.g. professors) but also by "young" people, of whom no one knows if they want to/can get ahead. Insofar, we belong to our research field.

the humanities, the cultural and the social sciences also tend to interpret the social and cultural commotion, groups and societies, conflicts and alliances as if they were not part of it, as if they would not carry culture and society inside themselves. It is true that science studies have been analysing science as a social event for quite some time (e.g. LATOUR 1987; KNORR-CETINA 1991). But their research almost entirely deals with "the others". Just as traditional European and North American cultural and social anthropologists concentrated on non-European cultures, cultural and social science studies inquired predominantly into technical and natural sciences (for a survey, see FELT et al. 1995). If, on the other hand, researchers of social and cultural disciplines (we include ourselves in this group) go for reconstructing the culture, social shape and history of their *own* discipline(s) or their *own* scientific community, they hardly ever speak of the social, cultural or political character of academic work. Jacques LE GOFF, for example, designed the history of French "nouvelle histoire", which used differentiated instruments of cultural and social sciences, as a history of "great men" (1994). "Far too long historians have written the history of their discipline using concepts which they would never have applied to a single subject", as Roger CHARTIER commented on such attitude (1992, p.10). Although ethnologists, historians, sociologists and others meanwhile have accepted the importance of a reflexive approach, many attempts to "objectify the objectifying subject" (BOURDIEU 1992, p.10), that is, the subject conducting cultural and social research, do not get beyond programmatic declarations (DRESSEL & LANGREITER 2003, pp.134-136)—except for a few exceptions, especially in ethnological and feministic contexts (e.g. AUSLANDER 1995; FUCHS & BERG 1993; EISCH & HAMM 2001; LINDNER 2000; NADIG 1989; NÖBAUER & ZUCKERHUT 2002).[2] [1]

1.1 The project "Self-Reflective Historical Anthropology"

In the research project "Self-Reflective Historical Anthropology"[3] we apply some of the tools of our scientific approach—namely historical anthropology (DRESSEL 1996; DÜLMEN 2001)—to "ourselves", meaning the academic areas we move in, or feel affiliated to. Since the boundaries of historical anthropology in Germany and Austria regarding both the contents and the social and institutional field are not well-defined, we have extended the area of research to include those involved in studies of culture. [2]

Thus, we try some cultural research of cultural research, in particular in the German speaking area. In this field we do not find completely independent, autonomous producers of academic knowledge; rather, we deal with a field

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2   The frequently quoted debate about writing culture (CLIFFORD & MARCUS 1986) was restricted to the reflection on practices of academic writing, which was equated with completed papers. Recently Norbert SCHINDLER has stated that the "post-modern critics [...] know nothing and worse do not want to know [...] what alongside with their academic socialisation and their character produces their particular style of writing" (2002, pp.289f.).

3   The project is financed (March 2000—February 2003) by the Austrian Science Fund (FWF) and assisted by the Institute for Interdisciplinary Studies at Austrian Universities (IFF) in Vienna (http://www.iff.ac.at). Head of the research project is Michael MITTERAUER. Both of us are full-time employed research assistants. Other assistants (on the basis of contracts for work) are Anelia KASABOVA (Sofia), Caroline zum KOLK (Paris) and Ulrike KRAMPL (Paris).

consisting of men and women, and a variety of age groups with a specific "Generationslage" [generational consciousness] (MANNHEIM 1928, pp.173f). Since the beginning 1970s there has also been an increasing number of people originating from different social and cultural milieus.[4] And finally, we come across proponents of a range of disciplines—including education, history, language studies, sociology, and folklore studies/European ethnology/cultural anthropology/ empirical cultural research. All these researchers have their own socio-cultural experiences and structures (BECHER 1989, esp. p.79; see also: FLECK 1980, esp. pp.52-70); they do not work along next to each other quietly, but quarrel, co-operate, ally, compete—hence emerge discussions, discourses, papers, books, sometimes entire new paradigms, schools or branches of research.[5] Social relations are "a key factor for a descriptive model of academic activity" (FELT et al. 1995, p.82). Their relations do not emerge and break in a "free play of powers", moreover, just as any other social field the field of cultural research is regulated, and features a hierarchical structure:

> "While publication constitutes the formal and explicit criterion for recognition, there is (here as elsewhere) an informal and tacit dimension which also has to be taken into account. However important quality may be, it is not only what you write but who you are and where you come from that counts." (BECHER 1989, p.54) [3]

The regulations and hierarchies are changeable, not static. By dint of specific supportive sanctions of universities and research funding, and with the aid of particular systems of rewarding, new generations of academics are recruited following particular criteria (MÜLLER 2000, esp. pp.291-295; STRASSER & SCHLIESSELBERGER 1998, p.229). Several networks of protagonists, some of them competing with each other, participate in the decision-making on who and what (contents, methods, theories) belongs to the field, who is going to be let in and who will not; for instance some are allowed to publish in a highly respected review, while others are not (ROTH 2002a, par. 20; 2002b, par. 21f). [4]

Finally, those willing to work as cultural researchers have to develop strategies and ways of acting, in order to fathom out the possibilities and barriers in academic life so that they can deal with them. Individuals have disparate positions in the field of cultural research: men and women, the younger and the senior protagonists, professors and so-called new generations of academics, to give just a few examples. That leads to defined repertoires of "structured options of action" (ALGAZI 2000, p.114). How do academics with an orientation towards cultural research act after having taken their degree? Does writing a dissertation play an important role for them? Do they follow the ways of a mentor (perhaps even a female mentor), hoping to obtain some backing (STRASSER & SCHLIESSELBERGER 1998)? Do graduates prefer individual action or are they eager to form groups of "the same kind" (NÖBAUER & ZUCKERHUT 2002, esp.

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4 Cf. generally Wolfgang WEBER "Priester der Klio" [Priests of Clio] (1984); Erika HAAS (1999, esp. pp.103-106) has studied the current situation of female students coming from different social contexts in four departments of Munich University.

5 Illustrative of scientific writing as a communication form and its general conditions and implications: RÜCKRIEM 2000, esp. pp.112-116.

Chapter III)? Do they strive for careers as researchers, or do they move towards completely different occupations? And what might all that mean for the self-image of the protagonists (or their identity)? [5]

We are interested in (our "own") science as a social, interpersonal and hence interactive event, and eventually as a personal one. The range of topics for such an investigation is almost infinite. If professional practices are on the agenda, even the experience of one single working day might raise an almost unlimited number of questions.[6] [6]

Although the categories of gender, generation and social background were essential in the beginning and are still important now, they are too general and too fundamental—concentration and reduction has proved to be necessary. New topics have arisen from discussions with colleagues at meetings and workshops or from university courses we held, and some problems and issues have become more specified. Resonance "from outside" indicated quite clearly whenever we had touched something suspenseful and relevant. "Work, leisure, and family lives of academics" is such an exciting matter (DRESSEL & LANGREITER 2002c), as well as the relationship between cultural research, political systems, and gender (DRESSEL, KASABOVA & LANGREITER 2003) or the restoration of gender relations in the humanities at German and Austrian universities after World War II (DRESSEL & LANGREITER 2002a). We came upon questions that partly had already been dealt with by cultural research in reference to other social areas, but have hardly ever been attended to within research of academic life per se: for example, "science and traditions" or "ageing experts". We are not at all interested in self-reflective practice and—in this context—the self-images of disciplines and scholars (DRESSEL 2001, pp.45-52). [7]

We argue that biographies are not individual and random, and that the persons in question represent a particular set of social, cultural, and other meanings and experiences (in correspondence with gender, origin, occupational milieu, age group, political experience etc.) that implicates certain possibilities, attitudes, and specific ways of acting in particular contexts. "Biography is, in concrete terms, socialness and subjectivity all in one" (ALHEIT 1995, p.88). Those involved in the sciences are in no way different from other social groups in these points. [8]

Life histories of historical anthropologists and cultural scientists are our primary source. Above all these texts consist of semi-structured narrative interviews.[7] We chose men and women as interview partners to represent various age cohorts, as well as various social and academic origins or positions. In order to broaden the

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6   Here might be a disadvantage, however: it is difficult to restrict and limit the topics. Once you have developed a taste for reflexivity, you are permanently on duty.

7   A semi-structured interview is one in which an introductory question is put, with the intention to have the interviewees reconstruct their life histories in the most ample detail possible. The questions put to the interviewees afterwards are either triggered by the narratives or refer to relevant areas of the research project on which the interviewees did not initiate discussion themselves. In this second phase, experiences beyond the field of science are important for us, e.g. life history before the years at the university, or social commitment. Again, our questions aim at motivating narratives and stories. The third phase is a reasoning: the interviewees have the opportunity to speak their minds on the conversation and interview situation.

ken of interpretation, we also interviewed colleagues in Bulgaria and France (to date we have completed 25 interviews). Although it had not been originally our intention, the current boom in autobiographical practice among cultural and social scientists, resulting in an abundance of published life stories, led us to refer to some of these autobiographies as a source.[8] For the sake of utmost openness and receptiveness about the peculiarity of our field of research we include all sorts of source material: along with the texts of our interview partners, journalistic productions, songs, and illustrations can be found in our fund (also very interesting but rare are scientific jokes). [9]

Returning once more to the image of the general's vantage point: this is a viewpoint we cannot take up, since we are involved in all matters we are exploring. Furthermore we hold a certain position in this field of cultural research: as a European ethnologist and a historian, both working at an interdisciplinary institute financed via research funding, and belonging to a younger generation (even if "young" has only relative meaning) (NAGEL & SIEG 2000, p.1). These and other experiences and (pre-) structuring influence our perception, our consciousness of certain problems (which, however, keeps changing), the interests we follow, etc. The idea for the project "Self-reflective Historical Anthropology" did not hit us by accident. For us to investigate into "our own" territory, starting from a definable situation in this our "own" field, brings about both problems and chances. [10]

2. What Happens?

Many things can happen when we become our own field of research, and many things do happen indeed. In the following, we will outline a few. As it was just mentioned, while researching in one's own area, immediate chances, but also problems, or dangers, so to speak, may turn up. We do not want to tell the story of our success (which we might well do), but rather speak of the problems that preoccupied us, and partly still preoccupy us. In the long run, mistakes and troubles have always pushed us "forward". In this respect we actually do tell a story of success—but in the first place we wish to present a few examples out of our research process and put them up for discussion. [11]

Regarding our methods, which have been roughly outlined above, we pick out two problems: a general one relating to biographical research, and a more particular one that has to do with working on or with academics using this very method.[9] We are quite conscious of the possibilities and limits of biographical

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8   The sample of autobiographers is differently structured than that of our interview partners. Those who make their autobiographies public have usually reached a certain age and a position in the field (e.g. FLECK 1996, LEHMANN & OEXLE 1997) that allow them to publish such a thing as a life history (exceptions are: AUSLANDER 1995; LIST 1999; SCHWEIGHOFER-BRAUER et al. 2002). Only at an advanced age and in an advanced position does it become possible to speak of more than "pure science" in public. These autobiographers have in a sense become contemporary witnesses, whom one is satisfied to listen to when they talk from a "subjective" perspective. Some critics spot a form of narcissism here (BOURDIEU 1993a).

9   Of course we might reflect on a lot of other problems: e.g. the specific interview situation when the own scientific community is investigated, the meaning of places (the workroom at the university or private rooms) where the interviews take place, or the technique itself. In this

research, we trust in its quality—we consider biographical research as an efficient technique to analyse and reflect upon academic practice (BERGER 2000, pp.24-30; ASH 1998, p.297)[10], which means to trace the correlation of society and academic practice through life stories of academics (LINDNER 1987, p.15). We are not interested in revealing private information, but we ask how social processes, structures and experiences, including those of academic life, impart themselves through individual biographies. Our central aim is not do shed light upon the private lives of academics, although sometimes this would be rather tempting, but on the practices of scholars working in a cultural or historical-anthropological way. [12]

The narrative interview aims at creating narrative space by applying a conception as open as possible. In our case, this means ample space to talk about autobiographical memories, current perceptions, forms of action, interests in academic life, or other things. The first interviews revealed not only the options but also the limits of this method—especially when the interviewees are academics. Sometimes their narrative habits hardly allow any enquiries at all. [13]

When analysing the social and cultural conditions, the social construction of academic knowledge or humanities as a socio-cultural practice it is most enlightening—although irritating at times—to discover that the knowledge of the scientific community is constructed in its nature, particularly so if you belong to this very scientific community yourself. Moreover, it is interesting to analyse the representations in which such knowledge is transported (even in the interviews). We consider a particularity of our project that our most important "sources" are at the same time our primary target group: we talk to members of the cultural research community, and we communicate our interpretations of the interviews to this very community through publications, lectures and other presentations. This creates a complex situation, from which we will single out the aspects of distance and nearness. [14]

2.1 Distance

Researching one's "own" area does not imply from the outset that there is nearness between the researchers and those researched. The field is heterogeneous and offers many opportunities for irritating experiences and distances/dissociations. At this point we will speak with two voices for a while, as our experiences differ—for example, we are differently situated with respect to our starting positions for an academic and institutional career. [15]

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context, experiences of other studies are interesting: e.g. techniques of psychodrama were applied in order to understand situations where the cognitive level failed (SPÜLBECK 2001). It would also be interesting to reflect upon the fact that, consciously or unconsciously, gender influences the interviewers and interviewees. (HAMM 2001, p.170)

10 See especially the following research projects: "Pioneers of Social Research: a Research Methods Teaching Resource" (Paul THOMPSON, Louise CORTI, both Essex, and Ken PLUMMER); more details: http://www.qualidata.essex.ac.uk/; Volker DEPKAT's (Greifswald) "Habilitation" project: "Historische Zäsuren und biographische Krisen im 20. Jahrhundert: Geschichtserfahrung im Medium autobiographischer Selbstauslegung", project outline: http://www.uni-greifswald.de/~histor/neuest/habil.htm [Broken link, FQS, December 2004]; see also the studies of INGRISCH and LICHTENBERGER-FENZ (e.g. 1999).

Nikola LANGREITER: I feel affiliated to our research field, although for me personally distance is rather more significant than nearness. Concerning my own academic position I am far apart from the majority of our interview partners: I am not established in academic life, but work as a freelancer; I am not qualified to assume a professorship, but have only recently taken my doctor's degree. As compared to most of our interviewees I am in a very different career situation; other categories (such as age, social background) produce distance as well. In addition, some parts of the field have remained rather puzzling to me until now. Therefore, for me it is not so vital to exoticise the "own" world (as BOURDIEU recommends for a "sociology of sociology"[11]) (see also: BOURDIEU & WAQUANT 1996, p.62f). [16]

Gert DRESSEL: I, too, think that I belong to our field of research, and in an early phase of our project, around the end of the 1990s, I also realised distances between myself and my interview partners, most of them professors. I found this confusing, and it influenced my style of interviewing. Only a short time before the first interview appointments I had published an introduction into historical anthropology; up to that time I had hardly received any feedback for it. This left me insecure, and at the same time produced a desire to obtain some acknowledgement from my early interviewees—all of them well established protagonists in the field of historical anthropology—in order to become a recognised "player" myself.[12] Hence I expected far more from these interview situations than simply a life story—sometimes in vane, especially when my interlocutors did not allow either interrogations nor any demonstration of my competence as historian. This annoyed me and at the same time frightened me. [17]

Recently in European ethnology a few authors have reflected on so called "research up": Asymmetries in the relation between researchers and those researched confront the researchers with problems such as self-assertion in the field: it may prove necessary for them to safeguard their own academic authority against the persons enquired (BECK 2000, p.225). A special form of the "Researcher's Fear of the Field" (as Rolf LINDNER named a text in 1981) may arise: the anxiety about lack of acknowledgement as an academic. Furthermore —not meaningless to all persons involved—there are inequalities concerning the

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11 "Indeed, I hold that in the case of sociology, a sociology of sociology is a fundamental dimension of the cognitive process. [...] [One has] to try and recognise, by means of sociology, the social determinants to which the sociologist is subjected, and thereby get them under control. Thus, the sociology of this science, the sociology of sociology, is not merely a special discipline among others: it is the precondition for any sociological practice, in that it affords the necessary instruments to recognise the social restraints which—in the form of outward pressure or, worse, of internalised restraint—may be effective at a time." (BOURDIEU 1993b, pp.13f)

12 The editors conjectured in their feedback that this is possibly a "German problem". Asking for possible national features of scientific practices is relevant to us as well. After all, the educational and university systems have predominantly been organised in national contexts over several decades. However, we doubt that there is a marked difference between Germany and France (Pierre BOURDIEU's studies are generally known), or between Germany and Austria, concerning paternalistic elements in the academic system and the importance of acknowledgement by established persons for "young" scholars. Rather, we would put up the question whether in this context practices of action and relationships are particularly dependent on the available material and institutional resources. We suppose that currently (as compared to the 1970s) mentor-mentee relationships and forms of (symbolic) acknowledgement have become more important.

economical, social and cultural capital. Sometimes (e.g. when managers or academics get "observed") "the interview situation will mutate into an audience." (WARNEKEN & WITTEL 1997, p.7; p.1f).[13] Writing about troubles, about the personal state (or personal anxieties) may have compensating effects, as Kaspar MAASE pointed out (1997, esp. p.388f; see also FUCHS & BERG 1995, p.22): This makes authors sympathetic and may be useful for their self-presentation. Such "confessions" build up trustworthiness and authority—which leads to the situation that even in a research-up setting balance of power is used for individual rather than collective purposes. [18]

We now return to the experiences shared by the two of us: in the course of the research process some things have changed. Not only is the publication of the book "Historische Anthropologie" (DRESSEL 1996) now farther back in time (and meanwhile has gained credit). Through working as a team and through our increasing integration into networks of communication, some of which we have created on our own, we have acquired professional competence, or we are now more sure of them. Not least this relaxed the research interaction. Once we had managed to make an appointment, most of the interviews were very pleasant; the cultural researchers got involved in our project. Despite all the proficiency and high status of the interviewees we hardly ever have been confronted with the frequently described problems in research-up situations. Meanwhile we do not draw direct comparisons with the people investigated anymore. [19]

2.2 Nearness

On the other hand we come closer—for instance through our project we get a deeper insight into academic life. The narratives of the interview partners sensitise us so that we think more systematically than is common in academic everyday life about how this academic life is organised. Official and unofficial mechanisms of selection and the predominant criteria of judgement show up on the agenda, and we observe which sort of behaviour is rewarded and which is not. This know-how might serve our own interests, which of course is a delicate matter asking for reflection. To be more precise: critical potential should also be used for reflecting our own situation: for instance, the relation between the positions we strive for, or our actual positions in social space, and our academic statements. [20]

As for self-interest, there is an episode of positive interconnection in our project history: a cultural researcher is interviewed during the pilot phase of the project. In the proposal written later, we cite his work, in part for pragmatic reasons—we know his texts well, having read them while preparing for the interview. Perhaps

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13 As a precaution, we want to point out that this is not a matter of "narcissist reflexivity" as Pierre BOURDIEU and others have criticised a special kind of reflections on field research. Rather, our reflections refer to the position of researchers in social space. Just as BOURDIEU did, we want to observe the restraints which can affect the academic subjects (1993a, pp.366, 373). It is astonishing how vehemently some people warn of too much reflection. Sometimes self-research or reflexivity is not only criticised but even ridiculed (although what form of reflexivity is not specified in these cases). Constructive discussions are an exception, e.g. the recently published volume "Dazwischen" ["in-between"]: LÖFFLER 2001.

as a result of the extensive citing, the same cultural researcher was asked to write an expert report on our proposal, which he did approvingly (which he let us know during the second interview). Obviously we are entangled into this part of the social world. We must not lose sight of the complex structure of conditions and (our) actions in this structure, and we ought to consider that we ourselves are influenced by academic life—its implicit and explicit rules. To stress it once more: the categories we apply on other academics apply to us as well. [21]

All existing distances notwithstanding, every now and then certain forms of nearness may emerge; such as when interview texts give an idea on how gender sets the course to (truly important) professional positions. When, for example, it becomes clear that a female academic can be an expert in her field, very much engaged in academic matters and ready to put many other things in hold, yet still she will fall behind her male colleagues with regard to her career. Then inevitably we feel affected and therefore near. Or—but still in that framework—sometimes an uncomfortable nearness will be produced through an autobiographic text containing familiar stories and images. For example, we found stories about the "aged expert" touching and at times "too close". Reports by academics who after their retirement and gradual exclusion from the familiar milieu suddenly felt lonely and had to realise that they would now have to establish some private life, because during their active time they had hardly had any bonds to family or friends apart from the scientific community (DRESSEL & LANGREITER 2002c, p.128). Here we ought to take up questions going beyond the private or personal —about identity and practice of academics in a more general sense, about the ideals of academic practice, about its gendered dimensions, etc. And we ought to resist the temptation to feel "better" (yes, understood as an ethical category) in comparison with the other, or at least we have to deliberate on such processes. [22]

The latter leads us to another lure coming up every now and then—namely to deal with how "awful" the others are, instead of looking into socio-cultural circumstances (e.g. the entanglement of academic discourses and references to everyday life. It may start with asking someone for self-presentation through a biographic interview and afterwards identifying this self-presentation—including the person involved—as not very sympathetic or at least odd (because of diagnosed overestimation of the self). Therefore, beside the "Researcher's Fear of the Field" (LINDNER 1981) there is the field's fear of the researcher. Especially when those researched on are academics themselves, they will know that academic work is not always "honest". Thanks to a few publications we know that such worries are appropriate (we will return to that point later). Complicated situations may arise. One cultural researcher told us in great detail about his family and social background for a start; yet after a short break in the conversation he vehemently withdrew. The interview came to a sudden end. We had the impression that the interviewee felt sorry for having got involved, and was worried about having exposed himself too much. More than a year later, a second interview was held in a different location. This second meeting was for the purpose of asking further questions and feeding back first interpretations. We did so following our intention not only to see our interview partners as "objects" of research, but also as discussants and evaluators. At the beginning, the interviewee was

once more reassured about our integrity, our thoughtful way of dealing with our material and so on. It was only then that the interviewee told us that this was the very thing which had caused suspicions and uncertainty in him. [23]

We have to be aware of the spectrum of "negative" and "positive" emotions on both sides of the research process, because they influence action here and there. This is true not only when we all of a sudden ask and interpret those who usually ask and interpret others (GOLDINGER 2002, p.258). In literature, especially one form of research up is put in connection with extraordinary ethical requirements: "Being socially closer to the field observed creates high demands for the researcher's behavioural role and for his responsibility during analysis and publication of the insights and experiences gained", according to Heiner GOLDINGER (2002, p.259) for example. We wonder about this (quite common) conclusion; does social, ideational, spatial, cultural ... nearness really imply *more* responsibility? Responsibility—above all, for self-protection?[14] [24]

2.3 Representations

This does not reduce our responsibility in the project—for instance for the appropriate forms of presentation and publication of the results. We keep facing this challenge, above all since positive models are not very frequent. In fact, a couple of publications based on similar studies demonstrate that there is a market for sensationalist reports on the cultural sciences and humanities, even though most of them are unspectacular. An example is a study by the sociologist Heinz BUDE (1997), who researched the ageing of a generation, the so-called "generation '68". For that purpose, he writes portrayals of German women and men more or less involved in the events around 1968. We were rather shocked by the results. At the same time, this pointed out to us what must not happen. BUDE's portrayals are based on contempt for the interview partners. He carelessly reveals private and intimate details. Among other things, BUDE dabbles in psychology, presumptuously inspecting and judging the looks, appearance, dress style, and furniture of his interviewees. He also judges their morals. Their first disadvantage is their readiness for giving an interview—a pack of busybodies, so to speak. BUDE does not shrink back from denominating one as "a figure of fun through and through" (1997, p.142). Actually, the author describes only pathological cases. Is he perhaps taking revenge for mortifications that he had experienced? Probably there are several possible interpretations. Anyway, we are warned to have respect and to seriously and uncompromisingly reflect our own position in the interviews and in the field as a whole. [25]

A similar example is Hans-Ulrich WEHLER's "Die Herausforderung der Kulturgeschichte" ["The challenge of cultural history"] (1998). Originally intending a critical debate of Michel FOUCAULT, the author condemns the philosopher with the help of intimate biographical details. Among other things, FOUCAULT ranked

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14 By the way: which implications does the frequently stressed opposite between "research up" and "research down" actually have? What are the consequences for "research down"-studies when "research up" "is not a 'walk' through the world of the privileged" (GOLDINGER 2002, p.260)?

with the "chorus of disappointed Maoists". WEHLER accuses him of having opted, in 1968, for "the most miserable of all possibilities. [...] And who was the first prominent western intellectual to embarrassingly praise the bloody regime of Khomeini in Persia?" (1998, pp.84f) etc. etc. Starting from "the suicide attempt of the grammar-school student" (1998, p.88), touching upon all sorts of things, WEHLER is finally about to argue FOUCAULT's sexuality more "unbiassedly": all this, of course, in order to "explain" and "contextualise" his interests in body history, practices of exclusion, and disciplinary society (1998, pp.88ff). [26]

We are aware of the importance of words, especially of written words, for cultural sciences. Therefore we try to handle them very carefully. Originally we intended to publish nothing without explicit consent of the interviewees. In practice, however, this proved to be impossible. Every article quoting passages of interviews would have had to be approved by the interviewees involved, which is not realistic. Thus, we settled on an extreme and consequent de-personalisation as an adequate form of talking and writing about our research results. [27]

Every research based on biographical texts faces the problem of de-personalisation. The problem increases when members of a scientific community are interviewed—all the more so when a community is like a family. One example is the German-speaking "Volkskunde", where everyone knows each other. De-personalisation is indispensable when researchers are not only the sources and the address but, beyond that, the topic of the study as well. Persons cannot be disguised only by pseudonyms. The readers would quickly find out who is talking about whom, or who is interpreted. Albeit we are not interested in uncovering private things and finding biographical curios, and we do not try psychological analyses, we have learned at scientific events that the academic public is interested in knowing the authors of biographical material.[15] Pierre BOURDIEU considered the readers of "Homo academicus" (1992, pp.32f) to be potential informers, which is why he encoded his data to an extreme degree. In the meantime we, too, have come to adopt similar practices. We use alphanumerical codes even for autobiographers and autobiographical works which have already been published. In addition, we have departed from presenting case studies in presentations or essays. [28]

When applying for the project in 1999, we described the biographical method in agreement with case-orientated techniques of interpretation and representation in biographical research (e.g. SCHÜTZE 1977; SIEDER 1994; ROSENTHAL 1995). Since then, we have been compelled to qualify our initial position. Although it is indeed a particularly delicate matter when the research is conducted on one's own scientific terrain, the previously mentioned difficulties draw our attention to more general problems of biographical research. It has become quite clear to us just how necessary it is in this and in other contexts to deal very carefully with the biographical approach and the data and texts generated by it. What is at issue

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15 The novel *Het Bureau* illustrates how people act when they are compelled to work intensively together and spend a lot of time with each another. The novel shows how a bureaucratic apparatus functions. Those in the profession are busy trying to figure out after whom among their colleagues the characters may have been modelled (MEURKENS 2001, pp.26ff).

here is nothing less than the ethical dimension of our work, as is the power (interpretive sovereignty) with which researchers consider themselves vested. In the course of biographical case studies it is not rare for researcher to "ascend" again to the vantage point of a powerful and scrutinising strategist on a "Feldherrnhügel" and "look down" upon those being researched, discovering above all their "faults" and to a far lesser extent their positive potential. When the field of cultural studies is itself the field of research, the representatives of the field (including the interview partners) expect that biographical information and interpretations be handled in a particularly trustworthy manner. Even if the field of cultural studies as a research subject itself thus calls for treatment which does not prevail elsewhere in this way, we as researchers should nevertheless treat the research subjects with esteem, be it in writing or otherwise. [29]

Despite these concerns, selected interview passages are of great methodical importance in our work. The project's hypothesis-generating practice is based upon intense examination of text segments (similar to the analyses in sequences in so-called case reconstruction) which we criticised above). But we distinguish at an earlier phase between our interpretations and the individual persons (who are thus not treated as cases) and assign these interpretations to particular categories of problems and questions. Also in lectures and papers we present particular issues or topics under discussion. Only short quotes are published form our biographical sources. We aim thereby to draw attention away from the persons themselves—and from the "riddle" of who is who—toward structures and processes in cultural studies that lie beyond the level of the individual. We want to write critical and analytical texts which are reasonable for our interviewees. [30]

We have mentioned the general trend concerning the biographical practice of cultural science. The critical reflection of our project in the context of this practice is important for us: who uses this cultural technique at what time? Who becomes a contemporary witness (by the way: do men more so than women do?)[16]? The biographical practice produces various effects and meanings. "Learning"—the broadening of the cognitive horizon—from biographical experience is one dimension (some groups are advised to be very active in this respect [ASH 1998, pp.298ff]).[17] Furthermore the reference to biographical experiences of one's own and of others have become an accepted strategy of reasoning, exculpation, legitimisation, and disavowal in scientific discussions (see above). Some histories of (the narrator's own) disciplines are told as a history of shared social, political, or cultural biographical experiences. Where it is widely accepted knowledge that the possibilities and limitations of science have to do with researchers as social protagonists, "experience" can become a key element of academic practice (resp. of the analysis of academic practice). This is more likely the case when "the own"

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16 In the frequently discussed volume "Versäumte Fragen" ["neglected questions"] there are only two women amongst 17 interviewees (HOHLS & JARAUSCH 2000).

17 The historian Mitchell ASH recommended a "change in the sciences through reflectivity"—the expansion of academic questioning by means of "learning from one's own biography". The idea is that autobiographical reflection would lead researchers to new and interesting issues (1998, p.297). Basically we do not cast doubt on such self-reflective practice. Far from it! The problem is, this procedure is recommended only to a particular group. ASH suggests a "learning from one's own biography" especially to his colleagues from the former German Democratic Republic.

is experienced in relation to social and academic hegemony (both institutional and cognitive). Two examples: cultural studies, with their expanded concept of culture, are traced back to their "founding fathers" who had proletarian backgrounds (e.g. BROMLEY 1999, pp.10f; LINDNER 2000, pp.15-47).[18] Feminist researchers relate their professional interest in women and gender relations to female experiences of being strangers or being powerless in male-dominated areas of society (e.g. AKASHE-BÖHME 1995; LIST 1999; a criticism: HASENJÜRGEN 1996, pp.42f). It is undeniable that many biographical studies analyse the complex mechanisms of the academic world as a hierarchical organisation (e.g. BOCK & LANDWEER 1994; INGRISCH & LICHTENBERGER-FENZ 1999; NÖBAUER & ZUCKERHUT 2001). Nevertheless, the borders between analyses of scientists' experiences in power relations that can be objectified and the staging of one's own academic history are not always clearly demarcated. The category of "biographical experience" has become an instrument of scientific politics in the struggle for resources. [31]

3. What Is It All About?

It ought to have become clear by now that the involvement in the own research field also includes chances. Science is interaction—this is a general experience. But the reflection on this interactive character is not a general and common practice. Mostly there are only theoretical debates. We turn science as interaction into a systematic object of our reflection—with all the effects concerning research instruments, interpretation, and representation. Practising l'art pour l'art, biographical research for biographical research, or science studies for science studies does not satisfy us. We want to initiate discourses where theory, empirical research, didactics, organisation, and social orientations of science are connected. In this context we do not want to be the upholders of moral standards (although this sometimes happens). In fact we try to implement our research results on different levels of academic practice. Our intervention ought to be subtle (WILLKE 1994, pp.88f). It is self-evident that we want to exert influence by papers. But we have had positive experiences with direct forms of interfering and interacting. In particular, we try to create structures and processes "in our own area", where we have more possibilities for action: in our working groups, in our department, in university courses, in self-organised workshops etc.—first of all with "our equals", with so called "Neue Selbständige" ["new self-employed workers"], with freelance cultural researchers. We have instigated a number of communicative "places" in the course of the project,[19] and we take advantage of a

18 Tony BECHER describes "specially reconstructed histories" as parts of disciplinary ideologies. Past events are selected and heroes are chosen, in order to socialise students in a specific way. Furthermore, the histories are weapons in quarrels and controversies. The histories are parts of the "cultural capital" which is inherited by those who are granted the membership in a group (1989, p.25).

19 In the context of the Section Historical Anthropology at the IFF in Vienna (http://www.iff.ac.at/kwa) we have initialised, among other things, the following activities in order to create a reflection-orientated network: the colloquium "Reflexive Historische Anthropologie" (January 2000—December 2002), historical anthropological seminars in the context of the annually held *Winter Balkan Meetings* in Blagoevgrad (Bulgaria), the workshops *Wandel und Konstanz* ["change and continuity"] (May 2000) and *Normen und Formen—Zur Darstellbarkeit reflexiver Forschungsprozesse* ["norms and forms—on how reflexive research processes can be presented"] (May 2001), the workshop *freiberuflich kulturwissenschaftlich arbeiten* ["freelance

variety of places where we receive impulses from "outside" and where we can meet with people who move in various academic circles but who are not involved in the project, in order to discuss and engage in informal analytic thinking with them regarding our interpretations, thoughts, and issues. This also allows us to monitor ourselves. In this context we also aim at revising images and practices of science—even though this is a hard project. But a consequent self-reflective approach (meaning unspoken rules, the disclosure of relationships and power relations, well-structured moderation and communication etc.) can change the organisation of science. Dominant strategies of excluding can be attenuated. We intend to cultivate forms of academic co-operation where various skills and potentials are integrated. Again, biographical reflection can be helpful in order to perceive, develop and communicate these potentials and professional and social competence. However, even though the project and a self-reflective practice of cultural sciences in general is important for us—this is only a small detail of the world. Wolfgang KASCHUBA (1999, p.200) has stated that researchers tend to attach a meaning to the objects of their studies which "outside of our research reality does not exist in the same way". Certainly, this difference becomes all the bigger the more one is involved in the field which is researched. [32]

References

Akashe-Böhme, Farideh (1995). Von der Fremdheit der Frauen in der Wissenschaft. In Andrea Eichner, Gabriele Hofmann, Frank Penner, Ute Ritschel & Pia Wagner-Theurer (Eds.), *Fachfrauen—Frauen im Fach. Beiträge zur 6. Arbeitstagung der Kommission Frauenforschung in der Deutschen Gesellschaft für Volkskunde, 22.-25. September 1995* (pp.251-263). Frankfurt a.M.: Institut für Kulturanthropologie und Europäische Ethnologie.

Algazi, Gadi (2000). Kulturkult und die Rekonstruktion von Handlungsrepertoires. *L'Homme. Zeitschrift für Feministische Geschichtswissenschaft, 11*(1), 105-119.

Alheit, Peter (1995). Die Spaltung von "Biographie" und "Gesellschaft". Kollektive Verlaufskurven der deutschen Wiedervereinigung. In Wolfram Fischer-Rosenthal & Peter Alheit (Eds.). *Biographien in Deutschland. Soziologische Rekonstruktionen gelebter Gesellschaftsgeschichte* (pp.87-115). Opladen: Westdeutscher Verlag.

Ash, Mitchell G. (1998). Geschichtswissenschaft, Geschichtskultur und der ostdeutsche Historikerstreit. *Geschichte und Gesellschaft, 24*, 283-304.

Auslander, Leora (1995). Erfahrung, Reflexion, Geschichtsarbeit. Oder: Was es heißen könnte, gebrauchsfähige Geschichte zu schreiben. *Historische Anthropologie, 3*(2), 222-241.

Becher, Tony (1989). *Academic Tribes and Territories: intellectual enquiry and the cultures of disciplines*. Milton Keynes/Bristol: The Society for Research into Higher Education.

Berger, Bennett M. (2000). Autobiographien: Soziologie der Soziologen. In Erika M. Hoerning (Ed.), *Biographische Sozialisation* (pp.21-32). Stuttgart: Lucius & Lucius.

Bock, Ulla & Landweer, Hilge (1994). Frauenforschungsprofessuren. Marginalisierung, Integration oder Transformation im Kanon der Wissenschaften? *Feministische Studien, 12*(1), 99-109.

Bourdieu, Pierre (1992). *Homo academicus*. Frankfurt a.M.: Suhrkamp. [(1988). *Homo academicus*. Stanford, Calif.]

Bourdieu, Pierre (1993a). Narzißstische Reflexivität und wissenschaftliche Reflexivität. In Eberhard Berg & Martin Fuchs (Eds.), *Kultur, soziale Praxis, Text. Die Krise der ethnographischen Präsentation* (pp.365-374). Frankfurt a.M.: Suhrkamp.

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work as a cultural scientist"] in cooperation with Verein für Kulturwissenschaft und Kulturanalyse and Österreichischer Fachverband für Volkskunde (January 2002) (http://www.kulturwissenschaft.at/freiberuflich_arbeiten/index.htm) as well as continuous intensive meetings of ca. 15 persons (predominantly freelancers) in the context of a *Jour Fixe* (since March 2002).

Bourdieu, Pierre (1993b). Satz und Gegensatz. Über die Verantwortung des Intellektuellen, Frankfurt a.M.: Fischer Taschenbuch.

Bourdieu, Pierre & Wacquant, Loïc J. D. (1996). *Reflexive Anthropologie*. Frankfurt a.M.: Suhrkamp. [(1992). *An invitation to reflexive sociology*. Chicago: Univ. of Chicago Press.]

Bromley, Roger (1999). Cultural Studies gestern und heute. In Roger Bromley, Udo Göttlich & Carsten Winter (Eds.), *Cultural Studies. Grundlagentexte zur Einführung* (pp.9-24). Lüneburg: zu Klampen.

Bude, Heinz (1997, Orig. 1995). Das Altern einer Generation. Die Jahrgänge 1938-1948. Frankfurt a.M.: Suhrkamp.

Chartier, Roger (1992). Kulturgeschichte zwischen Repräsentation und Praktiken. In Roger Chartier, *Die unvollendete Vergangenheit. Geschichte und die Macht der Selbstauslegung* (pp.7-23). Frankfurt a.M.: Fischer Taschenbuch. [(1988). *Cultural History: Between Practices and Representations*. Ithaca, New York: Cornell Univ. Press.]

Clifford, James & Marcus, George E. (Ed.) (1986). *Writing Culture. The Poetics and Politics of Ethnography*. Berkeley: Univ. of California Press.

Dressel, Gert (1996). *Historische Anthropologie. Eine Einführung*. Wien, Köln & Weimar: Böhlau.

Dressel, Gert (1999). Reflexive Historische Anthropologie als eine konsequente Historische Anthropologie. Oder: Warum auch wir uns zu unseren Forschungsobjekten machen sollten. In Gert Dressel & Bernhard Rathmayr (Eds.), *Mensch—Gesellschaft—Wissenschaft. Versuche einer Reflexiven Historischen Anthropologie* (pp.245-276). Innsbruck: Studia.

Dressel, Gert (2001). Wissenschaftspraxis—Biographien—Generationen. Ein paar Facetten einer Reflexiven Historischen Anthropologie. In Václav Buzek & Dana Štefanová (Eds.), *Menschen— Handlungen—Strukturen. Historisch-anthropologische Zugangsweisen in den Geschichtswissenschaften* (= Opera Historica, 9) (pp.27-69). Ceské Budejovice: Univerzita v Ceských Budejovicích, Historický ústa.

Dressel, Gert & Langreiter, Nikola (2002a) Aus der Wehrmacht an die Uni—aus der Uni in die Ehe. Restaurierte Geschlechterverhältnisse nach dem Zweiten Weltkrieg. In Andrea Griesebner & Christina Lutter (Eds.), *Die Macht der Kategorien. Perspektiven historischer Geschlechterforschung* (= Wiener Zeitschrift für Geschichte der Neuzeit, 2[2]) (pp.73-88). Innsbruck: Studien Verlag.

Dressel, Gert & Langreiter, Nikola (2002b). Ist der Rand das Zentrum? "KulturwissenschaftlerInnen" positionieren sich. *Historische Anthropologie, 10*(1), 154-164.

Dressel, Gert & Langreiter, Nikola (2002c). Nie Zeit, nie frei—Arbeit und Freizeit von WissenschaftlerInnen. In Sabine Gruber, Klara Löffler & Klaus Thien (Eds.), *Bewegte Zeiten. Arbeit und Freizeit nach der Moderne* (pp.121-138). München, Wien: Profil Verlag.

Dressel, Gert & Langreiter, Nikola (2003). Immerhin Programm—Reflexivität in den Cultural Studies. In Christina Lutter & Lutz Musner (Eds.), *Cultural Studies / Kulturwissenschaften—eine Zwischenbilanz für Österreich* (pp.143-160). Wien: Löcker.

Dressel, Gert; Kasabova, Anelia & Langreiter, Nikola (2003). Politiceski sistemi—naucna dejnost— akademicni biografii. Prelomi i kontinjuiteti (Political systems—scientific work—academic biographies. Breaks and continuities). In Radost Ivanova, Ana Luceva & Racko Popov (Eds.), Etnologicni aspekti na vsekidnevnata kultura (Socialism—Reality and Illusions. Ethnological Aspects of Everyday-Life) (pp.35-46). Sofia: Etnografski institut smuzej—BAN.

Dülmen, Richard van (2001²). *Historische Anthropologie. Entwicklung, Probleme, Aufgaben*. Köln, Wien & Weimar: Böhlau.

Eisch, Katharina & Hamm, Marion (Eds.) (2001). *Die Poesie des Feldes. Beiträge zur ethnographischen Kulturanalyse* (= Untersuchungen des LUI Univ. Tübingen, 93). Tübingen: Tübinger Vereinigung für Volkskunde.

Felt, Ulrike; Nowotny, Helga & Taschwer, Klaus (1995). *Wissenschaftsforschung. Eine Einführung*. Frankfurt a.M., New York: Campus.

Fleck, Christian (Hg.) (1996). *Wege zur Soziologie nach 1945. Biographische Notizen*. Opladen: Leske & Budrich.

Fleck, Ludwik (1980, Orig. 1935). *Die Entstehung und Entwicklung einer wissenschaftlichen Tatsache. Einführung in die Lehre von Denkstil und Denkkollektiv*. Frankfurt a.M.: Suhrkamp. [(1977). *Genesis and development of a scientific fact*. Chicago: Univ. of Chicago Press.]

Fuchs, Martin & Berg, Eberhard (1995²). Phänomenologie der Differenz. Reflexionsstufen ethnographischer Repräsentation. In Eberhard Berg & Martin Fuchs (Eds.), *Kultur, soziale Praxis,*

Text. Die Krise der ethnographischen Repräsentation (= suhrkamp taschenbuch wissenschaft, 1051) (pp.11-108). Frankfurt a.M.: Suhrkamp.

Goldinger, Heiner (2002). Methodik und Praxis des research up: als Ethnograph bei den Börsianern. *Zeitschrift für Volkskunde, 98*(2), 257-270.

Haas, Erika (1999). *Arbeiter- und Akademikerkinder an der Universität. Eine geschlechts- und schichtspezifische Analyse*. Frankfurt a.M., New York: Campus.

Hamm, Marion (2001). "Vat is all zis poetic business?" Poetische Sinnkonstruktion als Gedächtnisstrategie im Feld deutschen und jüdischen Erinnerns. In Katharina Eisch & Marion Hamm (Eds.), *Die Poesie des Feldes. Beiträge zur ethnographischen Kulturanalyse* (= Untersuchungen des LUI Univ. Tübingen, 93) (pp.161-183). Tübingen: Tübinger Vereinigung für Volkskunde.

Hasenjürgen, Brigitte (1996). *Soziale Macht im Wissenschaftsspiel. SozialwissenschaftlerInnen und Frauenforschung an der Hochschule*. Münster: Westfälisches Dampfboot.

Ingrisch, Doris & Lichtenberger-Fenz, Brigitte (1999). *Hinter den Fassaden des Wissens. Frauen, Feminismus und Wissenschaft—eine aktuelle Debatte*. Wien: Milena-Verlag.

Kaschuba, Wolfgang (1999). *Einführung in die Europäische Ethnologie*. München: Beck.

Knorr-Cetina, Karin (1991). *Die Fabrikation von Erkenntnis. Zur Anthropologie der Naturwissenschaft*. Frankfurt a.M.: Suhrkamp. [(1981). *The Manufacture of Knowledge: An Essay on the Constructivist.* Oxford: Pergamon Press.]

Krais, Beate (2000). Das soziale Feld Wissenschaft und die Geschlechterverhältnisse. Theoretische Sondierungen. In Beate Krais (Ed.), *Wissenschaftskultur und Geschlechterordnung. Über die verborgenen Mechanismen männlicher Dominanz in der akademischen Welt* (pp.31-54). Frankfurt a.M., New York: Campus.

Latour, Bruno (1987). *Science in Action. How to follow scientists and engineers through society*. Cambridge (Mass.): Harvard University Press.

Le Goff, Jacques (1994). Neue Geschichtswissenschaft. In Jacques Le Goff, Roger Chartier & Jacques Revel (Eds.), *Die Rückeroberung des historischen Denkens. Grundlagen der Neuen Geschichtswissenschaft* (pp.11-61). Frankfurt a.M.: Fischer Taschenbuch.

Lehmann, Hartmut & Oexle, Otto Gerhard (Eds.) (1997). *Erinnerungsstücke. Wege in die Vergangenheit*. Wien, Köln & Weimar: Böhlau.

Lindner, Rolf (1981). Die Angst des Forschers vor dem Feld. Überlegungen zur teilnehmenden Beobachtung als Interaktionsprozeß. *Österreichische Zeitschrift für Volkskunde, 77*(1), 51-66.

Lindner, Rolf (1987). Zur kognitiven Identität der Volkskunde. *Österreichische Zeitschrift für Volkskunde, 90*(1), 1-19.

Lindner, Rolf (2000). Die Stunde der Cultural Studies. Wien: WUV.

List, Elisabeth (1999). An den Rändern der Philosophie. Geschlecht, Marginalität und Dissidenz in wissenschaftlichen Institutionen aus dem Blickwinkel lebensgeschichtlicher Erfahrung. In Gert Dressel & Bernhard Rathmayr (Eds.), *Mensch—Gesellschaft—Wissenschaft. Versuche einer Reflexiven Historischen Anthropologie* (pp.31-44). Innsbruck: Studia.

Löffler, Klara (Ed.) (2001). *Dazwischen. Zur Spezifik der Empirien in der Volkskunde. Hochschultagung der Deutschen Gesellschaft für Volkskunde in Wien 1998* (= Veröffentlichungen des Instituts für Europäische Ethnologie der Univ. Wien, 20). Wien: Institut für Europäische Ethnologie der Univ. Wien.

Maase, Kaspar (1997). Volkskundliches Sprechen als symbolische Praxis oder: Stimmen der Volkskundler in Tropen. In: Rolf Wilhelm Brednich & Heinz Schmidt (Eds.), *Symbole. Zur Bedeutung der Zeichen in der Kultur. 30. Deutscher Volkskundekongreß in Karlsruhe von 25. bis 29. September 1995* (pp.387-398). München, Berlin: Waxmann.

Mannheim, Karl (1928). Das Problem der Generationen. *Kölner Vierteljahreshefte für Soziologie 7*, 157-185 a. 309-330. [(1951). On the problem of generations. In Paul Kecskemeti (Ed.), *Essays on Sociology of Knowledge*. New York.]

Meurkens, Peter (2001). Die Möglichkeiten der Volkskunde am gesellschaftlichen Diskurs teilzuhaben. Beispiele aus den Niederlanden. In Olaf Bockhorn, Editha Hörandner & Hartmut Prasch (Eds.), *Erlebniswelt Volkskultur. Referate der Österreichischen Volkskundetagung 2001 in Spittal/Drau* (= Buchreihe der Österreichischen Zeitschrift für Volkskunde, NS 17) (pp.11-30). Wien: Selbstverlag des Österreichischen Vereins für Volkskunde.

Müller, Albert (2000). Grenzziehungen in der Geschichtswissenschaft: Habilitationsverfahren 1900-1950 (am Beispiel der Universität Wien). In Christian Fleck (Ed.), *Soziologische und historische Analysen der Sozialwissenschaften* (pp.287-307). Opladen: Westdeutscher Verlag.

Nadig, Maya (1989). *Die verborgene Kultur der Frau. Ethnopsychoanalytische Gespräche mit Bäuerinnen in Mexiko.* Frankfurt a.M.: Fischer.

Nagel, Anne Chr. & Sieg, Ulrich (2000). *NachwuchshistorikerInnen-Initiative*. Available at: http://hsozkult.geschichte.hu-berlin.de [Date of Access: 08.06.2000].

Nöbauer, Herta & Zuckerhut, Patrizia (2002). *Differenzen. Einschlüsse und Ausschlüsse—Innen und Außen—Universität und freie Wissenschaft* (= Materialien zur Förderung von Frauen in der Wissenschaft, 12). Wien: Bundesministerium für Bildung, Wissenschaft und Kultur.

Nora, Pierre (1987). Vorwort. In Pierre Nora (Ed.), *Leben mit der Geschichte. Vier Selbstbeschreibungen. Pierre Chaunu, Georges Duby, Jacques Le Goff, Michelle Perrot* (pp.7-10). Frankfurt a.M.: S. Fischer.

Rosenthal, Gabriele (1995). *Erlebte und erzählte Lebensgeschichte. Gestalt und Struktur biographischer Selbstbeschreibungen*. Frankfurt a.M., New York: Campus.

Roth, Wolff-Michael (2002a, September). Evaluation and Adjudication of Research Proposals: Vagaries and Politics of Funding (87 paragraphs). *Forum Qualitative Sozialforschung/Forum Qualitative Research* [On-line Journal], *3*(3), Art. 25. Available at: http://www.qualitative-research.net/fqs-texte/3-02/3-02roth-e.htm [Date of access: 04.10.2002].

Roth, Wolff-Michael (2002b, September). Grenzgänger Seeks Reflexive Methodology (59 paragraphs). In *Forum Qualitative Sozialforschung/Forum Qualitative Research* [On-line Journal], *3*(3), Art. 2. Available at: http://www.qualitative-research.net/fqs-texte/3-02/roth/3-02review-roth-e.htm [Date of access: 16.10.2002].

Rückriem, Georg (2000²). "Es läuft". Über die Brauchbarkeit von Analogien und Metaphern. In Wolf-Dieter Narr & Joachim Stary (Eds.), *Lust und Last des wissenschaftlichen Schreibens* (pp.105-127). Frankfurt a.M.: Suhrkamp.

Schindler, Norbert (2002). Vom Unbehagen in der Kulturwissenschaft. Eine Polemik. *Historische Anthropologie, 10*(2), 276-294.

Schütze, Fritz (1977). *Die Technik des narrativen Interviews in Interaktionsfeldern—dargestellt an einem Projekt zur Erforschung kommunaler Machtstrukturen.* Bielefeld: Universität Bielefeld.

Schweighofer-Brauer, Annemarie; Schroffenegger, Gabriela; Gnaiger, Andrea & Fleischer, Eva (Eds.) (2002). *"Eigentlich lief alles nach Plan, bis..." Biographische Texte zu freien Wissenschaftlerinnen in Österreich*. Innsbruck: Studien Verlag.

Sieder, Reinhard (1994). Anmerkungen zur sozialwissenschaftlichen "Feldforschung". In Sepp Linhart, Erich Pilz & Reinhard Sieder (Eds.), *Sozialwissenschaftliche Methoden in der Ostasienforschung* (pp.165-181). Wien: Institut für Japanologie, Univ. Wien.

Spülbeck, Susanne (2001). Begegnung statt Dialog. Die Einbeziehung der Körpersprache in die Methodik der Feldforschung. In Katharina Eisch & Marion Hamm (Eds.), *Die Poesie des Feldes. Beiträge zur ethnographischen Kulturanalyse* (= Untersuchungen des LUI Univ. Tübingen, 93) (pp.124-139). Tübingen 2001: Tübinger Vereinigung für Volkskunde.

Strasser, Sabine & Schliesselberger, Eva (1998). Mutter oder Mentor? Zur Ambivalenz von Förderungsbeziehungen unter Frauen in der Wissenschaft. In Gudrun Perko (Hrsg.), *Mutterwitz. Das Phänomen Mutter—eine Gestaltung zwischen Ohnmacht und Allmacht* (pp.212-246). Wien: Milena-Verlag.

Weber, Wolfgang (1984). *Priester der Klio. Historisch-sozialwissenschaftliche Studien zur Herkunft und Karriere deutscher Historiker und zur Geschichte der Geschichtswissenschaft 1800-1970*. Frankfurt a.M. et al.: Peter Lang.

Wehler, Hans-Ulrich (1998). *Die Herausforderung der Kulturgeschichte*. München: Beck.

Warneken, Bernd Jürgen & Wittel, Andreas (1997). Die neue Angst vor dem Feld. Ethnographisches research up am Beispiel der Unternehmensforschung. *Zeitschrift für Volkskunde, 93*(1), 1-16.

Willke, Helmut (1994). *Systemtheorie II: Interventionstheorie* (= UTB für Wissenschaft: Uni Taschenbücher, 1800). Stuttgart, Jena: Gustav Fischer Verlag.

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